Contemporary Approaches to Psychology

HELSON
and BEVAN

165

THE
UNIVERSITY
SERIES
IN
PSYCHOLOGY

2
12.3.
165

CONTEMPORARY APPROACHES TO PSYCHOLOGY

THE UNIVERSITY SERIES IN PSYCHOLOGY

Editor

DAVID C. McCLELLAND
Harvard University

JOHN W. ATKINSON—*An Introduction to Motivation*

JOHN W. ATKINSON (Editor)—*Motives in Fantasy, Action and Society: A Method of Assessment and Study*

DAVID BAKAN—*Sigmund Freud and the Jewish Mystical Tradition*

FRANK BARRON—*Creativity and Personal Freedom*

BERNARD M. BASS and IRWIN A. BERG (Editors)—*Objective Approaches to Personality Assessment*

DAVID C. BEARDSLEE and MICHAEL WERTHEIMER (Editors)—*Readings in Perception*

RICHARD H. DANA—*Foundations of Clinical Psychology: Problems of Personality and Adjustment*

JOHN H. FLAVELL—*The Developmental Psychology of Jean Piaget*

J. P. GUILFORD (Editor)—*Fields of Psychology,* 3rd Ed.

HARRY HELSON and WILLIAM BEVAN (Editors)—*Contemporary Approaches to Psychology*

DAVID C. McCLELLAND—*The Achieving Society*

DAVID C. McCLELLAND, ALFRED L. BALDWIN, URIE BRONFENBRENNER and FRED L. STRODTBECK—*Talent and Society*

JOSEPH F. PEREZ, RICHARD C. SPRINTHALL, PAUL J. ANASTASIOU, and GEORGE GROSSER (Editors)—*General Psychology: Selected Readings*

THOMAS PETTIGREW—*A Profile of the Negro American*

HERBERT C. QUAY (Editor)—*Juvenile Delinquency: Research and Theory*

IRWIN G. SARASON (Editor)—*Contemporary Research in Personality*

PAUL J. STERN—*The Abnormal Person and His World: An Introduction to Psychopathology*

PAUL SWARTZ—*Psychology: The Study of Behavior*

/c

CONTEMPORARY APPROACHES TO PSYCHOLOGY

Edited by

HARRY HELSON and WILLIAM BEVAN

Kansas State University *The Johns Hopkins University*

In Association with

Jack A. Adams
University of Illinois

M. E. Bitterman
Bryn Mawr College

Gordon H. Bower
Stanford University

John F. Corso
State University of New York

W. R. Garner
The Johns Hopkins University

C. D. Creelman
University of Toronto

J. P. Guilford
University of Southern California

John F. Hall
The Pennsylvania State University

Jerry Hirsch
University of Illinois

L. Erlenmeyer-Kimling
Columbia University

John T. Lanzetta
Dartmouth College

Joan E. Sieber
Stanford Center for Research and Development in Teaching

E. Jerry Phares
Kansas State University

Julian B. Rotter
University of Connecticut

Wm. A. Wilson, Jr.
University of Connecticut

Martha Wilson
University of Connecticut

BUREAU OF EDNL. & PSYL. RESEARCH.
LIBRARY.

D. VAN NOSTRAND COMPANY, INC.
PRINCETON, NEW JERSEY
TORONTO LONDON MELBOURNE

VAN NOSTRAND REGIONAL OFFICES: *New York, Chicago, San Francisco*

D. VAN NOSTRAND COMPANY, LTD., *London*

D. VAN NOSTRAND COMPANY, (Canada), LTD., *Toronto*

D. VAN NOSTRAND AUSTRALIA PTY. LTD., *Melbourne*

Copyright © 1967, by D. VAN NOSTRAND COMPANY, INC.

Published simultaneously in Canada by
D. VAN NOSTRAND COMPANY (Canada), LTD.

No reproduction in any form of this book, in whole or in part (except for brief quotation in critical articles or reviews), may be made without written authorization from the publisher.

First Published January 1967
Reprinted September 1968

150
HEL

Bur... Edn. Psy. Rese...
DAVID HARE TRAINING COLL...
Dated 12.3.70
No 2208

PRINTED IN THE UNITED STATES OF AMERICA

Preface

This book was planned for the advanced undergraduate major in psychology and the graduate student, as well as for the professional psychologist interested in developments outside his own area of specialization. The panoramic survey of a field usually has been regarded as a useful pedagogical procedure in education. Generally, it has been felt that it should be conducted early so that it will provide the perspective needed for intelligent specialization in a more restricted field. We are of the view that the goals of a general approach may be more meaningfully met after a student has gained some intensive knowledge of one or more special areas of psychology. Hence our direction of this volume toward the advanced student, although some chapters may be read with understanding and profit by students who have a good general introductory course in psychology. And it is not unlikely that other chapters may be read with advantage by students in various specialized areas.

The book is the successor to *Theoretical Foundations of Psychology* (1951) which was due for revision or replacement in view of the many changes that have occurred in all fields of psychology during the last fifteen years. The present volume occupies a place between history and systems of psychology, which tend to stress theories, and handbooks of psychology, which tend to stress empirical and experimental findings. It is designed to cover theories and systematic issues as well as facts and experimental data. Since emphasis is given to developments following publication of the earlier book, this volume has been called *Contemporary Approaches to Psychology* with full appreciation of the hazards in asserting that any book, with the necessary lag in publication time, can be fully abreast of the most recent developments. Among the rapid, kaleidoscopic changes in the various areas of psychology, there are, nevertheless, ideas, theories, methods of approach, and experimental findings that persist over the years. Contemporary approaches, more often than not, depend upon and embrace previous findings. The present volume should therefore also provide the continuity that is the hallmark of scientific endeavor.

Like its predecessor, *Theoretical Foundations of Psychology*, this volume stresses concepts as well as facts, for ideas give form and substance to experimental findings. The significance of facts springs from their position and importance in systematic constructs, hence the emphasis in this book on theories, points of view, and fundamental assumptions. One of the chief lacunae in the education of many students of psychology is the absence of training in theoretical approaches to problems. Our tradition has been empirical, factual, experimental, and statistical. We tend to stress techniques of observation and experimentation, and statistical evaluation in our teaching rather than critical examination of assumptions and the theory or sense underlying the data. Techniques are often acquired by students with little understanding of their *rational* foundations or applications. Much of graduate training in psychology contrasts sharply with graduate training in present-day physics and chemistry, in which the student must be able to *think* as well as do. In the physical sciences, instruction is geared to aid and, if possible, to teach students how to theorize. Data in these areas are shown to follow from theories. While this is possible in only a few fields of psychology at present, we can, nevertheless, direct the attention of the student to the theoretical bases, explicit or implicit, underlying the divers kinds of data.

It was our aim to stress theories and data about equally but, as the reader will see, this was not achieved in all chapters both because of differences in emphasis in the various areas themselves and in the personal predilections of the contributors. However, we believe the treatments of topics in this book fairly well mirror prevailing balances in the various fields between experimental and empirical findings on the one hand and theoretical conceptualizations on the other.

Another aim of the present book is to highlight the presence of new areas of interest without losing continuity with the old. It was to be comprehensive but not encyclopedic. Restrictions of length and of preparation time inevitably work to the exclusion of some topics. Not every invitation to prepare a chapter eventuates in the final draft of a manuscript. Some areas, although of interest to psychologists, are amply accommodated in the texts of other disciplines and in the specialized texts of various areas of psychology. The extent to which the present book differs from its predecessor is suggested by the following statistics: Only two persons among the authors of the 1951 volume appear in this volume. Four chapters deal with topics, at most, only just at the psychologist's horizon in 1951. Two chapters have been expanded from one- to two-chapter treatments, and two others have been reduced to a single chapter. Qualitative differences in approach within chapters will be apparent to the reader familiar with the earlier volume.

We are indebted to many persons, publishers, journals, and scientific associations for their cooperation in the making of this book. Requests for permission to reproduce materials met with a favorable response in all but two or three cases, and we are grateful to the authors, editors, publishers, and others holding copyrights for permission to reproduce published tables, graphs, figures, and textual matter. Individual acknowledgments will be found in the references to original sources of publication in each case.

It will be evident to the reader that our publishers allowed as many tables and figures as each contributor felt he needed. The initial impetus for this successor volume to *Theoretical Foundations of Psychology* came from the publishers and was strongly supported by David C. McClelland, General Editor of Van Nostrand's *University Series in Psychology*. To both we owe a debt of gratitude for their part in making this book possible.

We are also happy to acknowledge the contribution of Mrs. Dolores A. Thorne, who collated and consolidated the indexes of names and subjects furnished by authors of the various chapters and also helped in many other ways with the final stages of this volume.

H.H.
W.B.

Contents

CHAPTER PAGE

PREFACE v

1. PROBLEMS AND METHODS OF PSYCHOLOGICAL SCALING 1

The Nature of Attributes 2
The Nature of Measurement 2
Some properties of scales
Nominal scales
Ordinal scales
Interval scales
Ratio scales
Other scale properties
Power versus restriction in scaling
The Basic Scaling Problem 5
The problem of generality
Latent versus manifest scaling
Nature of the subject's response
The assumed property of the response
The assumed property of the scale
Why latent property scales?
Some Scaling Techniques 8
Internal consistency of scales
Manifest-ordinal techniques
Rank ordering
Pair comparisons
Category ranks
Guttman's scalogram analysis
Coombs' unfolding technique
Manifest-Interval Techniques 14
Equisection
Equal-appearing intervals
Numerical category scales
Verbal rating scales
Checks on internal consistency
Latent-Interval Techniques 17
Normalized ranks
Normalized pair comparisons
Normalized category ranks
Manifest-Ratio Techniques 21
Ratio production
Magnitude estimation
Constant sum method
Latent-Ratio Technique 25
Other measurement techniques
The relation of response to attribute
Response as Direct Indicator of Attribute 28
Response as Indicator of Mediating Process 28
Correlated process
Interacting process
A Summary Evaluation 30
References 32

2. PHYSIOLOGICAL PSYCHOLOGY: NEUROPSYCHOLOGY 35

Introduction 35
What is physiological psychology?
Physiological theories of behavior
Theories and data
Basic Neurophysiology and Anatomy 36
Anatomy and physiology of the neuron
Synaptic transmission
Organization of the central nervous system
Subdivisions of the cerebral cortex
Neocortical Function 41
Function of the occipital cortex
Inferotemporal area
Occipital-inferotemporal interaction
Parietal lobe
Superior temporal cortex
Frontal lobe function
Limbic Areas and Reticular Formation 51
Hypothalamus and Reinforcement 53
Motor Systems 56
Structural Basis of Learning 58
Human Data 59
Three Recent Physiological Theories 61
Historical background
Hebb's neuropsychological theory
Pribram and neural regulation of behavior
Arnold's physiological theory of emotion
Summary of Theories 78
Approaches to Physiological Psychology 79
Other Theoretical Approaches 80
Recurrent Issues in Physiological Psychology 80
Mass action versus localization
Physiological nature of the behavioral unit
Summary 82
References 82

CHAPTER PAGE

3. BEHAVIOR GENETICS 91

Introduction 91
The Study of Behavior 91
  Description
  Taxonomy
  Analysis
Levels of Organization and Dimensions of Analysis 92
The Study of Heredity 93
  Transmission genetics
  Population genetics
  Physiological genetics
Properties of Individuals 102
  Organization of components
  Mitosis, differentiation, and development
  Meiosis, individual differences, and evolution
Properties of Populations 106
  Biological individuality
  Trait variations and the norm of reaction
  The abnormality of the normal
  Heritability and instinct
  Social structure and interaction and systems of mating
Methods and Data in Behavior Genetics 111
  Selection
  Inbreeding and consanguinity comparisons
  Strain and race comparisons
  Segregation analysis
  The single-gene hypothesis
  Two independent traits and correlation
  Polygenic characters
  Segregation analysis and human behavior genetics
  Diallel crosses
  Chromosome analyses
  Single-gene comparisons
  Pedigree analysis
  Family correlation
  Twin studies
  Human cytogenetics
Genetic Parsing, Natural Units, and Biochemical Pathways in Development and Behavior 128
Relation Between Evolution and Behavior 130
Cell Functioning and Memory 131
Heritability and Behavior 132
Summary 133
References 133

4. LEARNING IN ANIMALS 139

Early Developments 140
  Morgan
  Thorndike
  Reactions to Thorndike's theory
  The question of understanding
  Köhler
  Watson and Guthrie
Middle Period 150
  Tolman
  Hull
  Discriminative learning
  Latent learning
  The nature of conditioning
  Major revision of Hullian theory
Recent Developments 166
  Decline of S-R-reinforcement theory
  Two S-R-contiguity theories
  The new empiricism
References 176

5. VERBAL LEARNING 181

Short-Term Memory 182
Recognition Measures of Memory 186
Free Verbal Recall 187
Paired-Associate Learning 189
Discrimination 189
Response Learning 190
Association Formation 192
  Time course of association formation
Serial Learning 202
  Effective stimulus in serial learning
Long-Term Retention and Forgetting 207
Forgetting Without Explicit Interpolated Learning 211
Reinforcement in Verbal Learning 212
Temporal Parameters in Paired-Associate Learning 213
Variations in Degree of Reinforcement 215
References 217

6. MOTIVATION AND AFFECTIVITY 223

Introduction 223
Motivational Antecedents 224
  Physiological factors in primary need states
Acquired Needs 227
The Role of Rewards 230
Secondary Reinforcement 234

CHAPTER PAGE

Parameters of Secondary Reinforcement 235
- Amount of primary reinforcement
- Frequency of primary reinforcement
- Partial reinforcement
- Temporal relationships

Other Motivational Determinants 239
Environmental Enrichment 240
- Manipulative stimuli

Impoverishment of the Environment 241
- Berlyne's theory of arousal
- Response deprivation
- General considerations
- Frustration
- Affective states

The Influence of Motivational Determinants 247
Spontaneous and Consummatory Activity 248
- Spontaneous activity and primary needs
- Consummatory response and primary needs

Instrumental Behavior 250
- Learning as a function of reinforcement
- Amount of reinforcement

Motivational Theories 258
- Hull-Spence
- Drive-stimulus theory

Affective State Theories 260
- P. T. Young
- D. C. McClelland

References 263

7. SENSORY PROCESSES: SYSTEMATIC DEVELOPMENTS AND RELATED DATA 273

Introduction 273
Historical Perspective 273
Continua and Functional Relations 274
Sensory Stimulation and Behavior 276
Sensation and Perception as Conceptual Distinctions 277
Classical Psychophysics and Contemporary Formulations 278
- The psychophysical experiment

Theoretical Explanations of Sensory Discrimination 279
- The classical theory
- The theory of the neural quantum
- The theory of adaptation level

A Current S-R Model for Sensory Problems 287
Behavioristic Versus Phenomenological Terminology 287
A General Theoretical Formulation 288
Some Behavioral Studies of Sensory Processes 289
- Vision
- Gustation

Integration Through Information Theory 292
The Information Model Applied to Psychology 292
Information as a Quantitative Concept 293
- Transmission
- Sequential analysis

Some Studies on Information Processing in Man 297
- Transmission
- Information theory and its relation to general problems of behavior

Sensory Functions and Utilities 299
- Theory of signal detectability

The Signal-Detection Experiment 300
Essentials of the Theory with the Expected Value Criterion 300
Some Experiments Related to the Theory of Signal Detectability 303
Comments on the Advantages of the Theory of Signal Detectability 304
Summary 304
References 306

8. PERCEPTION 311

Some Physiological Considerations 313
Perception, Affect, and Motivation 314
The Phenomenological Approach 315
- Two experimental tests of configurational laws
- Information approach to form perception

Adaptation, Contrast, Assimilation and Constancy 323
Attention, Figure-Ground, and Distinctiveness 329
A Theory of Perception 331
Phenomena and Consequences of the Pooling Mechanism 331
- The problem of relevance

Summary 340
References 340

9. ENGINEERING PSYCHOLOGY 345

Introduction 345
Data and Issues 346
Display of Visual Data 346
Displays for the Presentation of Continuous Data 347
- Pictorial displays

CHAPTER PAGE

Practice effects and the design of visual displays
Human Tracking Behavior 356
Research Methods in Tracking 356
Basic Types of Tracking Tasks 357
Pursuit tracking
Compensatory tracking
Measurement of Tracking 358
Control System Dynamics 359
Aiding and Quickening 360
Rate aiding
Quickening
Properties of the Tracking Control 362
Control scale factors
Variables defining proprioceptive feedback
Multidimensional Tracking 365
Number of stimulus sources
Spatial separation of stimulus sources
Display integration
Anticipation and multidimensional responding
Mathematical models of tracking behavior
Servotheory
Monte Carlo model of tracking behavior
Human Vigilance 372
Significance for man-machine systems
Empirical findings
Theories of vigilance decrement
Conclusions on Vigilance Research and Theory 377
Summary 378
References 378

10. BEHAVIOR IN UNUSUAL ENVIRONMENTS 385

Introduction 385
The Psychologist's Treatment of the Environment 385
Preconceptions of the environment
Behavior and the physical nature of the environment
Phenomenological approach to environment
Functional definition of environment
Formal properties of the environment
Methodological issues of the environment
Identification of the Unusual Environment 392
Properties of the unusual environment
Unusual environments of the present chapter
Perception in Unstructured Visual Environments 394
Vision in the Ganzfeld
Figure perception and the Ganzfeld
Perceptual constancy and the Ganzfeld
Visual efficiency in the Ganzfeld of outer space
Behavior Under Conditions of Stimulus Impoverishment 396
Perception during stimulus impoverishment
Motor performance during stimulus impoverishment
Intellectual functioning and stimulus impoverishment
Emotional behavior during stimulus impoverishment
Group performance during stimulus impoverishment
Theoretical implications of stimulus impoverishment
Weightlessness 402
Sensory function and weightlessness
Psychomotor performance during weightlessness
Vigilance Behavior 404
Vigilance and conditions of stimulation
Vigilance and the observer
Theories of vigilance behavior
Summary 411
References 412

11. SOME THEORETICAL VIEWS OF CREATIVITY 419

Meanings of Creativity 419
A Multivariate View of Creativity 421
Experimental application of factor analysis
Factors and functions
Creative-Aptitude Factors 423
Fluency factors
Flexibility factors
Originality
Elaboration
Other kinds of factors
Creative Abilities and the Structure of Intellect 425
Divergent-production abilities
Abilities differentiated by content
Transformation factors
Evaluative abilities and others
Construct Validity of Factors and Tests 429
The Nature of Creative Production 431
Serendipity
Psychoanalytic theories
Creativity and high IQ

CHAPTER PAGE

- Spearman's theory
- Thurstone's theory
- Associative theory
- Wallas' stages of creation
- Rossman's stages of invention

A Transfer Theory of Creative Thinking 435

Motivation for Creative Thinking 435

- An Intellectual drive
- Secondary sources of satisfaction
- Factors of interest in thinking

Preparation for Creative Production 437

- Importance of information
- Role of systems
- Recall of information

Role of Incubation 438

- Hypotheses concerning incubation
- Incubation and spaced learning

Role of Insight or Intuition 439

- Emotional aspects
- Nature of insight
- Conditions affecting insight

Role of Fluency 441

- Quantity and quality of production
- Some principles of recall

Role of Flexibility 442

- Flexibility of classes
- Flexibility in form of transformations
- Redefinition and functional fixedness

Role of Evaluation 444

- Suspended judgment
- Effects of evaluative attitudes

Social Conditions Affecting Creative Performance 445

- Group creativity vs. individual creativity
- Homogeneous vs. heterogeneous groups

Some Special Social Conditions 446

- Imposed evaluation
- Group restrictions
- Self-imposed restrictions

Development of Creativity 448

Biographical Background 448

- Family and community characteristics

Relation to Chronological Age 449

- Childhood and youth
- Creative adults

Relation to Training 451

- Possibilities of training
- General training in creativity
- Special training methods
- Training for originality
- Some implications of creativity training for education

Summary 454

References 455

12. PERSONALITY THEORY 461

Introduction 461

Psychological Constructs 461

Dimensions of Personality Theories 463

- Systematic vs. nonsystematic
- Operational vs. nonoperational constructs
- Content vs. process
- Heredity and structure vs. experience
- Generality vs. specificity
- Internal vs. situational factors
- Motivational assumptions
- Personality change

Five Representative Theories 468

The Freudian Approach 469

Analysis of Freudian Theory 472

- Operational nature of the theory
- Content vs. process

The Adlerian Approach 475

Analysis of Adlerian Psychology 477

- Operational nature of the theory

Rogers' Self-Theory 479

Evaluation of Rogers' Theory 482

- Systematic nature of the theory
- Operational nature of the theory

The Personology of Gordon Allport 485

- Operational nature of the theory
- Content vs. process

Social Learning Theory 489

Evaluation of Social Learning Theory 493

- Systematic nature of the theory
- Operational nature of the theory
- Process vs. content

Present Status of Personality Theory: Conclusions 496

References 497

13. THE DEVIANT PERSONALITY 499

Introduction 499

Problems in Definitions 500

- Cultural relativism
- Subjective report
- Symptom manifestation
- Attempts at mastery
- Theoretical definitions
- Mental illness

Disease Entity Approach 502

Genetic Hypotheses 506

- Intelligence
- Functional psychosis
- Male homosexuality
- Neurosis

Body Types 508

Biochemistry 509

CHAPTER PAGE
Psychological Theory 512
Psychoanalytic Theory 512
Learning Theory 516
Concluding Discussion 521
References 525

14. SOCIAL PSYCHOLOGY: SOME SELECTED TOPICS 529

The Experimental Approach 529
Social Learning: The Socialization Process 533
  Methodological problems
  Some empirical findings
  General conditions in early environment
  Socializing agents and settings
  Aggression
  Dependency and achievement
Principles of Social Learning 538
  Social reinforcement
  Imitative learning
Cognitive Consistency Theories 543
Perception of Cognitively Balanced Units 543
Desire for Balance as a Motivator of Communication 545
Balance of Cognitive and Attitudinal Components 548
Cognitive Dissonance 548
Modes of Resolution of Belief Dilemmas 551
Cognitive Balance and Imbalance 554
Social Interaction 555
Social Perception 560
Group Composition 563
Summary 566
References 567

INDEX OF NAMES 573

INDEX OF SUBJECTS 585

CHAPTER 1

Problems and Methods of Psychological Scaling[1]

W. R. GARNER AND C. D. CREELMAN
The Johns Hopkins University *University of Toronto*

Quantification, measurement, scaling—these are all words which connote the use of numbers to describe phenomena. Whatever the term used, quantitative methodology has been the hallmark of a maturing science, and so it is with psychology. Quantitative methodology and measurement play very broad roles in psychology, as they do in any science.

One of the first uses of quantitative method is to specify the conditions of an experiment exactly, so that these conditions can be accurately reproduced on another occasion, and perhaps by another experimenter. For instance, we specify the physical intensity of a light, or of a tone stimulus, in exact terms, so that there can be no ambiguity to other psychological scientists about the conditions of our experiment.

In addition to precise statement of the conditions of the experiment, quantitative method should also include treatment of the data obtained. We need to be able to state the results of the experiment in a form which can be checked exactly by another experimenter. We use statistical techniques to describe the results of an experiment, and often also to estimate the likelihood that the same results, within a stated margin of error, would occur if the experiment were done again.

We can also use more abstract mathematics to show relations between dependent and independent variables, and the effect of the independent variable on some aspect of behavior. A mathematical equation can summarize and show most relationships between variables far more efficiently, and perhaps with greater meaning, than can the simple listing of experimental conditions and results. In recent years psychology has seen considerable use of such abstract mathematics to describe phenomena.

These uses of quantitative method are not unique to the science of psychology, nor are they the primary topic of this chapter. Rather, we shall be concerned with the measurement of psychological attributes, the problem area which has come to be called psychological scaling.

---

[1] The preparation of this chapter was supported in part by Contract Nonr-248(55) between the Office of Naval Research and The Johns Hopkins University. This is Report No. 23 under this contract. Reproduction in whole or in part is permitted for any purpose of the United States Government. Creelman was supported by a National Institute of Mental Health Postdoctoral Fellowship No. 12,140.

THE NATURE OF ATTRIBUTES

An attribute is an abstracted property of human experience. There are two critical aspects of this definition, each of which poses some special problems in the measurement of attributes, and at times this fact has led to some confusion about the nature of psychological scaling. The first aspect is that an attribute is an *abstracted* property of something, it is not the thing itself. Interestingly enough, while this fact makes for many of the difficulties of measurement, it is not unique to psychological measurement. When we talk about measuring something in an everyday situation, we are not really talking about measuring an object, or an event, but some abstracted property of it. When we measure length, or weight, for example, we are not measuring the object which has the length or the weight, but we are measuring some abstracted property, or dimension, of the object.

The second aspect of our definition is that an attribute relates to psychological experience—and it is this aspect which both makes it uniquely psychological and creates difficulties. Perhaps we can clarify this problem somewhat by contrasting an attribute (of experience) with a physical dimension which may be closely related to the experience. A sound can have a physical intensity, and we can measure its intensity with standard techniques of physical measurement. And we can experience loudness, but the loudness is not the same as the physical intensity. The loudness is something we experience; the intensity is something which is an aspect of the stimulus itself.

We speak, casually, of the loudness of a sound, rather than the loudness of our experience. Ordinarily there is little confusion created by our doing so. But when we become concerned about the measurement of loudness, the distinction becomes important, since we are required to investigate the properties of the attribute of loudness, even though we cannot in a direct physical sense get at the thing we are trying to measure.

This distinction between the attribute and the dimension of the physical object or event is fairly easy to see in those cases where there is an obvious physical counterpart of the attribute in which we are interested. In other cases, the distinction is more subtle. For example, when people are the stimuli we are interested in, and we want to measure something like leadership ability, or attitudes, it is often difficult to realize that the measurement we are concerned with is still not of the external stimulus, but of the underlying experienced attribute. If anything, the distinction becomes even more important in such cases.

THE NATURE OF MEASUREMENT

Psychological scaling is concerned with devising scales of measurement for psychological attributes. Before we present specific techniques for such measurement, it might be well to discuss the nature of measurement in more general terms, and then some of the special problems involved in the measurement of psychological attributes.

Measurement, in its broadest sense, is the assignment of numerals to objects, according to some specified rule. But this definition is much too simple, and even misleading, unless we consider the special problems of the use of numerals. Numerals are just symbols—the "1," "2," "53," etc., which we write or print. Thus if the problem of measurement were simply the problem of assigning numerals to objects by using some rule, we could use any rule we wish, as long as we use it consistently.

THE SCALE OF NUMBER The numerals, however, represent a particular kind of scale—the scale of number. The numeral "1" stands for a single object, the numeral "2" stands for two distinct objects, and each numeral we use has a direct meaning in that it represents a number of objects or events. The scale of number is a scale of counting, and it is the simplest and most fundamental scale of measurement we have.

In a strict sense, the numerals can have any properties—mathematical or otherwise—we wish to assign them. But since the numerals represent the fundamental scale of number, they are often assumed to have the properties of the scale of number. For example, suppose we have 12 objects. We can carry out several operations with these objects, and we can also carry out the equivalent operations with the numerals themselves. We can add 3 objects to our 12, and then if we count again, we will have 15 objects. But we can add the numeral "3" to the numeral "12" and get the same result somewhat faster. Or we can subtract objects, or their equivalent numbers, and either way we get the same result. We can even divide the 12 objects into 2 equal groups, and if we count each group we will find there are 6 objects in each group—a fact we can just as easily ascertain by doing the division on paper.

There are many properties of the scale of number, but when we assign numerals to attributes or dimensions other than number, we need to be careful to determine which properties of the scale of number are applicable. This problem has nothing to do with determining the properties of the scale of number; rather we must determine the properties of the attribute itself, and then be sure that the numerals are assigned so that the numerals properly reflect the properties of the attribute.

SOME PROPERTIES OF SCALES We have briefly alluded to some properties of the scale of number, but the more important properties need further explanation. These properties are often used to describe the nature of a psychological scale, since they limit the interpretation of scale values.

Nominal Scales. The simplest and most fundamental property of the scale of number is that of naming or identifying items or objects. As a simple example, we can ask each of several people what sex they are, and then assign to each the code number "1" if they say they are male, and the code number "2" if they say they are female. Such a use of numerals is measurement in a very primitive sense; it is the assignment of numbers according to a rule. But note that the only property of the scale of number which is applicable is the *identity* relation, namely, that all objects given the same number have the same sex. Here we can use no other property of the scale of number. For example, we cannot say that females are "more" than males, or that they are twice as much as males, even though the numerals represent these properties of the number scale.

Note, however, that we are free to change the assignment of numerals as long as we change all females together and all males together. We could have called females "1" and males "2." The reason we could do this is that the quality being "measured" does not have any measurable property other than identity or equivalence. The general point of our illustration is simply this: *The permissible rules for the assignment of numerals to objects depend on the properties of the attribute being measured,* and not on the properties of the scale of number. Thus we cannot determine what the permissible rules are without knowing something about the properties of the attribute itself.

Ordinal Scales. A second property of the scale of number is that of *order:* the number "10" is larger than the number "6," and in fact is larger than all numbers less than 10. Note that this property assumes the nominal property, since the same numeral would still be used for all objects which are identical.

The simplest use of ordinal scales in psychology is when we rank order a set of objects with regard to some attribute, and each object is assigned a single numeral to reflect its ordinal position. But we can also use a rank scale and have several different objects assigned the same rank number. The school grading scale of "A," "B," "C," "D," and "F" is such a scale, even though letters are used rather than numerals, since all "A" grades are better than all "B" grades, etc.

With the nominal scale we say that we could translate the numerals in any fashion which retained the identity relation. With an ordinal scale we can translate with any rule which preserves the original ordering of the numbers assigned to the objects. In other words, once a set of numerals is assigned, we are free to change them by writing any new set of numbers as long as the new numbers are a monotonic positive function of the original numbers.

Once again, whether we use the ordinal property of the scale of number for the assignment of numerals to objects depends on whether the attribute itself has the ordinal property. If it has, then we can use this more restrictive rule.

Interval Scales. A third, and even more restrictive, property of the scale of number is that of *equality of intervals.* If we add 6 objects to 24 objects to get 30 objects, the 6 objects are the same as if we add them to 40 objects to get 46 objects. Thus the numerical difference between 30 and 24 represents the same amount of number as does the difference between 46 and 40. The numerically equal intervals represent equal differences in number, and if an attribute has this same property, then the rule for assigning numerals should ensure that this property is properly reflected. Note that with the particular numerical example just used, we could change all the numbers by adding a constant, and it would not change the numerical value of the difference. Thus the intervals would still be equal.

If we are talking about actual number, of course, then we could not change the 6 and have it mean the same thing any longer, since 6 additional objects are just that, no more and no less. But if we are representing an attribute which does not have all the properties of the scale of number but does have the equal interval property, then we could multiply all of our numerals by a constant, and the two differences would still be equal. In other words, with interval scales we can translate our scale by any linear positive function—which is a more restrictive translation than the monotonic positive function permissible with the ordinal scale. In mathematical form, we are allowed to assign any new set of numerals, as long as the condition that

$$y' = a + by$$

is satisfied, where y is the original numeral and y' is the transformed numeral. The important condition, the equality of differences, will still hold with the new numbers.

There are innumerable examples in psychology where the ordinal property of the scale of number can be assumed for a psychological attribute; but with interval properties we are on less sure ground, and frequently the interval property is assumed but cannot be proven to hold.

The IQ scale is a good example of an assumed interval scale, since the difference between an IQ of 120 and one of 130 is assumed to be the same as the difference between IQ's of 90 and 100. Note that the entire scale could be moved up to center around 200, rather than 100, and it would not change any of the properties of the scale. So also could we double all the numbers and it would not change the equal interval property. So the actual numerals assigned are quite arbitrary, with the restriction that the assumed equal interval property must be reflected in the numerals.

Ratio Scales. An even more restrictive property of the scale of number is *equality of ratios*. The numeral "10" is twice the numeral "5," and so is the numeral "90" twice the numeral "45." Thus these two ratios are equal, and so are their counterparts in the scale of number, since in each case if the larger number is divided into groups each containing the respective smaller number, there will be exactly two groups. This property means that the numerical operations of multiplication and division are applicable to the attribute being measured—and in the case of a scale of number, they are.

Notice that in this case we are not free to transform the original numbers by adding or subtracting a number, because if we did, the ratios would no longer be equal. If we are dealing with any attribute which has the ratio property, then we can multiply all of the numerals by a constant and still retain the ratio property. What we may not do is add or subtract a constant. Thus if we have a scale with ratio properties, we can translate our numerals such that

$$y' = by$$

where y' is the numeral transformed from the original y.

In psychological research there are few examples of attributes which we can be sure have the ratio property. The difficulty has to do with the problem of an absolute zero. Notice that any permissible transformation of a ratio scale does not change the zero point on the scale, although the permissible transformations of an interval scale do. In fact, the basic requirement for a scale with ratio properties is that there exists an absolute zero—which means literally that no amount of the attribute exists. It is not sufficient to have a zero point on a scale, unless we can also be sure that the zero represents an absolute lack of the attribute.

The problem in psychology is twofold. First, there are not many attributes which can reasonably be assumed to have an absolute zero. For example, what would an absolute zero of intelligence be? Or what is the absolute zero of attitude toward the Republican Party? There can be neutrality of feeling, and the neutral position is often used as the zero point on a scale, but it does not represent an absolute lack of the attribute. The zero is simply a position between positive and negative attitudes. Or as still another example, what would the absolute zero of pitch be? It is difficult to picture what we might mean by "no amount of pitch," since pitch exists as a difference, but not strictly as a magnitude.

The other aspect of the problem has to do with our ability to determine the true, or absolute zero even if it might logically exist. Consider the psychological attribute of brightness, for example. We can produce a condition of no amount of light, but such a physical situation does not guarantee an absolute lack of brightness, since it is possible to have a blacker black than we have ever experienced. In neurological terms, we know that complete absence of light does not produce complete absence of nerve activity (Kuffler, FitzHugh, and Barlow, 1957), so it seems quite reasonable that absolute psychological black might never be experienced. So also with things like loudness; there may be a meaningful zero, but we probably can never experience it directly.

The scale of temperature gives a physical example of this problem. It has been known for some time that there is an absolute zero of temperature (which does not, of course, correspond to the zero of either the Fahrenheit or Centigrade scales), but physically producing such a condition is quite a different problem. The existence of an absolute zero could be deduced from the behavior of measurable temperatures, and its value relative to the Centigrade scale was finally determined. But historically the task of getting a ratio scale was made difficult by the inability to work directly with absolute zero.

This example points out another way of characterizing a ratio scale; it is one on which no negative numbers can be assigned. We can see that this is the case with counting objects, as in the scale of number, and with the Kelvin scale of temperature. In each case it is meaningless to speak of less than zero number of objects, or of a temperature less than absolute zero.

Other Scale Properties. The four kinds of scales we have discussed are certainly four of the more important scale types currently in use, but by no means are they the only ones possible. There are many other combinations of the properties of the scale of number which could also be properties of a psychological attribute. For example, it is possible that we can know that one interval on a scale is larger than another, but not be able to specify exactly how much larger it is. In such a situation, we could have not only an ordering of objects, but also an ordering of the intervals between objects. Coombs (1950) has in fact proposed such a scale, which we shall discuss in a later section. It is also possible to have a true ratio scale for intervals between objects, but only an interval scale for the objects themselves. Such a situation would occur if we could identify and measure a zero difference between objects, and at the same time be sure of the ratio properties of the differences.

POWER VERSUS RESTRICTION IN SCALING Frequently one hears of the power of a scale, and by power is usually meant the kinds of mathematical properties we have been discussing. A ratio scale is more powerful than an interval scale because it not only tells us about the numerical intervals of the measured attribute, but it also tells us about the ratios. And an interval scale is in turn more powerful than an ordinal scale because it tells us everything the ordinal scale does, but tells us about intervals as well. Psychological scalers usually try to devise scales with at least interval properties, even if some assumptions must be made in order to obtain the greater power. There are real advantages to the greater specification of an attribute which the more powerful scales provide.

There is, however, another side to this problem from the point of view of the scaler. He is more restricted in his use of numbers with the more powerful scales. As we have seen, with a scale as weak as the nominal scale, the only restriction on the scaler is that he use the same number for identical objects or amounts of an attribute. But with an ordinal scale, he must assign the numbers so as to reflect the ordinal relations inherent in the measured attribute; and with more powerful scales, he is even more restricted. With the scale of number itself, the most powerful scale, he of course has no choice at all, but must assign numbers to attributes in an exactly specified manner—the number "10," for example, can be used for ten objects only, not more or less.

The more powerful scale has greater utility once it has been established, but the scaler has much less freedom in establishing the scale values of the more powerful scale in the first place. In effect, the scaler is always in a certain amount of conflict. He wants to establish as powerful a scale as possible, but he is much less sure that he has done so correctly than if he had attempted to establish a weaker scale. Often the scale produced is a compromise between a scale with maximum power and a scale with minimum restriction on the scaler.

THE BASIC SCALING PROBLEM

Any scaling experiment will be concerned with three sets of variables, and it will be well at the outset to understand the different role each of the three plays. In the most simple case this is easy to visualize. The three variables are *stimuli*, a set of objects which we have chosen to use, *subjects*, to whom the objects are presented, and *responses* which are required by the experimental situation. The role that each of these variables can play in the scaling process can vary, but it is always the case that an analogous set of three variables must be chosen. Each is important, and each must be carefully considered.

It might be well to recall that the problem of psychological scaling is the problem of assigning numerals to an attribute—which is an abstracted property and not to be confused with the object itself. Nevertheless, often the only way we can specify how much of the attribute corresponds to a stated numeral is to point to a particular object as an illustration. Thus, as a practical matter, we do end up assigning numerals to objects—but our purpose is not to say that the object is the numeral. Rather, it is to say that this object can call out the attribute in this amount.

Likewise, while we are interested in the effect of the stimulus objects on the persons to whom they are presented, it is true that all we have to go on is the responses that we obtain from our subjects. Then a double inferential chain is necessary (in the scaling process). First the stimuli are chosen to represent the world of possible stimuli in some representative fashion, and then the responses are taken to represent the experience of the subject in some meaningful sense. The response must also reflect the aspect of the experiment that we are interested in. This is not always as easy as it might seem. For instance, as we shall see in Chap. 8, the response from a subject to the effect that he "sees" a very weak light can not always be taken at face value, but it is determined by many aspects of the situation beyond the intensity of the light itself.

We have been talking about objects as the sources of the attribute, although some of our illustrations have used people as the objects. In reality, if we think of the objects as stimuli, we can scale any one of these three variables—the stimuli, the subjects, or even the responses. For example, there need be no simple relation between the responses used by the subject in the experiment and the underlying metric of the attribute. Certainly if the responses are verbal rather than numerical, we need to scale the responses before we can scale the stimuli; in fact, one can hardly be done without the other. If we manage to assign numerals correctly to the stimuli, then we must also find a numerical scale for the responses, since we are looking for a functional relation between the stimuli and the responses.

It is also quite possible that we could determine the amount of an attribute corresponding to the subject himself. This is commonly the case when we measure attitudes. The function of the objects now is to provide a way for the subject to express an amount of an attribute in himself, the subject, rather than the object. For example, if the objects have a predetermined amount of the attribute, then we can determine the subject's perception of himself by finding which objects he will accept as representative of himself, or desirable to him.

Abelson (1960) has expressed these interrelations in another form. He distinguishes between *agents*, *objects*, and *modes* as the three variables required in a psychological scaling problem. While his distinctions leave some ambiguity, in principle they are sound. To illustrate with a reasonably simple case, suppose we want to scale the loudness of several traffic sounds. Our scaling problem then becomes one of assigning numerals to various sounds, which are the objects, in such a way that the underlying attribute is properly reflected.

But we must have some agent which defines the attribute, so we use one or more subjects to judge the loudness of the stimuli. The subjects, then, are the agents through which we determine the scale values to assign to the stimuli.

The agent expresses his judgment through a mode, which is the actual set of responses which he is allowed to use.

This is a useful way to visualize the scaling problem, and yet we shall see that it is an oversimplification (as are most ways of describing the problems of psychological scaling). There are some scaling techniques in which both the stimuli and the subjects are scaled from the same set of data, so that the objects and the agents play interchanging roles in the same scaling problem. And there are other cases where the stimuli and responses are scaled from the same set of data, and in a real sense play interchanging roles as objects and modes.

What should be kept clear, however, is that we are interested in scaling an attribute, an abstraction about the perception of either stimuli, people, or even of responses. Since our interest is in the abstracted attribute, we can scale it with reference to one or more of these three basic variables, sometimes simultaneously, or at least from the same set of data.

The Problem of Generality. There is another reason why our original three-variable framework used the term *subjects* rather than *agents*, and that is to illustrate a pervading problem in all psychological research. To a greater extent than other sciences, psychology must always face, and attempt to answer, the question of the generality of an experimental result—and generality usually, but not always, means with respect to some specified population of people. We could use just one subject as our agent to scale the loudness of stimuli, with a single response mode, but we don't do so because we want some assurance that the scale obtained is not unique for our one subject. Thus we use several subjects and pool or average data across them in order to have the greater generality which average data give us. Or, alternatively, we can look for differences between subjects to determine how generally applicable the scale is.

If our intent is to establish differences between subjects, then frequently the objects become the means of establishing generality. We might want to know, for example, whether sounds seem louder to one person than to another. But we would ordinarily be unwilling to draw such a conclusion on the basis of judgments about a single stimulus. Rather, we would want to know whether subject A always rates auditory stimuli as louder than does subject B. Thus either subjects or objects can be used to establish generality. In still other cases, we would want to establish generality across responses, since the scale we obtain should not be unique to a particular set of possible responses. If subjects are, for example, asked to judge the brightness of a light, the scale we obtain should be the same whether he uses responses between zero and ten, or between zero and 100.

LATENT VERSUS MANIFEST SCALING The data obtained to construct a psychological scale may be used in many ways. But there are differences in how the scale can be constructed other than those we have discussed, and one of the major differences is whether the latent or the manifest properties of the data are used to construct the scale.

The manifest properties of the data are, as the term suggests, the evident, easily seen, and interpreted properties. The latent properties are those which must be extracted from the data, which are inherent, but are not readily perceivable. The latent properties are just as important as the manifest properties, perhaps even more so. Psychological scaling techniques have tended more and more to the use of latent properties, for a variety of reasons.

When we distinguish between scales based on manifest or on latent properties of data, we are not concerned with whether the data themselves have manifest properties: All data do. Rather, our concern has to do with the measurement properties of the scale we construct in relation to the measurement properties of the manifest data. Ordinarily, scales based on latent properties will have measurement properties different from, and usually more powerful than, the measurement properties of the responses (the manifest data). Indeed, the object of most latent scaling techniques is to arrive at such stronger measurement properties. There are three major points in the construction of a psychological scale which are relevant to the question of whether we have a scale based on manifest or on latent properties.

The Nature of the Subject's Response. The first, and frequently most important consideration, is the nature of the response required of the subject. We can require him to use nominal, ordinal, interval, or even ratio scale types of responses, and if we intend to use the manifest properties of the data, then we can construct a scale with no more powerful measurement assumptions than the subject was asked to use. For example, if we ask subjects simply to order several stimuli for aesthetic preference, then we cannot construct an interval or a ratio scale based on manifest properties of these data.

The Assumed Property of the Response. It is not necessary, of course, that the experimenter assume the responses have the measurement properties the subject was told to use. If the subject was instructed to make ratio judgments of the brightness of lights, we need not assume that he was able to do so. We might assume that he was only capable of making interval, or perhaps just ordinal, judgments. Logically, it is possible to assume more powerful properties of the response continuum than the subject used, even though it is rarely done. For instance, we could instruct the subject to rank order a set of stimuli, and then assume that in reality the ranks represent an interval scale. If we do so, then we would be tapping a latent property of the data, since the interval property was not evident.

The Assumed Property of the Scale. The scale finally constructed will not necessarily have the same measurement property as the responses used by the subject, or even the same as that assumed by the experimenter. One can, as we shall see, assume only that the subject made ordinal responses, but from these an interval scale can be constructed, if some further assumptions are made in treating the data.

When the nature of the response, the assumed properties of the response, and the assumed property of the scale are congruent, then we have a scale based on manifest properties of the data. If they are not congruent, then the scale is to some extent based on latent properties. It should be clear that the distinction between latent scales and manifest scales is not a sharp dichotomy. Rather, there are all degrees of variation from scales based entirely and simply on manifest data, to scales based partially on latent properties of the data, to scales which bear little relation to any evident or manifest properties of the data.

WHY LATENT PROPERTY SCALES? The construction of a scale based on manifest data is basically so simple and straightforward that we can wonder why latent property techniques are used at all. If we can directly ask a subject how loud a tone is, or how beautiful a tree is, why not do so and be done with it? The answer lies in the very nature of the psychological scaling problem itself.

In psychological scaling we try to assign numerals so as to reflect the properties of an attribute —the abstraction which cannot be directly observed. Therefore, when we have used manifest data we have begged what may be the most important question of all, whether there is, in fact, such an attribute, and if so, what its properties are. To use manifest properties of the response requires a very powerful pair of assumptions. We must assume that the attribute exists and has the measurement properties which we assign to it; and we must also assume that these properties can be directly reflected by the subject in his manifest response.

It is not sufficient to say that we have a scale for an attribute because we can show a functional relation between a set of objects and a set of scale values. We must first determine what the properties of the attribute itself are (Does it have an absolute zero, for example?), and then what numerals to assign the objects having the property. In other words, we must establish a relation between the attribute itself and some aspects of the scale of number.

Scales based on latent properties of data are much like hypothetical constructs or intervening variables (Green, 1954); they are mathematically constructed to explain relations between other variables. Rarely do such scales have properties more powerful than an interval scale, but they usually are assured of this property in a meaningful way. A scale based on manifest data where the responses have ratio properties may frequently appear to have such properties; but the greater power of the ratio scale comes at considerable cost. There is seldom any clear evidence for the meaningfulness of the presumed attribute in the first place.

Most latent scale techniques have experimental or mathematical converging operations (Garner, Hake, and Eriksen, 1956) built into them. Converging operations serve as separate checks on the legitimacy of the concept. For, after all, an attribute (by definition not directly observable) is just that—a concept, and a concept is quite meaningless unless it is both abstracted and general. To identify a concept from a single experimental or mathematical operation is to establish meaningfulness by fiat, a procedure which is hardly in good scientific taste. The latent techniques thus generally provide greater generality, even though at a possible cost in measurement power.

SOME SCALING TECHNIQUES

Each scaling problem poses different requirements of technique, so there have been many different scaling techniques developed. Many differences in technique are due to differences in the attributes or the objects to be scaled. These differences make it difficult to organize the various techniques in a meaningful fashion. Some means of organizing the techniques must be found if we are to present a clear picture. We could organize them according to the nature of the attribute, according to the nature of the objects to be scaled, or even according to the nature of the responses used. But these methods do not recognize the most important related aspects of a psychological scale, namely, what its measurement properties are and how the scale is related to the underlying attribute.

We have, therefore, chosen to organize the techniques according to these two major criteria: First, the measurement properties of the scale; and, second, whether the scale is based primarily on latent or on manifest properties of the data. In describing the kinds of scales, we shall use terms like "manifest-interval," which means that the scale obtained has interval properties, and that these properties were inherent in the manifest response.

Internal Consistency of Scales. Before describing the specific techniques, one other comment is worth making about the experimental problems of psychological scaling. Good experimental technique requires some check on the internal consistency of the data. The concept of internal consistency is an important one, and we shall frequently refer to it. Basically it is concerned with the validity of a particular scaling procedure, or with the validity of the assumption that a particular attribute can be scaled with the specified scale properties.

The essence of the concept is this: If a scale has the assumed properties, and if the particular scaling technique is valid for determining these properties, then the experimental results should show certain internal relations which are consistent with the assumed properties. To illustrate, let us suppose that we are trying to establish an ordinal scale for three objects, A, B, and C. If a large number of subjects all agree that A is greater than B, and that B is greater than C, then they should also agree that A is greater than C in order for these data to be internally consistent. If instead they all agree that C is greater than A, then we have a reliable result (reliable because of intersubject agreement), but we would question the legitimacy of the scale because of the lack of internal consistency. As another example, suppose that we have found experimentally that A is twice as much of something as B, and that B is twice as much as C. Then our experimental procedure should also show that A is four times as much as C. If it does not, then we reject the assumption that proper ratio scaling has been accomplished.

If a set of data are not internally consistent, we are not always sure why. It may be that the assumption of the existence of a scale with the specified properties is in error. Or it may simply be that our experimental technique is inadequate for determining the scale. If data are not internally consistent, then we often try another technique to determine whether it is the original technique which is at fault rather than the assumption about the properties of the attribute itself.

Many scaling procedures have checks on internal consistency as an integral part of the technique itself. In particular, the latent techniques frequently contain and even emphasize such checks because of the importance of the assumption of scalability with these techniques. With the manifest techniques, such checks can (and should) be made, but ordinarily they involve carrying out more than a single experiment.

MANIFEST-ORDINAL TECHNIQUES The ordinal property of psychological scales is so commonly accepted that essentially all scales with only ordinal properties are based on manifest data. In other words, latent techniques are not usually used to obtain ordinal scales because most experimenters are willing to accept the ability of the average subject to make ordinal judgments.

Rank Ordering. Probably the simplest of all scaling techniques is simple rank ordering. A subject is shown a set of stimuli to judge, say, for his aesthetic preference; or a supervisor is asked to rank the employees under his supervision; or a subject is asked to judge the brightness of several gray stimuli. In each case, if there are relatively few objects to rank, the subject is simply required to place the objects in rank order, or to assign a rank number to each object. These rank numbers are then taken to be the scale values.

In order to provide some generality to the scale and thus to provide some check on the validity of the assumption that there is an attribute to be scaled, several subjects or raters may be required to rank the same objects. The data of Table 1-1 show ranks from an hypothetical example where four raters each rated five workers. The rank numbers can then be summed to give a composite rank scale, or we can take averages for each worker to obtain an average rank. It should be clear, however, that these average ranks, with their unequal numerical spacings between stimuli, do not give scale properties beyond those of ordinal scales. To avoid any misinterpretation of the meaning of such average ranks, it is better to reassign whole rank numbers to the objects, as has been done in Table 1-1.

TABLE 1-1

Relative ranking of workers in a shop by different raters. The over-all ranking is from the sums of the ranks given each worker.

Raters	*Workers*				
	Joe	Bill	Andy	Jack	Sam
Foreman	2	1	3	4	5
Superintendant	3	2	1	5	4
Shop steward	2	1	4	3	5
Psychologist	3	2	4	1	5
Sum of ranks	10	6	12	13	19
Average rank	2.5	1.5	3	3.25	4.75
Over-all rank	2	1	3	4	5

Pair Comparisons. If we have a relatively small number of objects to be ranked, a slightly more sophisticated technique can be used, one which provides a greater check on the internal consistency of the ordinal assumptions. With this technique, the objects, say 10 of them, are formed into all possible pairs. (With 10 objects, there are 45 such pairs.) Now these pairs are presented one at a time, and the subject is required to state which stimulus has the greater amount of the relevant attribute, or which person, say, is more skilled as a machinist. Thus with this technique, instead of each subject ranking all 10 objects at one time, he ranks 2 objects in each of 45 pairs.

The advantage of this technique is that it provides a check on the ability of subjects to rank the stimuli in the first place, because if the objects really have a consistent ordering for the subject, then many of the pair judgments are predictable from others. To illustrate, suppose that one subject chooses A as greater than B, and then chooses B as greater than C. It is now clear that he should also choose A as greater than C, and if he fails to do so more often than would be predicted on a chance or error basis, we have cause for rejecting the assumption that the objects can be ordered with respect to the specified attribute.

Table 1-2 shows a set of hypothetical data from a pair comparisons experiment, displayed in the usual form. In this table the stimuli are listed both across the top and down the side, and each cell represents a single pair which has been judged. The entry in a cell of the table shows the proportion of choices (out of all subjects making the choices) on which the stimulus listed at the top was chosen over the stimulus listed at the side. In the illustration, 50 subjects were required to choose between pairs of paintings, on the basis of aesthetic preference. For instance, painting "A" was preferred to painting "B" by 35 of the 50 subjects, for a proportion of .70. Likewise all subjects preferred painting "A" to painting "H."

In this type of experiment, like simple ranking, data from just one subject could give us the rank values for the objects—if his pair choices were internally consistent. However, we can obtain greater generality by pooling responses across subjects, as we have done in our illustration. If we now simply sum the total number (or proportion) of times that each object is chosen when it is paired with each of the others, we have an average rank scale. And again as with rank ordering, the stimuli should be assigned whole rank numbers, since the differences in numbers of choices do not necessarily reflect differences in the intervals between objects. Thus we still have just an ordinal scale, one based on manifest ordinal properties of the judgments.

TABLE 1-2

Pair-comparison method of ranking*

		Paintings							
		A	B	C	D	E	F	G	H
Paintings	A	(.50)	.30	.14	.06	.06	.02	00	00
	B	.70	(.50)	.38	.22	.12	.08	.08	00
	C	.86	.62	(.50)	.32	.30	.14	.10	.06
	D	.94	.78	.68	(.50)	.32	.36	.18	.08
	E	.94	.88	.70	.68	(.50)	.38	.16	.10
	F	.98	.92	.86	.64	.62	(.50)	.36	.10
	G	1.00	.92	.90	.82	.84	.64	(.50)	.22
	H	1.00	1.00	.94	.92	.90	.90	.78	(.50)
Sum of proportions		6.92	5.92	5.10	4.16	3.66	3.02	2.16	1.06
Rank		1	2	3	4	5	6	7	8

* Entries in the table are the proportion of 50 subjects who rated each painting across the top as better than the painting listed at the side. Final ranking of the paintings is done from the sums of the proportions, which reflect the number of occasions each painting is preferred to all others.

Category Ranks. Both the ranking and pair comparisons techniques are feasible only when we have a reasonably small number of objects to be scaled. With as many as 20 objects, ranking becomes very difficult, and pair comparisons (with 190 pairs of objects) becomes prohibitively difficult. We then must resort to a somewhat modified ranking or ordering technique.

With the category ranks technique, fewer rank categories are used than there are objects to be judged. For example, we might have 60 different stimulus patterns to be judged for goodness of pattern, but we ask the raters to use just five different response categories, and to use these categories as ranks only. In our illustration in Table 1-3, there are just five rank categories, in which rank "1" means the best patterns, and rank "5" means the poorest patterns.

With this technique, of course, each rank category will be used for many different stimuli, but the different subjects or raters will not all assign the ranks to the stimulus objects in exactly the same way. So for each stimulus we will have a distribution of rank categories. From this distribution we could compute a mean rank, as has been done in Table 1-3, but this procedure is open to question because the rank categories do not have interval properties. A better procedure, also shown in Table 1-3, is to compute the median rank assigned to each stimulus by interpolating from the cumulative frequency distribution for each stimulus, and these median ranks are shown in the table. Again, reassigning whole rank numbers to the stimuli is appropriate, since the differences in median rank do not indicate interval properties of the attribute in question.

Guttman's Scalogram Analysis. There are two scaling techniques which lead to ordinal scales that depend to some extent on latent properties of the data. One of these is Guttman's Scalogram technique (1950), which produces a scale on which both stimulus objects (usually test items)

TABLE 1-3

The method of category ranks*

	Stimulus Patterns					
Rank	1	2	3	4	19	20
1	.50 *.50*	.40 *.40*	.45 *.45*	.30 *.30*	.10 *.10*	00 *00*
2	.30 *.80*	.35 *.75*	.15 *.60*	.25 *.55*	00 *.10*	.10 *.10*
3	.05 *.85*	.15 *.90*	.15 *.75*	.20 *.75*	.15 *.25*	.10 *.20*
4	.10 *.95*	00 *.90*	.15 *.90*	.10 *.85*	.40 *.65*	.40 *.60*
5	.05 *1.00*	.10 *1.00*	.10 *1.00*	.15 *1.00*	.35 *1.00*	.40 *1.00*
Median	1.50	1.79	1.83	2.30	4.13	4.25
Mean	1.90	2.05	2.30	2.55	3.90	4.10
Rank	1	2	3	4	19	20

* The upright numbers are the proportion of the raters placing each of the patterns listed across the top into each of the categories of "goodness of pattern" listed down the side. The slanted numbers are the proportion of raters giving the listed rank or better, obtained by adding down each column. The median is obtained from an interpolation to find the rank number which 50 per cent of the raters exceed for each pattern. The mean category values for each pattern are listed as well.

and subjects can be placed. The essence of the technique is the determination of the validity of the ordinal assumption for an attribute. Unless it can be shown that both objects and subjects can be ordered with regard to a single attribute, then Guttman argues that we have no grounds for attempting to produce an ordinal scale at all. This technique is most appropriate with attributes such as abilities, where both stimuli and subjects can be presumed to elicit the attribute.

While the Guttman technique has been developed to a considerable degree of sophistication, we will describe it only sufficiently to show the basic principles which are operating.

The kinds of stimulus items used for a Guttman scale are usually those which can be answered dichotomously—accept or reject, right or wrong, etc. The requirement of the technique is that only those items can be used which can consistently be ordered with regard to preference or ability, and that subjects can also be consistently ordered with respect to the items. The example we have chosen is shown in Table 1-4, with five arithmetic questions. These items are chosen to correspond to an increasing level of difficulty, such that the simple addition problem is easier than the double-digit addition problem, and so on. If these items do in fact represent a single attribute continuum, then subjects should be right on all problems easier than the most difficult one they solved, and they should be wrong on all problems more difficult than the easiest one they failed. Thus the subject who correctly answered the last item should have correctly answered all other items; and the subject who failed the first item should have failed all other items, because they are more difficult.

TABLE 1-4

An arithmetic test which establishes a Guttman scale*

Test Items	*Subjects*					
	A	B	C	D	E	F
1. $2 + 2 = 4$	−	+	+	+	+	+
2. $11 + 23 = 34$	−	−	+	+	+	+
3. $6 \times 12 = 72$	−	−	−	+	+	+
4. $48 \div 4 = 12$	−	−	−	−	+	+
5. $78 \div 13 = 6$	−	−	−	−	−	+
Number correct	0	1	2	3	4	5

* The items are arranged in order of difficulty, and the subjects are arranged in order of ability. A "plus" means that the subject listed at the top got the item correct, and a "minus" means that he failed the item. All possible sets of scores are listed.

This requirement, of perfect ordering of the stimulus objects, means that there are only six possible scores, and each possible score represents a single subject scale type. The six possible scores, of course, are the numbers from "0" through "5," and each number is uniquely associated with a particular pattern of right and wrong answers. Thus a "3" means that these subjects were right on the third item and all items less difficult, but were wrong on the last two items.

There are, naturally, many difficulties involved in establishing such a scale with more realistic types of items, and with items which are not answered dichotomously, and these problems are discussed in Stouffer et al. (1950). But the basic concept of this technique is not difficult, and it is of interest because of its central emphasis on the internal consistency check as the primary requirement which must be satisfied. We have noted that most scaling techniques do have some degree or form of internal consistency check inherent in them, but none of the other techniques so thoroughly establishes the validity of the fundamental assumption involved in all psychological scaling, namely, that an attribute actually exists with the assumed or stated properties of measurement. Far too often this assumption remains just that, an assumption, with little clear proof of its validity.

Coombs' Unfolding Technique. Coombs (1950) has described a scaling technique which also locates both stimulus objects and subjects on the same attribute. His technique makes direct use of ranking by subjects, and it is in this respect based on manifest properties of the data. But his technique also, with sufficient data, makes possible the determination of an ordering of the intervals—leading to what Coombs calls an ordered metric type of scale, a scale which is intermediate between those with ordinal and those with interval properties.

The best way to explain the unfolding technique is to start with the end product desired and see what kinds of data could occur if this scale were so. We have, following Coombs' terminology, a *J* scale of some attribute, which is a scale on which stimuli can be located, and on which the desired or preference position of the subject can also be located. To illustrate, let us suppose that we have five different levels of sugar concentration in a soft drink, and we know the scale values of the attribute of sweetness, but also the preference position of each subject with regard to the attribute of sweetness.

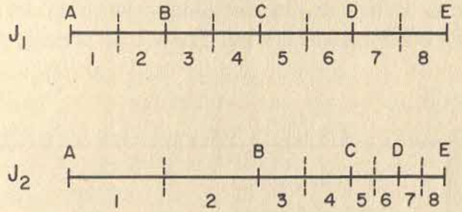


Fig. 1-1. Two different J scales on which five stimuli (A through E) are located as points. The numbers indicate possible ranges of positions where subjects could also be located. Each subject position will lead to a different order of preference.

Figure 1-1 shows two possible J scales. On the J_1 scale, we have located all five stimuli equidistant —thus we are temporarily assuming these five stimuli to constitute a known equal interval scale. Now each subject will have some preferred sweetness, so each subject can also be located along the scale. The arabic numerals indicate ranges within which subject preferences can lie, and each range will have a different effect on the responses given by the subject.

In the actual experiment, we ask each subject to rank order his preference for the five sweetnesses, and we assume that he will rank order according to the nearness of the actual sweetness to his preferred sweetness. With this assumption, we can predict exactly what the rank orders of the five stimuli will be for each range of possible preference positions. The rank orders for each preference position are:

1 ABCDE
2 BACDE
3 BCADE
4 CBDAE
5 CDBEA
6 DCEBA
7 DECBA
8 EDCBA

For this particular J scale, no other rankings can possibly occur, and if in an experiment we obtain only these rankings out of all the possible rankings of five things (5!, or 120), then we have learned two things: First, we know that the stimuli can in fact be ordered; and, second, we can determine the ordering by unfolding the various rankings of preferences to give us the original scale of stimuli themselves. The term "unfolding" is used because in effect the subject, in making his preference rankings, folds the scale of sweetness over, using as a hinge his own position on the scale, and in reconstructing the scale, we, in effect, unfold the rankings.

But now let us see what would happen if the intervals between the stimuli on the J scale were not equal. The J_2 scale of Fig. 1-1 shows such an hypothetical scale, and once again we can ask what rankings could be produced by subjects if their preference positions were in the intervals indicated by the arabic numerals. In this case, the following preference rankings could occur:

1 ABCDE
2 BACDE, or BCADE, or BCDAE
3 BCDEA, or BCDAE
4 CBDEA, or CDBEA
5 CDEBA
6 DCEBA
7 DECBA
8 EDCBA

Where there is more than one possible preference ranking for a given range of positions, the exact ranking depends more precisely on the position of the subject's preference. Altogether, with this set of scale values for the sweetness attribute, eleven different rankings can occur, and once again, we can reconstruct the ordering of the stimuli by unfolding the preference ranks.

While we shall not go into more detail on the unfolding technique, it should be clear by now that for any particular scale of sweetness, a limited number of possible preference rankings can occur, and if we have enough different subjects rank the stimuli to obtain a large number of different preference positions, then we can not only learn the ordering of the stimuli, but we can also order the sizes of intervals between the stimuli. Thus we can obtain an ordered metric scale.

Notice that with both the Coombs and the Guttman techniques, internal consistency is a critical part of the procedure. Furthermore, in both techniques the same basic principle is used to determine internal consistency: to limit the number of possible results which can occur and which are acceptable as satisfying the measurement requirements for the scale. In the Guttman technique, with dichotomous items, it is possible for 2^n patterns of results to occur with n different items, but only $(n + 1)$ results are accepted as satisfying the ordinal requirements. In the Coombs technique, there are $n!$ ways of ordering n items, but for any scale spacing, only $[\frac{1}{2}n(n-1)+1]$ possible orderings will occur. In our example of five stimuli, there are 120 ways of ordering the stimuli, but only eleven ways for any one actual spacing.

MANIFEST-INTERVAL TECHNIQUES The principal aspect of the manifest-interval scale techniques is that some direct response is required, and either because of instruction to the subject, or assumption on the part of the experimenter, the data are treated as though they have interval properties. The major differences in technique have to do with the kinds of stimulus objects used, and the limitations these put on the experimental procedure.

Equisection. Suppose that we want to determine psychological scale values for stimuli which are on a true physical continuum—one which can be manipulated easily to produce continuous variation. There are not many physical continua of this sort which are of interest to the psychological scaler, but there are some. Both frequency and intensity of sounds can be so manipulated; so also can physical brightness, and even hue or saturation, with a sufficiently complicated apparatus; electric shock, vibration, and the intensity of odors are others.

For our example, we shall use intensity of sounds, with loudness being the psychological attribute whose scale we wish to determine. The procedure is fundamentally very simple: We provide two tones of fixed intensity, and then require that the subject adjust the intensity of other tones until he has them arranged to provide a series of equal loudness intervals.

Each of the tones thus produced, plus the two tones which defined the original range of loudness, is assigned a numerical value with equal interval spacings to correspond to the assumption that the tones produced by the subject provide an interval scale of loudness. In order to provide generality to the resultant scale, we would ordinarily have each subject make the adjustments several times, and we would use several different subjects. Some measure of central tendency (usually the mean) of the intensities is then used to define the average value of the points on the intensity continuum which provide an interval scale of loudness.

The number of stimuli which are to be adjusted by the subject is a matter of choice on the part of the experimenter. In the limiting case, where two end stimuli are provided, and the subject then adjusts a single stimulus to a value midway between the two, the method is called "bisection," because the subject is required to bisect an interval.

In order to provide a check on internal consistency, it is a good procedure to use at least two different sets of values for the fixed end stimuli, and to have the ranges overlap. Then we would want to use enough stimuli in each range to give a substantial overlap in the number of stimuli common to both sets. In the illustrative data shown in Fig. 1-2, seven stimuli (two end stimuli plus five adjustable stimuli) were used with two ranges of intensity (50 to 90 db sound pressure level, and 70 to 110 db). The same subjects were required to adjust five stimuli within each range, and the data shown in Fig. 1-2 are averages of the adjusted intensities. For each range, the numerical values 3 through 9 were assigned to the seven stimuli. This is shown by the dots for the higher range, and the triangles connected by dotted lines for the lower range.

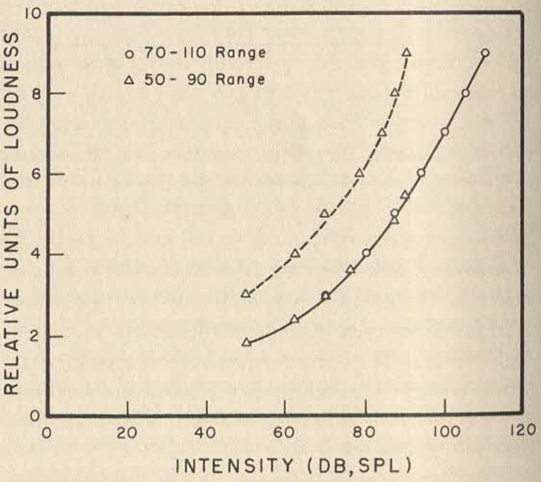


Fig. 1-2. The equisection technique, using seven stimuli over two different ranges of audio power. The solid line shows the final scale, and the data for the 70 to 110 db range. The dotted line shows the untransformed data for the 50 to 90 db range. These were transformed to yield the final scale by the formula $y' = .58y - .025$. (After Garner, 1954*a.*)

To construct the final scale, shown by the solid line, the interval size of 1 unit was contracted for the data from the lower range, and the whole curve was moved downward by subtracting a constant. It should be remembered that with an interval scale we are free to use any linear transformation of the scale values, which is to say that we can adjust both the intercept and the slope to provide the best fit of the two overlapping sets of equisection data. In other words, we use two degrees of freedom from the lower values to establish our scale unit for the upper values, and it should now be clear why it is best to use several intermediate stimuli in equisection. If we had only two overlapping points, then with two degrees of freedom to determine the scale values, we can assure that the two sections of the curve will fit together in the overlapping range. It would still be possible for the two sections to provide an apparently discontinuous function, but an oddly shaped or broken curve would be our only check on the validity of the interval assumption.

On the other hand, if three or more stimulus values overlap, then the curvature, or the shape of the function, can be seen to be the same or different for the same range of intensities. Clearly if our assumption that an interval scale can be formed is valid, then we should obtain the same scale for the same range of intensities, regardless of the end stimuli used to obtain these particular scale values. Thus we can provide a check on internal consistency, while at the same time extending the range of intensity over which we obtain a psychological scale.

Equal-appearing Intervals. If the objects which we want to scale cannot be measured by a continuous physical dimension, then we must resort to other procedures. Suppose, for example, that we wanted to scale verbal ability in individuals as judged by other people; or we wanted to scale the "goodness of pattern" of several geometric patterns. In these cases we are not able to tell the subject or rater to adjust the stimuli to provide equal intervals, because the stimuli are fixed, and cannot be adjusted.

In this case we can instruct the subject to give a number to each stimulus, so that the numbers he uses are spaced by equal intervals. Or, alternatively, we could have the subject sort the stimuli into adjacent categories, with the instruction that his sortings are to satisfy the interval requirements. The data would be in exactly the same form as with the method of category ranks, as shown in Table 1-3. The only difference lies in the instruction to the subject to use his categories as equal intervals rather than as ranks. With the assumption that he does so, however, it becomes legitimate to use the mean category value for each stimulus, rather than the median, as we suggested before.

To contrast the method of equal-appearing intervals with that of equisection, we might note that in the latter method stimuli are chosen or set by the subject to define a series of points on the attribute continuum, and the intervals between these points are then assumed to be equal. With equal-appearing intervals, however, we never have stimuli which define equal intervals as points on the continuum. Nor do the category values either used or assigned do so either, since they define a range of values, not points. Rather, the assumption is that each response category provides a range of values, an interval, which is equal to all other intervals. And it is also assumed, of course, that the intervals are in proper order and are contiguous.

Numerical Category Scales. The method of equal-appearing intervals is normally used where a large number of stimuli must be judged, and each subject rates or judges each stimulus just once. A similar (in fact, logically identical) method is used where there are relatively few stimuli, but each stimulus is rated several times by each subject, on a numerical scale, with instructions to the subject to use the scale values as an interval scale. The term "category scale" is usually applied to this method, and we have called it the method of numerical category scales to distinguish it from those methods in which verbal, or even spatially arranged, categories are used.

As an illustration of the use of this scale, Fig. 1-3 shows some data from an experiment by Torgerson (1960). For stimuli, seventeen shades of neutral gray paper were used, and each was judged five times by each of sixteen subjects. The stimuli were presented in random order, and the subjects rated each stimulus on an eleven-point scale (0 through 10), and they were instructed to use the numerical scale as an interval scale. For one set of experiments, they judged lightness, with the larger numbers indicating more lightness, and for another set they judged darkness, with the larger numbers indicating more darkness. Figure 1-3 shows the mean category ratings as a function of reflectance, for each type of judgment. In this particular experiment, internal consistency was demonstrated by the fact that the function for the attribute of lightness is the inverse of the function for the attribute of darkness. This check, of course, is possible only where the same stimuli

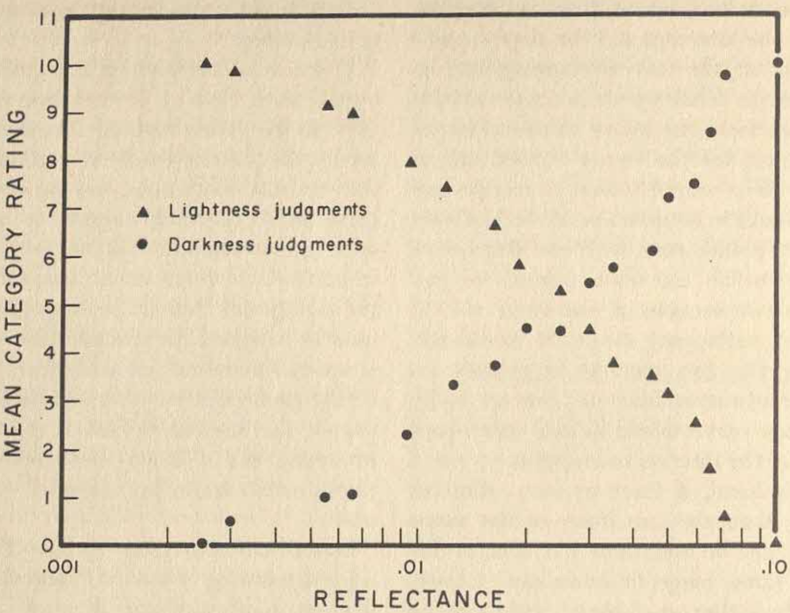


Fig. 1-3. Results of a numerical category scaling experiment. In one case the subjects were asked to judge relative lightness, and in the other, relative darkness, each on an eleven-point scale. (After Torgerson, 1960.)

can be judged on two attributes which are themselves the inverse of each other.

Verbal Rating Scales. One last technique is worth mentioning briefly, and that is the verbal rating scale. The term "rating scale" is itself a very general term which can be used to describe any response scale used by subjects in a rating or judging task. A verbal rating scale differs from a method of equal appearing intervals or the numerical category scale only in that verbal labels are used as the responses. For example, we might have a seven-category scale for judging brightness: very very dark, very dark, dark, neutral, light, very light, very very light. If in summarizing the data we assign equally spaced numerals to these response categories, and use metric statistics, we are in effect assuming interval properties of the scale. The only essential difference between this method and other manifest-interval techniques lies in the kind of response scale used.

The usual grading scale, mentioned earlier as an example of an ordinal scale, is often treated as if it had interval properties, as if it were a verbal rating scale. This is done when arbitrary numbers are assigned to the grades for the purpose of making "grade-point" averages. The assumption is that the distance between an "A" and "B" is the same as the distance between "D" and "E"—precisely the assumption of the interval scale—when consecutive numbers are assigned to the grades.

Checks on Internal Consistency. Techniques of scale construction based on manifest properties of the data do not ordinarily include self-checking procedures for internal consistency as an inherent aspect of the technique. So with these techniques the internal consistency check must be provided by carrying out two or more experiments which should logically lead to the same resultant scale, either in whole or in part.

The Torgerson experiment, with judgments of lightness and of darkness, illustrates one method of providing a check on internal consistency, but obviously such a check depends both on the nature of the stimuli and on the nature of the attributes. There is, for example, no inverse attribute for pitch, for skill, or for most aptitudes.

The other easily used check on internal consistency is the one we mentioned for the equisection technique—namely, duplication of the experiment with different but overlapping ranges of stimuli. In the case of the loudness experiment we had a physical dimension which corresponded to the attribute, so we could select fixed end stimuli ahead of time and there was reasonable assurance that overlapping scale values would result. The problem is somewhat different with stimuli such as art objects, foods whose relative preference we are trying to scale, or sociability of people. Here, as we saw, the equal-appearing intervals technique is usually used, since we cannot give our subjects direct control over the stimuli. In this case an equivalent procedure to the consistency check used with equisection is possible. We can conduct subexperiments in which three or more stimuli are duplicated in different sets of stimuli. We then would have the same sort of check as before, where the scale values assigned to the stimuli are transformed so that the common ones from the different subexperiments coincide. If this can be done with three or more stimuli there is strong evidence that the interval property assumed for the responses is valid. Unfortunately this sort of internal check is seldom performed.

LATENT-INTERVAL TECHNIQUES There are logically many ways to construct psychological scales with interval properties based on latent properties of the data—and in a sense the Coombs unfolding technique is an illustration. But in psychological scaling, essentially all techniques are based on the acceptance of manifest ordinal properties, with interval properties added by a further assumption about the statistical *distribution* of the scale values. And the common assumption is that psychological processes are distributed according to the normal distribution.

Over 100 years ago Fechner first constructed a psychological interval scale based on latent-interval properties. He integrated jnd's to give a scale of sensory magnitude, using the assumption that all jnd's are equal in sensation. This assumption made it possible to construct a scale with interval properties.

Thurstone (1927) extended the principle used by Fechner, to provide a much more general law. In effect Fechner had assumed that differences noted equally often constitute equal psychological differences, since a jnd is defined as a constant proportion of judgments that one stimulus is greater than another. But Fechner made no assumption about the distribution of the judgments. Thurstone's extension was to assume that the psychological process which leads to difference judgments is normally distributed. From this assumption psychological distance can be deduced if we know the proportion of times that one stimulus is chosen over another, since there is an invariant relation between probabilities and normal deviates or standard scores. Thus if we know the proportion of times that A is chosen over B, we can determine the interval distance between A and B by converting the proportion into the equivalent standard score from normal curve tables. Thurstone developed a fairly complete mathematical model based on this *Law of Comparative Judgment* (the name given to the normality-interval assumptions), but for our purposes, the important principle is that psychological distances with interval properties can be deduced from proportions of ordinal judgments.

Thus in all of the latent-interval techniques, the manifest data have ordinal properties and these are accepted as valid. With the assumption of the normal distribution of judgment or psychological process, interval properties are obtained for the psychological scale. The differences between the various techniques have to do with the nature of the manifest data.

Normalized Ranks. The simplest procedure for obtaining a manifest-ordinal scale is to have several subjects rank order a set of stimuli. With the illustrative data of Table 1-1, we could then sum up the rankings to obtain generalized, or average, rank, but we pointed out that the stimuli should then be reranked to avoid the implication that the unequal numerical differences between successive sums have any interval meaning. The size of these differences can have no meaning, because only ordinal judgments were made, and we do not know the interval distance between successive ranks.

If we assume, however, that the stimuli are normally distributed in scale value, then we can construct an interval scale. The procedure to use is as follows: In our illustration we had five different stimuli to be ranked. If these stimuli are normally distributed, then our best working assumption is that each stimulus occupies 1/5 of the total continuum of possible values. In other words, the lowest ranked stimulus lies somewhere between 0 and 20 per cent of the total range of values, the second stimulus lies somewhere between 20 and 40 per cent, the third between 40 and 60 per cent, the fourth between 60 and 80 per cent, and the fifth between 80 and 100 per cent. Now we do not know exactly where each stimulus lies, but our best estimate is that it lies in the middle of its range of possible values. So we assume that the first stimulus is at the 10th percentile, the second at the 30th percentile, and so on to the last, which is at the 90th percentile. The standard score equivalent of each percentile is then determined from normal tables, and these values are accepted as our best estimate of the interval scale values. For example, the normalized rank for a rank of 1 is +1.28, which is the normal deviate equivalent of .90.

The ranks for each of the four raters in Table 1-1 are shown as normalized ranks in Table 1-5, and now we can reasonably take the mean of these normalized ranks, since we are averaging values with interval properties. These mean normalized ranks are shown at the bottom of the table.

Since both positive and negative numbers make up the scale, it becomes hard to interpret. Often the numbers are transformed so that they are all positive. In the last line of the table this has been done, and in addition, the numbers have all been multiplied by 10, to give them more spread. In an interval scale all that we must preserve is the relative sizes of the distances between scale values, and this has been done with the transformation. The final scale is related to the original by the formula $Y = 10X + 11.925$.

In our example we had our subjects rate just five objects. If more objects are rated the same basic procedure is used, but the total continuum is simply divided into more steps. So, for example, if we had ten ranks, we would have divided the total range into ten equal parts, 0 to 10, 10 to 20, etc. And the midpoints of these ranges, 5, 15, 25, etc., would have been used to determine the normal deviate equivalent value. Otherwise the technique would be just as illustrated.

Normalized Pair Comparisons. The second technique we discussed for obtaining manifest-ordinal scales was pair comparisons, in which each object is paired with every other object in the set. A group of subjects is then required to indicate a preference for one object from each pair. The data from such a technique were shown in Table 1-2, where each cell shows the proportion of times each painting was chosen over each other painting. These data can be used to construct an interval scale with the normalizing assumption, if we convert each of the proportions into its equivalent standard score, and from these standard scores compute mean distances, in standard score units, between successive stimuli. Once again, as with normalized ranks, the manifest ordinal property of the data is accepted.

The rationale for the scale construction is as follows: First, consider each column in Table 1-6. The values in the first column show the normal deviate scores when each painting has been compared with the first painting. Since the painting against which each of seven paintings has been compared is constant for that one column, then, with the normal-interval assumption, the values in that column can be used directly as interval scale values for all of the paintings. A scale value of 0 is assigned to the first painting—the standard—quite arbitrarily, and this procedure is perfectly all right, since we are only constructing a scale with interval properties, and such a scale has no true or absolute zero point.

TABLE 1-5

The normalized rank procedure*

Raters	Joe	Bill	Andy	Jack	Sam
Foreman	.53	1.28	0	−.53	−1.28
Superintendant	0	.53	1.28	−1.28	−.53
Shop steward	.53	1.28	−.53	0	−1.28
Psychologist	0	.53	−.53	1.28	−1.28
Sum	1.06	3.62	.22	−.53	−4.37
Mean normalized rank	.265	.905	.055	−.132	−1.092
Transformed scale	14.575	20.975	12.475	10.600	1.000

* Each entry is the standard score equivalent of the rank given in Table 1-1. The transformed scale (Y) is related to the original scale (X) by the formula $Y = 10X + 11.925$.

In a logical sense, one column of comparison data is all that is necessary to construct the latent-interval scale. However, we could just as well use any column instead of the first, because in each column seven paintings have been compared to a single standard. In each column, however, the scale we construct would assign the value of 0 to the painting which had been used as the standard of comparison. Now, if the data were perfectly reliable, and the interval scale assumptions involved were valid, each of these eight scales would be exactly the same except for the arbitrary location of the zero point. Since the location of the zero value is quite arbitrary, we could simply change each set of scale values so that the same stimulus was given a value of 0. Then we could average the scale values for each stimulus to obtain a more general scale.

There is a major difficulty with this simple procedure, however, which is illustrated in Table 1-6, and that is that each column—with the eight different paintings used as standards—does not provide an actual scale value for each of the eight paintings, since in extreme cases one painting is chosen or preferred over one or two others all, or nearly all, of the time. In these cases standard scores are indeterminate. Thus we have eight different scales in the eight columns, but not all of the same stimuli are scaled in each column.

What we must do is determine the difference between each pair of adjacent stimuli by computing the average difference in scale value for all scales where both paintings of a pair had scale values. To illustrate, both paintings A and B have scale values in the first five columns, so we can obtain five estimates of the difference between A and B by using the scale values in these five columns. The average difference between scale values is, for these five columns, .592 standard score units.

In like manner, we determine the average difference in scale value between B and C, using the seven columns in which scale values occur for both of these stimuli. And so on, until we have scale value differences between all successive stimuli, the differences in each case being the mean of all the differences which occur for that pair of stimuli. These mean differences are shown at the right of Table 1-6.

The final scale values are then obtained by assuming the successive scale value differences as shown in the last column of Table 1-6—and once

TABLE 1-6

Normalized pair comparisons, using the data from Table 1-2*

		Paintings								Mean Difference	Scale Value
		A	B	C	D	E	F	G	H		
Paintings	A	0	−.52	−1.08	−1.55	−1.55	—	—	—	.592	2.786
	B	.52	0	−.31	−.77	−1.18	−1.41	−1.41	—	.371	2.194
	C	1.08	.31	0	−.47	−.52	−1.08	−1.28	−1.55	.392	1.823
	D	1.55	.77	.47	0	−.47	−.36	−.92	−1.41	.189	1.431
	E	1.55	1.18	.52	.47	0	−.31	−.99	−1.28	.276	1.242
	F	—	1.41	1.08	.36	.31	0	−.36	−1.28	.381	.966
	G	—	1.41	1.28	.92	.99	.36	0	.77	.585	.585
	H	—	—	1.55	1.41	1.28	1.28	.77	0		0

* The proportions were taken to be estimates of the probability that each painting would be chosen over each other painting, and the numbers here are the standard-score equivalents of these probabilities. At the right is shown the mean distance in standard score units between the paintings, and the final scale, with the least preferred painting assigned a value of zero.

again, we can shift these scale values to provide any zero point we wish. We actually can multiply the scale values as well, and there are theoretical reasons for multiplying by the square root of 2. It is probably just as well to leave the numbers in this form, however, since they are easily and directly interpretable back to the original proportions from which they were obtained.

The method of normalized pair comparisons provides an internal consistency check which is not possible with normalized ranks. It will be recalled that we could consider each column in Table 1-6 to provide an estimate of the scale values for all stimuli, but we then use the mean scale differences to obtain our final set of scale values. Each separate scale, however, should correspond to this average scale within the expected error of measurement, if the zero point of the scale is adjusted. If we use the average scale to predict each separate scale, we can convert the scale values back into expected proportions, and if these predictions do not correspond, again within error of measurement, we must reject the assumption that all scales are in fact the same. In such a case, there are many alternative models which can be used, the most common of which is to assume that the standard deviations are not equal. In other words, we keep the normality assumption, but we do not assume that the normal distribution for each stimulus has the same standard deviation. But it is not our purpose to go into this level of detail here.

Normalized Category Ranks. The third technique for obtaining manifest-*ordinal* scales is that of category ranks, and once again the data can be used to make a latent-*interval* scale if we make the normality assumption. The data which we will use are from Table 1-3, for the method of category ranks. However, data can just as well be used from the methods of equal-appearing intervals, numerical categories, or rating scales, if we assume that the only valid manifest property of the obtained data is the ordinal property.

Many techniques with different names have been suggested for making latent-interval scales based on category data. As Guilford (1954) points out, however, they are all basically the same. Saffir (1937) first used the method, basing his procedure on a technique developed by Thurstone and called the technique the method of *successive categories*. Guilford (1938) proposed a method which he called the method of *absolute scaling*. Attneave (1949) proposed what he called the method of *graded dichotomies*, and Garner and Hake (1951) described a method for constructing an *equal discriminability* scale. All of these methods, however, involve the same essential principles, depending primarily on the normality assumption to create a latent-interval scale from data with manifest-ordinal properties.

The first step in constructing a latent-interval scale is illustrated in Table 1-3 itself, where the lower of each pair of numbers in each cell is the cumulative proportion that each stimulus was ranked in the given category or lower. Thus these cumulative proportions must be considered to be the proportion of judgments falling below the upper category limit for each category rank. Naturally, all cumulative proportions are 1.00 for the highest category ranks, since each stimulus pattern was given a category rank by each of the twenty raters, and the proportions must sum to unity.

Now we can assume that these proportions are normally distributed, and can convert the cumulative proportions into standard scores from the normal curve tables. These values, shown in Table 1-7, are the assumed psychological scale values for the upper response category limits, and the last (fifth) category rank has no value because the normal curve deviate score for a proportion of 1.00 is at plus infinity.

We now have a scale value for the upper interval limit of each category rank, except in those cases where the cumulative proportion is at or near 1.00. (We have, as is customary, not used cumulative values above .95 or below .05 because of the high statistical error obtained with such proportions.) We can consider, however, that at each upper interval limit we also now have a scale value for each successive stimulus. Thus each row provides us with a set of stimulus scale values. We can obtain estimates of the average scale difference between successive stimuli from the columns in which both stimuli of the pair have actual values, and we can cumulate these average differences to obtain a final scale for the stimuli. The zero value can be assigned to any stimulus we choose. This calculation is illustrated in Table 1-7.

A check on internal consistency is provided in much the same manner as with normalized pair comparisons. We can use the average scale to compute the differences between stimuli at the upper interval limits, and then convert these back to cumulative proportions. Then we can compare these with the original proportions and decide whether the same psychological scale was operating for all stimuli. If not, then other models can be used, especially those which assume unequal standard deviations.

MANIFEST-RATIO TECHNIQUES Any technique based on manifest-ratio properties of a subject's or rater's responses must make the parallel assumptions that the attribute in question actually has ratio properties and that subjects can directly perceive and describe these properties. Specifically, the ratio property requires that the attribute has an absolute zero, since without an absolute zero an expression of ratios is quite meaningless (see pp. 4–5).

In some form, then, manifest-ratio techniques require that a subject express a relation between two or more stimulus objects in a ratio form. The objects need not, of course, be what we would ordinarily call stimuli but could be people, although the ratio techniques have rarely been used with such objects.

Ratio Production. Probably the oldest technique of this sort is ratio production. The method has frequently been called "fractionation," since a subject is required to produce one stimulus magnitude which is some specified fraction of another, but since the subject can just as well be asked to produce a stimulus which is a constant multiple of the other, the broader term, ratio production, is preferable.

As we have just indicated, the basic procedure is to give the subject a standard stimulus which presumably has the attribute under consideration, and then require that the subject produce another stimulus which bears some constant stated ratio to the standard. This method is, of course, similar to equisection in that the subject produces a stimulus (he could even produce a series of stimuli) which has some stated numerical relation to a standard. As with equisection, the method is amenable only to those psychological attributes in which there is an easily manipulable physical dimension which is a counterpart to the psychological attribute. Loudness has been the most frequently used attribute, and we shall use it to illustrate the method.

In its logically simplest form, we would start with a loud standard stimulus and require a subject to produce a stimulus which is half as loud. If we arbitrarily assign a numeral, say, 100, to the standard, then the numeral 50 would be assigned to that stimulus which the subject says is half as loud. Then we would use that stimulus as a standard, and ask the subject to produce one which is half as loud as it, and we would then assign the numeral 25 to it. And so on to a loudness of 12.5, 6.25, etc.

Actually, this logically simple procedure is rarely used because of the possible experimental biases which could occur with the successive decrease in loudness. So the usual procedure, illustrated in Fig. 1-4, is to use a series of fixed standards, say every 10 db. Then each subject adjusts another stimulus to sound half as loud as each standard. The standard stimuli are used in random or counterbalanced order, and each of several subjects may make several productions for each standard. These various values are then averaged

TABLE 1-7

Normalized category ranks, using the data from Table 1-3*

Category	*Stimulus Patterns*						
	1	2	3	4		19	20
1	00	−.25	−.13	−.52		−1.28	—
2	.84	.67	.25	.13		−1.28	−1.28
3	1.04	1.28	.67	.67		−.67	−.84
4	—	1.28	1.28	1.04		.38	.25
5	—	—	—	—		—	—
Mean difference		.060	.287	.187			.100
Scale value	0	.060	.347	.534		10.310	10.410

* In this case we have used the cumulative proportions to give standard-score values.

2208

2208 3

150 HEL

to yield an average curve showing the relation between the intensity of the standard and the intensity judged to be half as loud. This is the experimentally obtained curve shown in Fig. 1-4.

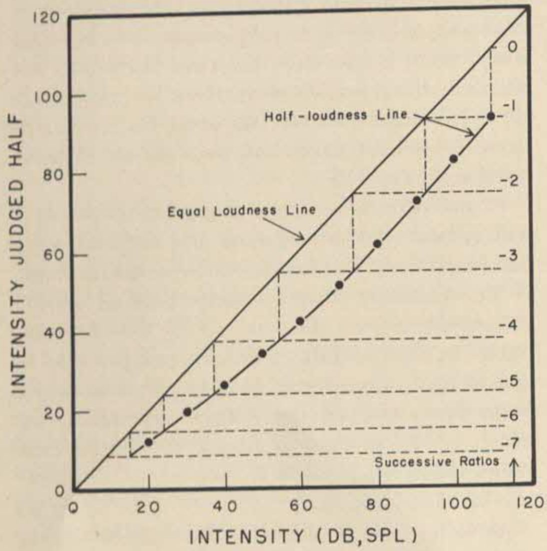


Fig. 1-4. The ratio production method, as applied to half-loudness judgments. The points show the mean half-loudness setting for each value of sound intensity. When 110 db was presented, the mean setting was 95 db, etc. The line drawn through these points enables us to estimate what would be the value set as half of any given sound intensity. The dotted lines are used to estimate the expected values of successive halvings, which are used to construct the scale of Fig. 1-5.

The psychological scale of loudness is then constructed by interpolating to obtain successive half-loudness intensities. To illustrate, we can arbitrarily assign a value of 100 to an intensity of 110 db, and then we read from the function in Fig. 1-4 the intensity which is judged, on the average, to be half as loud. This intensity is assigned a scale value of 50, and then we determine, by interpolation, what intensity was judged half as loud as it. That intensity is given a value of 25, and it then is entered to determine what intensity was judged half as loud as, etc., to intensities as low as were experimentally used. The loudness scale thus obtained, as a function of intensity, is shown in Fig. 1-5.

The kinds of internal consistency checks which can be used with this procedure are primarily concerned with the ability of the subjects to use the ratio numbers correctly. We could, for example, run exactly this same experiment except that the subject is required to produce a stimulus twice as loud; or he could be required to produce stimuli one-third as loud, or even three times as loud. Each of these procedures, of course, should lead to the same loudness scale if subjects use the numbers correctly.

Magnitude Estimation. With the ratio production technique, the subject is given a numerical value and is required to adjust stimuli to satisfy the criterion. With magnitude estimation, as with the manifest-interval techniques of equal-appearing intervals, numerical category scales, and rating scales, the stimuli are given and the subject is required to indicate, in this case always numerically, the relations between the stimuli.

There are two magnitude estimation procedures which can be used. The first is simply to present one stimulus at a time and require the subject to indicate its numerical value, with any modulus or unit of measurement he chooses. This procedure is a direct numerical rating procedure, and the scale values are simply the mean numerical values produced. The ratio properties of the scale depend entirely on the assumption that ratio properties were in fact used by the subjects.

The second procedure is to present one stimulus

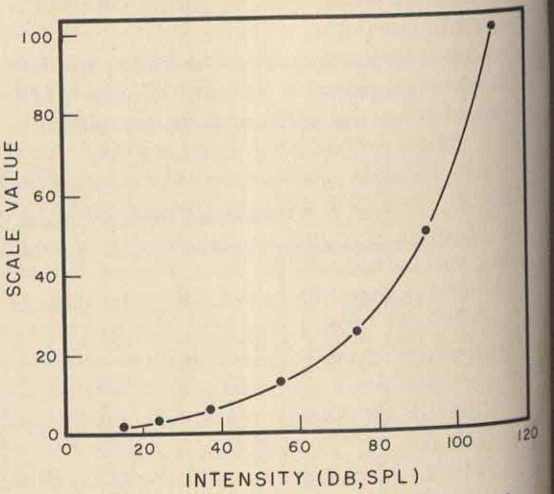


Fig. 1-5. A loudness scale constructed from the data of Fig. 1-4, assuming half-loudness judgments reflect halving of subjective intensity. The upper point, at 110 db, is called 100. Then the next point, set as one half of 110 db, is plotted at 50, etc. (After Garner, 1954*a*.)

as a standard and assign to it a numerical value beforehand, which becomes the modulus, or unit of measure. Then other stimuli are presented, and the subject is required to state what numerical value should be assigned to each, given the standard modulus. The standard stimulus can be presented each time that another comparison stimulus is presented, or it may be presented only occasionally.

In order to provide a check on internal consistency, it is preferable to use at least two different standard stimuli, either with the same or different numerical modulus. Table 1-8 shows some illustrative data in which two different standard stimuli were used, each with the same numerical modulus. The stimuli were weights which were lifted. The values shown are the mean numerical values given by the subjects for each of the other weights.

TABLE 1-8

Results of a magnitude estimation experiment in subjective weight, using a standard*

Weight	*Standard* 90 g	*Standard* 190 g	*Converted* 190 g scale
20	10		
30	19		
40	29		
60	54		
70	68	21	68
→ 90	100	31	100
102	120	38	123
110	140	43	139
120	160	49	158
160	250	76	245
180	300	92	297
→ 190	320	100	323
220	400	128	413
280		190	613
350		260	839

* The first data column shows the median number assigned to each weight listed at the left, when a weight of 90 g was assigned the number 100. The next column shows the median number assigned to each weight when the standard was 190 g, again called 100. The final column shows the transformed scale from the 190 g data. Each number was multiplied by 100/31 to get a new value where 90 g again has the value 100. (After Stevens and Galanter, 1957.)

Each of these two sets of data can be used to construct a psychological scale of weight, or we can convert both of them to the same numerical modulus to construct a single composite scale. We simply multiply (or divide) all the numbers obtained with the second standard so that the numerical value of the second standard corresponds to its value when judged against the first standard. A ratio scale allows any multiplicative transformation, so this change is quite valid. Note, however, that we are not free to add or subtract a constant from either scale, since the ratio scale does not allow such a transformation. If we had to do so in order to make the two scales agree, we would have evidence that the numbers were not being used as ratios.

These two scales, transformed to the same numerical modulus, are shown in Fig. 1-6, and in our particular example, the two scales agree very well.

The Constant Sum Method. Metfessel (1947) suggested still a third technique for obtaining ratio scales, and this technique has come to be called the constant sum method. This technique is analogous to the method of pair comparisons, except that subjects make ratio rather than ordinal judgments.

The experimental situation is one in which we have a fixed number of stimuli which we want to scale on some attribute, and since the stimuli remain constant, it is not necessary that there be a physical dimension corresponding to the attribute. Each stimulus is paired with each other to form all possible pairs. The pairs are then presented to a subject one at a time, and he is required to assign numerical values to the two stimulus objects of the pair. The restriction on his numerical assignment is that the sum of the two numbers used must be some constant stated by the experimenter, usually 100. Thus, to illustrate, a subject may say that stimulus A receives a value of 60, and B a value of 40. Now if a subject can divide a fixed sum this way, it implies that he can form ratios, and we are free to deduce that the numerical ratio between the scale values for stimuli A and B is 1.5 to 1. The subject makes such a judgment for every pair and he may make several judgments of each pair. In addition, several subjects will be used to give generality to the scale.

There are alternative ways of treating the data, and it is possible to use the data in their direct numerical form. Torgerson (1958), however, has suggested a procedure which is arithmetically very simple, and which keeps the rationale of the procedure quite similar to the treatment of data with normalized pair comparisons.

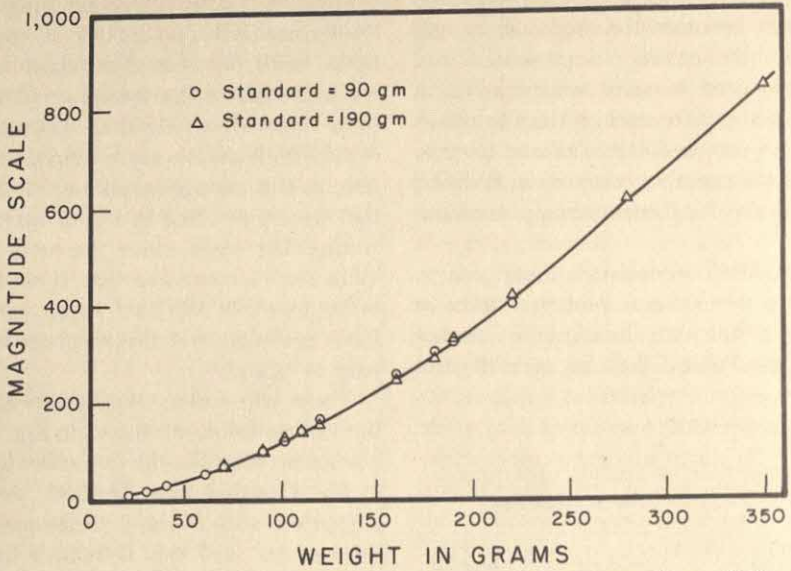


Fig. 1-6. A scale of subjective weight, based on magnitude estimation from Table 1-8. Two sets of points are from the two different standards, and the overlap of the two sets is a check on consistency.

The essence of the procedure lies in the relation between logarithms and ratios. If we take logarithms of the numerical values on a ratio scale, then equal logarithmic differences are equivalent to what were originally equal ratios. Thus a series of numerical values on a ratio scale of 1, 2, 4, 8, and 16 become in logarithms 0, 0.3, 0.6, 0.9, and 1.2. So if we convert our ratio numerals obtained from the data into logarithms, we can deal with numbers in which a constant difference always means a constant ratio, regardless of the actual values of the original numbers.

Our procedure, then, is to take each pair of numbers which a subject produces, express the numbers as a single ratio, and then convert this ratio into its equivalent logarithm. Thus the values of 60 and 40 assigned to A and to B, are expressed as a ratio of 1.5 to 1, with a logarithmic equivalent

TABLE 1-9

The constant sum method*

	Bill	Joe	Andy	Jack	Sam
Bill	0	−.71	−.66	−.88	−1.17
Joe	.71	0	−.15	−.36	−.59
Andy	.66	.15	0	−.07	−.22
Jack	.88	.36	.07	0	−.18
Sam	1.17	.59	.22	.18	0
Mean difference	.606	.182	.122	.206	
Log scale	1.116	.510	.328	.206	0
Ratio scale	13.06	3.24	2.12	1.67	1

* Each worker was paired with each other worker and the raters were to divide 100 so as to reflect the ratio of actual performance. The ratios were converted into logarithms, and the entry in each cell is the mean of the logs, reflecting the average ratio for the worker listed at the top to the worker listed at the side of the table. The mean difference between the columns is then made into a scale by successive addition (as in an interval scale), and the final scale is the antilog of each value.

of 0.176. This value then represents the logarithmic *difference* between A and B, which is equivalent to the ratio.

For each pair of stimuli, we obtain the mean of all such logarithmic differences, and these means are entered into a table in Table 1-9. The value of 0 occurs on the diagonal because it is assumed that if each stimulus were compared to itself the numerical values would be 50 and 50, giving a ratio of 1.0 and a logarithm of 0.

Now this table can be interpreted exactly as Table 1-6 was. Each column of the table gives a perfectly valid psychological scale, in which all stimuli have been compared with a single standard, but the standard is different in each column. Except for error of measurement, these scales should all be the same, although the zero point is different for each scale because of the different standard stimulus used. This is then an interval scale in logarithmic form. A change in the zero point here amounts to multiplication by a constant.

To obtain an average scale, we can, just as with normalized pair comparisons, compute the average difference between scale values for all pairs of stimuli, regardless of the standard. If some stimuli received all 100 points when compared to some others, then we have no usable scale difference, because the ratio is infinity. So we obtain the average only for the real pair differences, and then add up these differences to get the complete scale in logarithmic form. All that is now required is to reconvert these values back to numerical form. We can, of course, multiply these numbers by any constant value we choose.

Our internal consistency check lies in the fact that each stimulus was used both as a standard and as a comparison for every other stimulus. In other words, each stimulus serves as a modulus, and a change in modulus should not change the scale except for a multiplier constant. If the scales are not the same, then we know that our assumption of ratio properties is not valid.

A LATENT-RATIO TECHNIQUE A scale with more powerful measurement properties presumes all the properties of the less powerful scale. Thus an interval scale also has the ordinal property, and a ratio scale has interval properties as well as ordinal properties. This fact suggests that a logical procedure for checking on the internal consistency of any scale is to show that the scale obtained is consistent with properties of a lower-order scale. To be more specific, suppose we have constructed a loudness scale with any of the three manifest-ratio techniques. It is quite possible to obtain a scale which is consistent within the same framework of measurement, but which might not be consistent with properties of a scale obtained with, say, a manifest-interval or a latent-interval technique. Since there are major differences in these techniques, particularly when latent procedures are involved, we might not always expect consistency between them, and to some extent the lack of consistency can be justified on the basis of the fundamentally different assumptions involved.

However, many of the manifest techniques are basically the same, and are applied to interval or to ratio scaling problems with the only change being the assumed ability of the subject to make use of scales with higher (but inclusive) properties. Thus the ratio production technique is exactly the same as the equisection technique except that in the latter case the subject is instructed to produce equal intervals, while in the former he is instructed to produce a given ratio. In similar fashion, the magnitude estimation technique is the same as the numerical category techniques (and even the method of equal appearing intervals, or rating scales).

In these cases of analogous techniques it would be reasonable to assume that a scale based on the ratio technique could accurately predict the scale based on its equivalent interval technique. Rarely, however, do these analogous procedures lead to the same psychological scale (see Stevens and Galanter, 1957), a fact which must raise a serious question concerning the validity of the manifest procedures. When such failures of agreement occur, we do not, of course, know which technique is at fault, or whether either technique is valid.

It is possible, however, as Stevens (1951) has shown logically, to produce a scale with ratio properties which does not require the same strong assumptions about the manifest properties of the responses as are necessary with manifest-ratio techniques. Thus a ratio scale can be constructed based on latent properties of data.

A technique was developed and used by Garner (1954*a*) for doing just this with loudness. First, we construct two loudness scales, the first based on ratio production (specifically, fractionation to half loudness), and the second based on equisection. Now, however, we do not make the assumption that the ratio of half was in fact used by the subjects, but rather make the much weaker assumption that the ratio was the same for all judgments, but of unknown value. With this assumption, we can still determine a loudness function, but we do not know the ratio that was used. This function will be related to the true (latent) loudness function as

$$L = ab^x$$

where L is the true loudness function, a is an arbitrary unit of measurement, b is the value of the unknown ratio, and x is the value of the successive ratio. We can assign any value we choose to a, so we are left with a single unknown value on the right of this equation.

From the data of the equisection experiment we can also determine the loudness function except that we do not know the value of the intercept constant—the location of the zero point. In equation form, we know that

$$L = c(Y - d)$$

where L is as above, c is an arbitrary unit of measurement, Y is the loudness value assigned from the interval data, d the unknown intercept constant.

Now in reality we have only two unknown terms, the value of the ratio from the fractionation experiment and the value of the intercept from the equisection data. But we have two independent sets of data, and we can thus estimate these two unknown values so as to lead to the same loudness scale. We need not here go into the details of the arithmetic procedure used to provide the estimates, but what is important is that these estimates must satisfy the condition of a single loudness function. Thus this procedure, as with the Guttman and the Coombs techniques, has the check on internal consistency as its primary requirement, and a psychological scale is not formed unless the internal consistency check has shown the validity of the assumed attribute.

There have been relatively few attempts at using latent scaling techniques to construct ratio scales of psychological attributes. Michels (1954) has published a scale of brightness based on fractionation judgments, and Michels and Doser (1955) have done the same for a loudness scale. But their techniques, which we shall discuss at greater length a bit later, are more concerned with theorizing about the nature of the scale than with developing a method for constructing one.

OTHER MEASUREMENT TECHNIQUES The problem of measurement in psychology is ubiquitous. To discuss techniques of psychological scaling requires considerable arbitrary selection, since the logical measurement problems associated with what we have called scaling occur in other areas of psychology as well.

Psychologists measure such things as intelligence, abilities, skills, attitudes, interests, and so on. All of these can be considered problems in the measurement of attributes, and yet there are some differences which led us to exclude them from our discussion.

We have discussed problems where there is little question about the existence of a psychological attribute. Our concern has been with how one would determine the scale properties of an attribute and then arrive at a set of numerals to assign to objects which elicit the attribute to a greater or lesser extent. Furthermore, we have restricted ourselves to those cases where the attribute can be defined as a single continuum.

We have not discussed measurement of such things as intelligence and ability, because these are usually defined in terms of multiple tasks where the measure makes no pretense of having scale properties assignable to an attribute. For example, a final examination in a course in psychology will have many items, and the final score will be the sum of the items correct. But all items do not measure the same attribute, and in fact most examinations are constructed to ensure that they do not, by deliberately including items that are uncorrelated. Such tests make the problems of measurement no less difficult and intriguing (see Gulliksen, 1950), but they are simply of a different kind.

On the other hand, many highly sophisticated measurement techniques are concerned primarily with the establishment of the existence of underlying continua, or attributes. Factor analysis as a technique, for example, is less concerned with the measurement properties of an attribute than in discovering how many attributes are exhibited in a given number of persons and tests. The Lazarsfeld technique of *latent structure analysis* has been omitted for this reason also (perhaps incorrectly); it is more concerned with establishing the existence of attributes than with measuring their amounts (see Lazarsfeld's Chap. 10 in Stouffer et al., 1950; Torgerson, 1958).

THE RELATION OF RESPONSE TO ATTRIBUTE

We have discussed several techniques of psychological scaling—techniques which are quite different in procedure, but which differ also in the basic philosophy underlying them. Does it make no difference which technique we choose to use in scaling a particular attribute? Some psychologists would argue that any technique can lead to a scale which is valid, as long as we state what the particular operations were which led to it. But such a position is essentially defeatist, since it refuses to face the problem of the nature of the attribute itself, and the relation of the obtained scale to the attribute.

In particular, the difference in underlying philosophy between the manifest and the latent techniques is fundamental. The manifest techniques rely on the ability of a subject to describe his experience of an attribute using numbers with the proper measurement properties. The latent techniques make no such assumption, but on the contrary assume only that there is an attribute which affects the responses, although not in a simple one-to-one fashion.

Actually, there are many theoretical positions which have been taken on the relation of the manifest response to the attribute, and in this section we will discuss some of these positions. While such a discussion need not necessarily be part of a chapter on psychological scaling, the choice of technique cannot but depend on the assumptions which the experimenter makes concerning the relation of response to attribute. So, in a sense, these assumptions are part and parcel of the scaling problem.

To classify various theoretical positions, we will use Fig. 1-7 as a framework to discuss the problem. We can consider four processes as critical for scaling an attribute. First, of course, there are the objects with which the attribute is presumed to correspond, but the attribute is not identical with the object. The object will have many properties, and even if it has a closely related physical dimension as one of its properties, there is no necessary reason why the scale values of the attribute should correspond to those of the physical dimension.

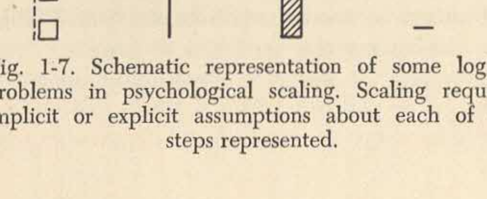


Fig. 1-7. Schematic representation of some logical problems in psychological scaling. Scaling requires implicit or explicit assumptions about each of the steps represented.

Second, we have the attribute itself. It exists as an abstraction from the object, but an abstraction which is directly experienced in most cases by a human subject or observer. It is the scale values of this attribute which we seek in psychological scaling, and as we have mentioned before, one of our major problems is to determine what measurement properties exist in this attribute. To ask this question is quite different from asking how the attribute is related to a physical dimension, or to the responses, because we want to know about the attribute itself, and we cannot determine whether, for example, the attribute has a true or absolute zero by showing that it is related to a physical dimension which has an absolute zero.

Third, in Fig. 1-7, we have a mediating process—some process which intervenes between the attribute and the response which reflects it. We have used the term "mediating process" as a general one, and in specific cases we might choose to call it an intervening variable, or hypothetical construct. But whatever it is called, we must consider the possibility of its operation. And more importantly, we must consider the possibility that it adds its own characteristics in relating the response to the attribute. In other words, if there is a mediating process, and we want to find out what the nature of the attribute is, then we will have to learn something about the nature of the mediating process as well.

Last, there is the response process itself, but by no means is this the least of our problems. If a response must be used to indicate the properties of the attribute, then we must be concerned about the properties of the response process itself. To illustrate, if the attribute has interval properties, then the response must also have at least these properties in order to have it correctly indicate the properties of the attribute. And it is not a foregone conclusion that the properties of numbers, as used by a human subject, necessarily have the ratio properties of the scale of number.

As we have mentioned before, nearly all psychologists seem to agree that responses can at least indicate ordinal properties of an attribute;

that whatever distortions might be introduced by a mediating process or by peculiarities of the response process itself will be distortions of interval and ratio properties, not of the ordinal properties. Thus most theoretical positions have been primarily concerned with the interval and ratio properties of the attribute as evinced by the responses.

RESPONSE AS DIRECT INDICATOR OF ATTRIBUTE

The simplest assumption which can be made about the relation between response and attribute is that the response directly and correctly indicates properties of the attribute. If a mediating process is assumed to exist it is also assumed to be bypassed or short-circuited by the response process. To say that this assumption is the simplest is not to say that it is the least complicated, because it is one of the most difficult to prove. Consequently, its validity is ordinarily of the face variety, i.e., it is valid by experimenter's fiat.

In recent years Stevens (1957)—and some of his coworkers—has been the major proponent of this position, although he has been concerned almost exclusively with psychophysical attributes, i.e., those sensory attributes for which there is a reasonable equivalent physical continuum such as brightness, loudness, etc. He has specifically argued for the use of manifest-ratio scaling techniques, and while in principle he accepts any of these methods as valid, his own recent work (Stevens, 1959) suggests a strong preference for the method of magnitude estimation.

This primary emphasis on a specific technique leaves some open questions, since we would expect that all techniques of the same logical class should be equally capable of leading to the same psychological scale. In other words, there should at least be generality with regard to technique.

Even further, as we pointed out earlier, a ratio scale should correctly predict interval properties; and if we accept the validity of manifest-ratio techniques, then we should also expect the equivalent manifest-interval techniques to provide the same psychological scale within the limits of the technique. But the manifest-interval techniques do not provide the required cross check.

The reason for this difficulty may lie in the problem of the nature and properties of the response process itself, since it cannot accurately indicate properties of an attribute unless it is used accurately, with properties at least as powerful as those of the attribute. We saw in Fig. 1-3 that Torgerson's data showed an inverse relation between scales of darkness and scales of lightness when numerical category scaling was used. Torgerson also obtained judgments with the method of magnitude estimation. For both darkness and lightness he obtained a scale quite different from that obtained with numerical category scaling, but the logarithm of the magnitude estimation scale was linearly related to the numerical category scale. Furthermore, the scales of lightness and of darkness obtained with magnitude estimations were related in a reciprocal rather than an inverse fashion. These results of Torgerson make clear that human subjects do not use numerical scales in the same way when they assign numerical categories rather than magnitude estimations. Furthermore, if we accept the results from each method on its own merits, we would have to conclude that the attribute of darkness is the inverse of the attribute of lightness in one case, while it is the reciprocal in the other.

These results make it clear that the validity of the required assumptions for manifest-ratio or for manifest-interval techniques are in considerable doubt. The minimum checks on the validity of the assumptions do not provide favorable results, and we can only conclude that it is not self-evident that the manifest techniques are valid.

RESPONSE AS INDICATOR OF MEDIATING PROCESS

Most modern psychologists assume that the response is an indicator of some intervening variable or mediating process. Some, however, assume that the response is a relatively direct indicator of the intermediate process, and others that the response may be quite indirectly related. We shall discuss this first class of theories now, and the others later.

Correlated Process. One of the most straightforward theories that the response is related to a mediating process states that the response is directly related to some process which is itself correlated with the attribute. There are many instances of this point of view in the literature on scaling of skills, attitudes, etc. For example, the halo effect is a description of the tendency to rate persons on one attribute according to their previous ratings on another attribute. De Soto (1961) has emphasized this tendency of people to rate other people or objects in a consistent simple ordering.

More specifically, again in the realm of psychophysical continua, Warren (1958) developed the *physical correlate* theory of judgment. Basically, he argues that the responses made by subjects do not indicate the properties of the attribute itself, but reflect instead the properties of a correlated physical dimension, with which the subject has had experience in using numbers.

He argues, for example, that where subjects have had direct experience in using a numerical scale, their manifest-ratio responses will be linearly related to the underlying physical continuum. Thus judged heaviness will be directly related to the physical scale of weight, because we all have experienced pounds and ounces. He presents evidence to show that this is so when the contaminating factor of size is ruled out. In like manner, judgments of sweetness will be directly related to physical concentration of the sweetener, since people have had experience with amounts of sugar. And judgments of distance or length will be directly related to the physical dimension for the same reason.

Most people have had no direct experience with the physical dimension of intensity, and thus cannot judge attributes such as brightness or loudness directly. But there is a physical dimension closely related to loudness and brightness with which people have had considerable experience, and that is distance. So Warren argues that a judgment of half-loudness or half-brightness is really a judgment of twice the equivalent distance, and presents data to show that judgments of brightness or loudness, and distance are related. In other words, a subject says that a given sound is half as loud as another, when its source seems twice as far away.

The evidence which Warren presents (see Warren, Sersen, and Pores, 1958, and Warren and Poulton, 1960) is convincing, and certainly suggests that subjects can *learn* to use numerals with ratio properties if they learn them along with a physical dimension which in fact has those properties. Whether they can ever transfer this skill to descriptions of an attribute with which they have had no direct numerical experience remains an open question.

Interacting Process. Another point of view toward the relation of response to attribute is that there is a mediating process which constantly interacts with the attribute, so that the response to any stimulus object is a joint function of the attribute and the mediating process. While Helson and Michels did not describe their theoretical positions in exactly this form, it is one way of considering their approach.

Helson (1948, see also 1959) originally formulated his *adaptation-level* theory with primary concern for determining what stimulus conditions led to a judgment of "neutral" with a verbal rating scale technique. He argued that the adaptation level at any given instant was a weighted geometric mean of all stimuli, past and present, and their effects on the attribute being judged. The adaptation level will be constantly changing as new stimulus objects are experienced. The neutral judgment, whether the verbal label on a rating scale, or the middle value on a numerical scale, will always correspond to this adaptation level.

But Helson further argued that all other judgments are made relative to this adaptation level. He was concerned not with just the neutral judgment, but with all verbal or numerical judgments over the entire length of the scale. In other words he stated a theory regarding the relation between the responses and the attribute. This relation was made mathematically much more explicit by Michels and Helson (1949), who derived a relation between responses and stimulus intensity which shows a formal correspondence to Fechner's law, but with this important difference: Fechner assumed that the origin (in the mathematical sense) of the sensory attribute was the absolute threshold, while Michels and Helson argue that the origin is the adaptation level. More specifically, they argued that the first response category (on a categorical scale) corresponded to a stimulus magnitude 1-kth below the adaptation level (where k is the number of judgment categories below neutral), and that all other responses were adjusted to satisfy this requirement.

Michels (1954) carried this development further by interpreting fractionation judgments of brightness along the same lines, and later (Michels and Doser, 1955) by doing the same for loudness judgments. This is not the place to go into the exact mathematical formulation which Michels used, but the point of view expressed by Helson and by Michels about the relation between the response and the attribute is an important one.

It should be remembered that the adaptation level is constantly changing, and that it will be different for every set of experimental conditions.

Thus to argue that the response is always relative to the adaptation level is to say that there is no invariant relation between response and stimulus object, or the underlying physical continuum. We can express this position either by arguing that the attribute scale is not stable, or by stating that the adaptation level is a mediating process which constantly interacts with the attribute (or possibly with the response continuum) so that we can never experimentally obtain an invariant relation between stimulus and response.

Certainly this relativistic position with regard to psychological scaling has considerable experimental backing, since most experiments on scaling do not show an invariant relation between response and the underlying physical dimension. Garner (1954*b*), for example, has shown that fractionation judgments obtained with the method of constant stimuli were almost completely dependent on the range of the comparison stimuli presented, i.e., on the context of the stimuli presented for judgment. It could still be possible, however, that there is a meaningful stable attribute, and that our problem is to determine what its properties are by attempting to carry out experimental operations which can take these response and judgment characteristics into account.

RESPONSE AS AN INDIRECT INDICATOR OF ATTRIBUTE The third major position taken concerning the relation of response to attribute is that the response does not directly reflect properties of the attribute at all, even through a mediating process, but rather that the response is only indirectly related to the attribute. Thus we do not assume that the numerical properties of the response indicate the numerical properties of the attribute.

Essentially all latent techniques make this assumption. With normalized pair comparisons, for example, the subjects are not even required to make more than an ordinal judgment. And with normalized category ranks, even if the subjects actually used an interval response, this property is not assumed to hold when the scale values are determined. For example, subjects can use the numerical category scaling method, but the experimenter then assumes only the ordinal property in constructing his scale.

Even the technique which Garner (1954*a*) used to construct a loudness scale from equisection and fractionation judgments dropped the assumption that the stated numerical ratio for the fractionation settings was the true ratio.

Clearly, if the numerical properties of the responses are not considered adequate to reflect accurately the properties of the attribute, some assumptions must be made in order to get numerical properties back into the scale, and herein lies the crux of the problem with these indirect and latent techniques. When such assumptions are made, they must be validated before we can reasonably accept the psychological scale as meaningful. With a technique such as normalized ranks, no means is provided for checking on the validity of the normal assumption; but with most of the other techniques, such procedures are available. With normalized pair comparisons, for example, the ability to use the average scale to check back on each individual scale makes it possible to show when the normal assumption does not apply.

In a sense, these techniques aim to provide scale values for an attribute which is really a hypothetical construct. It may or may not "exist," but its assumed existence, with the stated properties, often allows one to integrate a large amount of data.

A SUMMARY EVALUATION

We have been unable to avoid evaluative comments both about the techniques we have discussed and about the various theoretical positions regarding the relation between response and attribute. It might be well, however, by way of a summary, to state explicitly what we consider to be the criteria by which techniques should be evaluated, and some opinions about the techniques.

A psychological scale, and even the attribute itself, is a concept, one which the experimenter uses because it provides meaning and generality, and it allows a larger body of data or facts to be integrated into fewer working principles. The whole role of such concepts in a science revolves around the idea of generality. Science does not search simply for data, or even for facts. Rather, it searches for facts which have some degree of generality, so that we do not have as many facts as there are possible events in the world. Generality can exist for many different kinds of things, and here is where we must look in evaluating the techniques: In how many different ways do the scales provide generality? While there are many different things over which we could expect generality, there are several which are of primary importance in evaluating scaling techniques.

Time. At a minimum a psychological scale must be invariant across time, but we have little problem in this regard, since most techniques which have survived have shown reasonable invariance when the experiment is simply repeated at another time.

Subjects. Once again, there is little difference between the techniques in this regard. Most of them make provision for obtaining data from several different subjects, so that we can be reasonably assured of invariance across a specified population.

Objects. We would like to find psychological scales which hold for all the stimuli or objects which are presumed to contain the attribute. That is to say the scale should be invariant in its properties regardless of the particular stimulus objects used to determine the scale.

In this regard, the manifest techniques show a real weakness. In fact, this is the real essence of the Helson and Michels position, that under changed stimulus conditions the manifest response itself will change, even for the same stimuli. Thus the scale of loudness we obtain with the fractionation technique depends on the particular stimuli which the subject encounters (Michels and Doser, 1955, and Garner, 1954*b*).

On the other hand, Jones (1960) has shown that the method of successive intervals (normalized category ranks) is invariant even when different specific stimuli are used in establishing the scale. He compared this method to manifest-interval techniques, but his conclusion might well be extended to other manifest techniques. Jones further showed invariance of the latent technique with changes in the actual response continuum used.

Method. In an earlier section we remarked that generality should be established with regard to responses, but in a broader sense we need to require generality with regard to method. A particular method usually specifies a class of responses, and while we can change the actual numerical values within the same basic method, we are more interested in generality across classes of responses rather than just across different possible numerical values.

Of greatest concern is that a psychological scale should be invariant with regard to methods which are logically equivalent, or where one method implies the possibility of another. If subjects can make ratio judgments, then they should also be able to make interval judgments with regard to the same attribute. But as we have seen we do not obtain the same psychological scale when these different techniques are used. It is in fact this very difficulty which has led many experimenters to search for the one "correct" technique. But a scale which is unique to a particular method is a concept so narrowly defined that it can have little general utility.

Garner (1958) has specifically argued for the use of a latent scale of loudness based on the discriminability criterion for exactly these reasons, since the criterion involved in most of the latent techniques has much greater generality from one method to another. Jones (1960) has supported this argument for stimulus materials which are not of the psychophysical variety. Actually, psychological scales based on latent properties of data show more correspondence to scales based on manifest-interval techniques than these do to scales based on manifest-ratio techniques.

Thus the available evidence suggests that the latent scales have greater generality than do the manifest scales, particularly those manifest scales which assume the ability of subjects to use ratio properties. Perhaps part of the reason lies in a remark we made earlier to the effect that the latent techniques require special assumptions; but the requirement of these assumptions is sufficiently obvious that most experimenters go to great pains to establish their validity.

In the ideal case, then, a scale should have generality over time, subjects, objects which elicit the attribute, and method. As we have suggested, the last, the problem of generality over different methods, is at present the greatest stumbling block to the development of psychological scaling. When we pointed out that the latent procedures seem to provide somewhat greater generality than the manifest techniques, we dodged a very real problem. We were unwilling to say that the techniques were more successful because they reflected actual processes inside people; that they really reflected the metric properties of attributes. We were unwilling to say this because we do not know whether the statement is true or not.

Ideally, a scale which has the greatest generality is the one that has the greatest correspondence with the internal attributes and mediating processes. Such a scale could only grow from a theory about how people make judgments about their experience, when the theory is applied to the procedure used in constructing the scale. It would be unfair to state that such a theory does not exist; the normality assumption used in most of the latent scaling procedures is really the first step toward such a theory. Lately many theories have been advanced about different aspects of the judgment process. So far little work has been done toward applying these theories to problems of scale construction. Meanwhile, as we have seen, many useful procedures have been developed for the psychologist to use if he has need to measure psychological processes.

REFERENCES

Abelson, R. P. Scales derived by consideration of variance components in multi-way tables. In *Psychological Scaling* (Gulliksen, H. and Messick, S., eds.). New York: John Wiley & Sons, Inc., 1960.

Attneave, F. A method of graded dichotomies for the scaling of judgments. *Psychol. Rev.*, 1949, **56**, 334-340.

Coombs, C. H. Psychological scaling without a unit of measurement. *Psychol. Rev.*, 1950, **57**, 145-158.

De Soto, C. B. The predilection for single orderings. *J. abnorm. soc. Psychol.*, 1961, **62**, 16-23.

Garner, W. R. A technique and a scale for loudness measurement. *J. acoust. Soc. Amer.*, 1954*a*, **26**, 73-88.

———. Context effects and the validity of loudness scales. *J. exp. Psychol.*, 1954*b*, **48**, 218-224.

———. Advantages of the discriminability criterion for a loudness scale. *J. acoust. Soc. Amer.*, 1958, **30**, 1005-1012.

——— and Hake, H. W. The amount of information in absolute judgments. *Psychol. Rev.*, 1951, **58**, 446-459.

———, ———, and Eriksen, C. W. Operationism and the concept of perception. *Psychol. Rev.*, 1956, **63**, 149-159.

Green, B. F. Attitude measurement. In *Handbook of Social Psychology* (G. Lindzey, ed.). Reading, Mass.: Addison-Wesley Publishing Company, 1954.

Guilford, J. P. The computation of psychological values from judgments in absolute categories. *J. exp. Psychol.*, 1938, **22**, 34-42.

———. *Psychometric Methods.* 2d ed. New York: McGraw-Hill Book Company, Inc., 1954.

Gulliksen, H. *Theory of Mental Tests.* New York: John Wiley & Sons, Inc., 1950.

Guttman, L. *Measurement and Prediction.* (Stouffer, S. A. et al.) Chaps. 2, 3, 6, 8, and 9. Princeton, N. J.: Princeton University Press, 1950.

Helson, H. Adaptation-level as a basis for a quantitative theory of frames of reference. *Psychol. Rev.*, 1948, **55**, 297-313.

———. Adaptation-Level Theory. In Psychology: *A Study of a Science*. Vol. I. (Koch, S., ed.). New York: McGraw-Hill Book Company, Inc., 1959.

Jones, L. V. Some invariant findings under the method of successive intervals. In *Psychological Scaling* (Gulliksen, H. and Messick, S., eds.). New York: John Wiley & Sons, Inc., 1960.

Kuffler, S. W., FitzHugh, R., and Barlow, H. B. Maintained activity in the cat's retina in light and darkness. *J. gen. Physiol.*, 1957, **40**, 683-702.

Metfessel, M. A proposal for quantitative reporting of comparative judgments. *J. Psychol.*, 1947, **24**, 229-235.

Michels, W. C. An interpretation of the bril scale of subjective brightness. *J. opt. Soc. Amer.*, 1954, **44**, 70-74.

——— and Doser, B. T. Rating scale method for comparative loudness measurements. *J. acoust. Soc. Amer.*, 1955, **27**, 1173-1180.

——— and Helson, H. A reformulation of the Fechner law in terms of adaptation-level applied to rating-scale data. *Amer. J. Psychol.*, 1949, **62**, 355-368.

Saffir, M. A. A comparative study of scales constructed by three psychophysical methods. *Psychometrika*, 1937, **2**, 179-198.

Stevens, S. S. Mathematics, measurement, and psychophysics. In *Handbook of Experimental Psychology* (S. S. Stevens, ed.). New York: John Wiley & Sons, Inc., 1951.

———. On the psychophysical law. *Psychol. Rev.*, 1957, **64**, 153-181.

———. Cross-modality validation of subjective scales for loudness, vibration, and electric shock. *J. exp. Psychol.*, 1959, **57**, 201-209.

——— and Galanter, E. H. Ratio scales and category scales for a dozen perceptual continua. *J. exp. Psychol.*, 1957, **54**, 377-411.

Stouffer, S. A., Guttman, L., Suchman, E. A., Lazarsfeld, P. F., Star, S. A., and Clausen, J. A. *Measurement and Prediction.* Vol. IV. Princeton, N. J.: Princeton University Press, 1950.

Thurstone, L. L. A law of comparative judgment. *Psychol. Rev.*, 1927, **34**, 273-286.

Torgerson, W. S. *Theory and Methods of Scaling*. New York: John Wiley & Sons, Inc., 1958.

———. Quantitative judgment scales. In *Psychological Scaling* (Gulliksen, H. and Messick, S., eds.). New York: John Wiley & Sons, Inc., 1960.

WARREN, R. M. A basis for judgments of sensory intensity. *Amer. J. Psychol.*, 1958, **71**, 675-687.

——— AND POULTON, E. C. Basis for lightness-judgment of grays. *Amer. J. Psychol.*, 1960, **73**, 380-387.

———, SERSEN, E. A., AND PORES, E. B. A basis for loudness-judgments. *Amer. J. Psychol.*, 1958, **71**, 700-709.

CHAPTER 2

Physiological Psychology: Neuropsychology[1]

WM. A. WILSON, JR. AND MARTHA WILSON
University of Connecticut

INTRODUCTION

What Is Physiological Psychology? Physiological psychology is generally considered to be the study of the physiological and anatomical bases of experience and behavior. Thus English and English (1958) offer the definition: "the study of the correlations of physiological processes or activities with behavior or act." Consistent with such definitions, experiments are considered to be examples of work in this field if they involve, in addition to some behavioral procedure or measure, physiological manipulation or physiological measurement of the subject. Physiological manipulation can be relatively peripheral (intravenous injection or sensory-nerve stimulation) or central (brain stimulation or destruction); physiological measurement can be central (recording of brain potentials) or peripheral (measurement of blood hormone levels). These examples demonstrate the width of the definition offered, but further reflection discloses its possible ambiguity.

Experiments on humans who have suffered accidental brain damage are generally included in the field of physiological psychology, although within the actual study performed by the psychologist, no physiological manipulation of the brain takes place. Studies of different strains of animals may be considered as part of this field, even when selective breeding is not part of the procedure of the experiment. On the other hand, a study of the effects of prior training upon present behavior is not generally considered physiological psychology, even if the experimenter believes that the training has affected the brain. These difficulties in determining the necessary characteristics of a "physiological manipulation" could be paralleled with problems in defining a "physiological measure."

A solution to the problem seems to be offered by considering the intent of the experimenter (or the reader)—an experiment is part of physiological psychology if the data are used to throw light upon a proposed correlation between physiology and behavior. This would appear to be an awkward conclusion, but consideration of it does turn our attention to theories, where proposals regarding the physiological correlates of behavior would be expected to reside most abundantly.

Physiological Theories of Behavior. A physiological theory of behavior is generally considered to be a set of statements which purports to describe the relationship between physiological and psychological variables, and in so doing, to provide an explanation of psychological facts. Under such a definition physiological theories are subject to the criticisms which have been leveled at explanatory theories in general (Skinner, 1950), and at reductionistic theories specifically (Jessor, 1958).

---

[1] We are grateful to Magda Arnold, Donald O. Hebb, and Karl H. Pribram for various forms of help. Mortimer Mishkin made many valuable suggestions; we are happy to acknowledge our debt to him.

Any consideration of such criticisms must start with the realization that it is in fact not easy to maintain a distinction between physiological and nonphysiological theories. Let us consider briefly several sorts of theories which might be offered to explain behavior. One theory may include the suggestion that a psychological process (e.g., learning) consists of a certain physiological process, which is known to occur under certain conditions. Another theory may state that the nature of learning requires that a mechanism exist with certain formal characteristics without identifying this mechanism with any known structure or process. Still another theory will provide merely a statement of the conditions for learning without stating explicitly the fact that a mechanism could be constructed (or, equivalently, a computer program written) which would "learn" only under such conditions. All of these theories might be equally successful (or unsuccessful) in predicting behavior. They will differ in the extent to which they explicitly attempt to identify construct with structure or physiological process, but no obvious cut-off points distinguish the physiological from the nonphysiological theory. We may add to these considerations the fact that some authors mention physiological correlates much as an afterthought to a nonphysiological discussion, and that many others cast their allegedly nonphysiological theories in molds actually suggested by physiologists of an earlier day, as Hebb (1949) has pointed out.

In fact, it can be argued that theories which are apparently nonphysiological are particularly likely to produce reductionistic explanations of behavior. The psychologist who sets forth a theory involving a behaviorally defined construct may incite others to search for a physiological equivalent for his construct, and some theorists, such as Deutsch (1960), while proposing a formal structural model, explicitly suggest that its physiological correlates are useful objects of search.

On the other hand, Hebb and Pribram, whose theories we shall consider below in some detail, despair of the possibility of establishing complete equivalence between physiological and psychological variables, and they feel that both kinds of variables are necessary for a complete description of the factors influencing behavior. Hebb states, for example: "Though we must get as much value from neurologizing as we can, it will never be possible to substitute neurology for psychology. . . . We must use larger-scale psychological conceptions as well as the smaller-scale neurological ones" (1958, p. 265). According to Pribram: "Different parts of the brain are shown to have different functions. . . . But these different functions are not to be the loci of some simple psychological process inferred directly from the observed behavior. Rather, the brain process is shown to be a critical determinant of the behavior under certain conditions" (1960*a*, p. 22).

Theories and Data. Even if we construe the phrase rather broadly, we find that there are not many physiological theories of behavior, if one seeks general theories such as those that have been developed from specific theories of learning or perception. It is not surprising to discover, therefore, that only a few physiological experiments currently are done as explicit tests of a global physiological theory (Davis, 1962). Most general theories have not provided many explicit predictions which were immediately testable, although they have in some cases provided the impetus for research of a given kind.

This deficiency of theories and of theory-stimulated experiments may be somewhat misleading, however. Throughout the history of physiological psychology, there have been identifiable questions which have served to organize research efforts. These questions have usually been posed as brain-behavior correlational problems, and they usually have been related to one or another rather general physiological theory of behavior. We shall discuss some of these issues after surveying some basic physiological and anatomical "facts," the present state of some fields of neuropsychology, and some of the broader present-day views of physiological psychology.

BASIC NEUROPHYSIOLOGY AND ANATOMY

It is to the nervous system that we must turn if we wish to study the question of the correlation between behavior, on the one hand, and physiological activity and anatomical structure, on the other. Of course, many other aspects of bodily activity may affect behavior (Beach, 1958), but the nervous system is predominantly important if any system of the organism is to be identified as providing the structural basis of behavior.

Anatomy and Physiology of the Neuron. The basic unit of the nervous system is the single nerve cell, or *neuron*, a cell which has been specialized for integration and conduction of stimulation. Structurally, the typical neuron may be divided into a *cell body* and its processes (see Fig. 2-1). Conventional descriptions state that *dendrites* are relatively short processes which conduct excitation toward the cell body, and that the *axon* is a single long process, often branched, which leads excitation away from the cell body. With the realization that the cell body and its internal structures are concerned primarily with metabolic maintenance of the neuron, it has been suggested (Bodian, 1962) that the position of the cell body should not be taken as a focal point for a structural definition of the parts of the cell. According to this view, the *dendritic zone* is the receptor surface of the cell; this may include the surface membrane of the cell body, or tapering extensions of the cell body (the dendrites) or even parts of the axon. The dendritic zone is influenced, either by reception of activity of other nerve cells, or by excitation from environmental stimuli, to initiate an electrochemical change and under certain conditions, to generate a response in the axon. The axon is generally a long single process, which transmits impulses away from the dendritic zone. The axon ends by branching into a group of terminals which usually comes into close proximity with other nerve cells, or muscle or glandular cells, and affects the activity of those cells by electrical or chemical means. The *perikaryon* or cell body, while essential to the life of the neuron, is probably not related specifically to the functions discussed above; the cell body may be in the dendritic zone or next to or within the axon.

The neurons make up only 50 per cent of the mass of nervous tissue. Much more numerous are the *glial* cells, which are found in close contact with all the exposed surfaces of the neurons. The axon, itself, is covered by elongated glial cells, the so-called Schwann cells, the coiled membranes of which form the myelin sheath which surrounds the axons of many neurons (Robertson, 1962). The intimate relationship between some of the

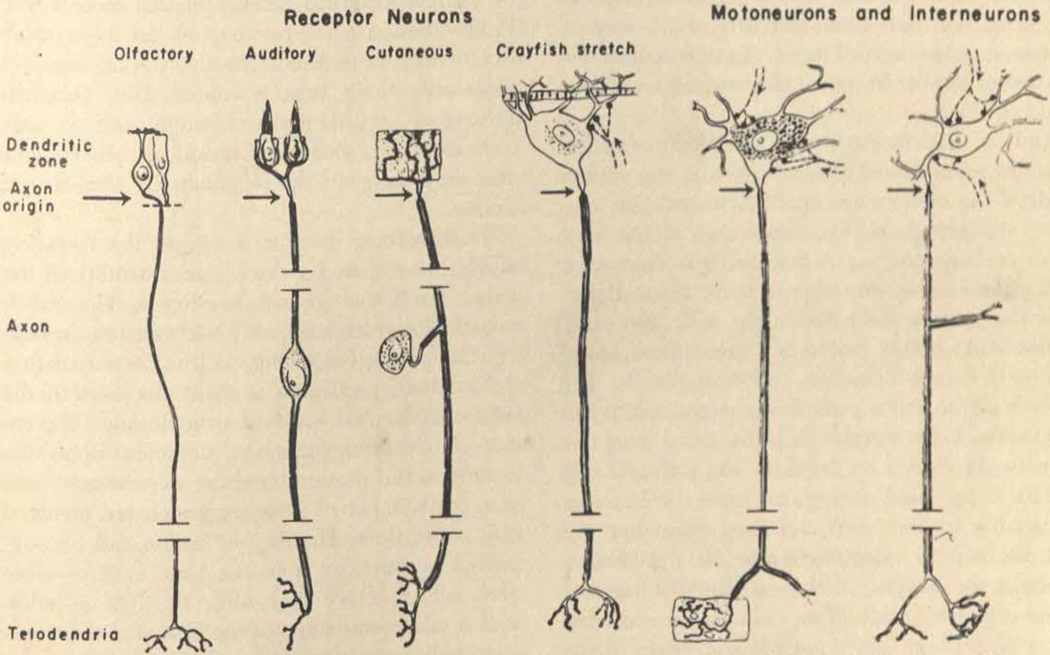


Fig. 2-1. Neurons from different parts of the nervous system. The cell body is found at different locations relative to the axon in the various types of cell. Synaptic connections are represented by the dotted lines ending on the cell bodies and dendrites of the neurons on the right. (From Bodian, 1962, p. 325)

glial cells and the capillaries which bring blood to the brain has led to the suggestion that the glia serve to control the passage of substances from the blood into the neural cells. There is very little evidence from electrophysiology that the glia are directly involved in the conduction of impulses which characterizes neural activity.

Neurons are linked together in such a fashion that the excitation in one cell can effect (and affect) excitation in other cells. What is the nature of this excitation *within* a cell, and how is it transmitted between cells? The most striking event is the spike potential or nerve impulse, the electrically measurable activity of the axon, which moves along the axon rapidly as a result of electrochemical activity of the axon membrane. This activity is properly characterized as "all-or-none" in that the size of the response is not related to the intensity of the stimulus which arouses it, if this stimulus is strong enough to produce any response at all. Since this activity depends upon energy sources which are available all along the length of the axon, the impulse travels without decrement, being equally strong at all points along the length of the axon.

The situation is different in the dendritic zone, however. Here the responses are graded, depending upon the nature and intensity of the stimuli. These responses spread locally, but decrementally, becoming smaller in size as they spread from their point of origin.

Only a short description of the electrochemical changes which underlie conduction in the various parts of the neuron, and conduction between neurons, will be given. The membrane of the axon in its resting state is not permeable to Na^+ ions, and a greater concentration of these is found outside the neuron than inside the cell. The equilibrium that results produces a greater concentration of K^+ ions inside the cell than outside. The outside of the cell is positively charged relative to the inside. If the membrane is disturbed (and one way to do this is to decrease the difference in electrical potential at a given point) it becomes permeable to Na^+ ions and they move into the cell. K^+ and Cl^- ions move also; the net effect is to make the outside of the cell slightly negative relative to the inside. This shift in the electrical characteristics at one point has the effect of depolarizing (i.e., eliminating the difference in potential across the membrane) at the adjacent point of the cell. The membrane become permeable at the next point, and the process continues. In such a fashion, the excitation sweeps along the length of the cell membrane from the point of origin, destroying the ionic imbalance at each point. The impulse may be measured as the change in the electrical characteristics at a point, from the resting state to the state during excitation. This change is equivalent at all points (since the ionic distribution across the membrane is equivalent at all points) so that the size of the impulse is the same and conduction is without decrement.

Synaptic Transmission. The foregoing description applies principally to conduction along the axon, but we must still consider how excitation passes from one cell to another, and how conduction takes place within the dendrites and cell body.

The axon terminates in a group of smaller processes, the axon *telodendria,* which themselves terminate very close to the surface membrane of another neuron (or an effector structure). The cleft, approximately 200 Å in width, between two nerve cells, is called a *synapse.* The prevalent present-day view is that the electrical activity of the axon causes the release into the synapse of chemicals which affect the membrane of the next neuron, and initiate electrical activity in this second cell. Within the terminal portions of an axon small vesicles may be seen with the electron microscope; presumably these vesicles contain the "chemical messengers" which transmit from cell to cell. Acetlycholine is one such chemical; others have been suggested but the evidence for them is not as clear.

The excitatory substance affects the receiving cell by causing an increase in permeability of the nearest portion of its cell membrane. The imbalance of ions which has been maintained in the resting state of the (receiving) cell is decreased to a slight extent, producing a slight decrease in the electrical potential across the membrane. The extent of this depolarization depends upon the amount of the transmitter substance released and upon the number of synapses which are involved at any one time. The depolarization can be augmented by impulses from the same axon or other axons which arrive soon after the first impulse, since it takes some time for the effects of the transmitter substance released by the first impulse to be dissipated.

The situation is complicated further by the fact that not all impulses are excitatory, i.e., impulses in some axons tend to *inhibit* activity in the cells with which they have synaptic connections. These axons apparently produce a transmitter substance which causes hyperpolarization rather than depolarization of the postsynaptic membrane. Thus, the resting cell will inaugurate nerve impulses only if the excitatory influences acting upon it at one time are sufficiently greater than the inhibitory influences.

If depolarization is sufficient at any time to reach the threshold for activation of the initial portion of the axon, then a propagated nerve impulse in the axon will be started. An impulse may also be inaugurated in certain portions of the dendritic zone; these impulses conduct with decrement over the soma-dendritic membrane and may contribute to the inauguration of the axon response —the nerve impulse proper.

Organization of the Central Nervous System. A brief description is given here of the major structures in the brain in order to provide an orientation to the discussion which follows. The way in which the nervous system is described anatomically is not devoid of theoretical implications. If we seek to determine the function of the structures of the brain, the way in which we delimit those structures will affect our conclusions. Furthermore, categorizing certain structures as belonging to the same system clearly implies a belief in a common function. We shall give as purely geographical a description of the major structures as we can (see Fig. 2-2).

On the basis of embryological considerations, the brain can be divided into three major parts: the hind brain (*myelencephalon* and *metencephalon*), the midbrain (*mesencephalon*), and the forebrain (*diencephalon* and *telencephalon*). The *myelencephalon* or medulla oblongata may be considered as almost a continuation of the spinal cord. Through it course tracts of fibers, ascending and descending. Several of the cranial nerves enter the central nervous system at this level and many essential activities of the body such as respiration are partially controlled by "centers" here.

The *metencephalon* includes the *cerebellum.* This structure has usually been assigned a part in the control of motor activity because of its spinal connections, its relationship to the motor areas in the forebrain, and some of the effects resulting from damage to it. It must be noted that it does

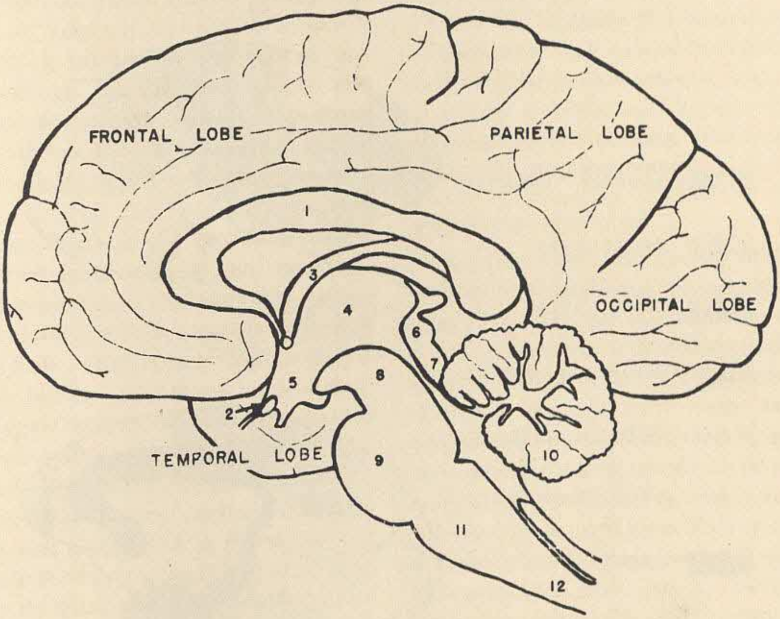


Fig. 2-2. Medial view of the human brain. (1) corpus callosum, (2) optic chiasm, (3) fornix, (4) thalamus, (5) hypothalamus, (6) superior colliculus, (7) inferior colliculus, (8) midbrain, (9) pons, (10) cerebellum, (11) medulla oblongata, and (12) spinal cord. (Based on Plate I of W. J. S. Krieg, *Brain Mechanisms in Diachrome.* 2d ed. Evanston, Ill.: Brain Books, 1957.)

receive afferent input from various receptors as well.

The midbrain, or *mesencephalon,* includes the *superior* and *inferior colliculi,* areas related to vision and audition, respectively. Ventral to this is the major portion of the *brain-stem reticular formation,* a collection of cell bodies and short axons which constitute part of the "arousal" system. These cells can be stimulated by many other parts of the central nervous system, and activity in this area causes widespread changes in the rest of the brain and spinal cord.

The *thalamus* is a prominent structure in the *diencephalon* of the forebrain. Microscopic inspection of this area aids in the distinction of various subdivisions of cell masses (called nuclei), most of which are named in accordance with their spatial positions. The investigation of the function of the various nuclei is intimately linked with studies of their afferent and efferent connections, which will be discussed below. The other major structure of the diencephalic region is the *hypothalamus.* Its many subdivisions are important in the control of motivated behavior and autonomic function.

The cerebral hemispheres of the *telencephalon* are a prominent part of the brain as it is viewed in a typical mammal. These structures are made up of external cell layers and many fiber tracts which connect the cells with the rest of the nervous system. Among the fiber tracts are the corpus callosum and other commissures which connect the two hemispheres with each other. The major divisions of the hemispheres are the *frontal, parietal, occipital* and *temporal* lobes. It is convenient to delimit a fifth lobe, the *limbic* lobe, which comprises a group of structures found on the medial and basal surfaces of the hemisphere. These structures, including the cingulate cortex, hippocampus, amygdala, and frontotemporal cortex, are structurally different and phylogenetically older than the *neocortex,* or true cortex, of the lateral surfaces of the hemisphere (Pribram and Kruger, 1954). Some of these structures are intimately connected with the olfactory bulb, and at times, all of them have been described as rhinencephalic and have had olfactory functions attributed to them.

The *telencephalon* also includes some subcortical nuclei, the basal ganglia. Prominent are the caudate nucleus, the putamen, and the globus pallidus. They have long been considered to be

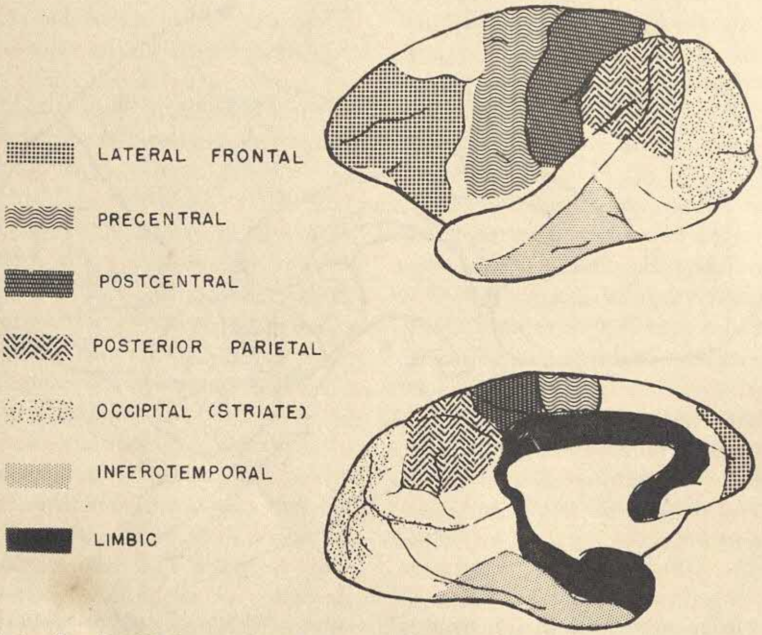


Fig. 2-3. Lateral and medial views of the brain of the rhesus monkey, showing the major areas of neocortex discussed in the text. The extent of the limbic cortex is also indicated. (Outline of cortical sulci based on Figs. 1 and 2 of G. von Bonin and P. Bailey, *The Neocortex of Macaca Mulatta.* Urbana, Ill.: University of Illinois Press, 1947.)

part of the motor system but more recent evidence suggests that a different description of their function is necessary (Rosvold, 1964).

Subdivisions of the Cerebral Cortex. Various methods have been used to provide divisions of the cerebral cortex finer than the gross portions delimited by separation into lobes (see Fig. 2-3). Thus, maps of the cortex may be based on electrophysiological data. Stimulating at the receptor level gives rise to evoked potential changes measurable at the cortical surface; such changes are found in a characteristic region for each sensory organ, and the areas thus delineated in the parietal, temporal, and occipital lobes have been described as the "primary projection areas" (Woolsey, 1958). Stimulation of the posterior portion of the frontal lobe gives rise to motor responses: areas thus described have been called "motor areas." If the parameters of stimulation, the nature of the recording instrument, and the condition of the animal are controlled, areas may be reliably defined. However, all the variables just listed are important, and it is now clear that the assignment of a name which seems to describe the function of an area is unjustified when with other conditions of stimulation a different area would be delimited. This conclusion applies particularly to the definition of "association areas," which were described as those regions which neither responded electrically to peripheral stimulation nor gave rise to motor responses.

Studies of the microscopic anatomy of regions of the cortex have led to systems which differentiate areas on the basis of the density of different kinds of cells and fibers in the various layers of the cortex. Several such systems have been advanced, and the reliability of the procedures upon which they are based has been severely criticized (Lashley and Clark, 1946; Clark, 1962). Some few distinctions can be reliably made, however, and many authors feel that major areas can be identified throughout the cortex.

The cortex can also be divided into areas on the basis of thalamic degeneration studies. Since destruction of the end of an axon in the cortex results in degeneration back along the nerve fiber to the cell body in the thalamus, it is possible to trace the origin of projections to the cortex by examining the parts of the thalamus which have undergone *retrograde degeneration* after circumscribed cortical lesions.

Rose and Woolsey (1949) labeled as *extrinsic* those nuclei which were considered to be relay nuclei, receiving afferent input from the receptor organs, and sending projections directly to the primary projection areas of the cortex. *Intrinsic* nuclei, on the other hand, were those with no apparent extrathalamic input, but with projections to the cortex, presumably the association cortex. Pribram (1958) suggested that the same names (extrinsic and intrinsic) be employed to describe the cortical areas thus defined. Nuclei which serve as relays between the brain-stem reticular formation or the hypothalamus and the cortex were included among the extrinsic nuclei.

The names may still be retained as useful, but recent discoveries suggest a more complex picture of connections, particularly for the intrinsic nuclei (Diamond and Chow, 1962). At least in the cat, the posterior group, which are classified among the intrinsic nuclei, do appear to have extrathalamic input. This input comes from the receptors, over small, slow-conducting fibers which follow a path different from that of the low threshold, phylogenetically newer fibers which carry input to the extrinsic nuclei. These intrinsic nuclei apparently project to what are called secondary projection areas, which may overlap two or more sensory spheres.

Table 2-1 outlines the various nuclei of the thalamus and their connections. This scheme includes Pribram's distinction of the gross anatomical groups of an external portion and an internal core; the reticular nucleus, which forms an outer shell, is added to the picture.

NEOCORTICAL FUNCTION

The effect of an impulse originated by a stimulus may be widespread throughout the nervous system. To a certain extent, however, all patterns of activity originating with stimulation of one sensory organ (the retina, for example) follow a common general path which may be distinguished from the path followed by impulses beginning with stimulation of other sensory systems. Investigators have looked to these modality-specific systems to explain the modality-specific aspects of experience and behavior. As we have noted above, these systems include areas of the cerebral cortex which are called *primary projection areas.* Destruction of these cortical areas can cause deficits in behavior which is under the control of stimuli presented to the corresponding sensory system. But there are other areas of the cortex which also appear to be important to modality-specific behavior. These are portions of what has classically been called the *association cortex* (Flechsig, in Von Bonin, 1960). The visual primary projection area in the monkey, located in the occipital lobes, and an association area in the temporal lobes, known as the inferotemporal cortex, both appear to be essential to normal utilization of visual information. With this basic result in hand, the attention of investigators has tended to turn away from questions of physiological psychology, such as "what is the locus of visual perception?" to questions of psychological physiology, such as "what is the function of the occipital area?"

The fact that a certain form of behavior is altered after ablation (or other physiological manipulation) does not of itself prove that the function and the structure are specifically correlated. Control studies involving other behavioral measures and other structures must provide us with the pattern of "double dissociation" (Teuber, 1955) before any specificity may be claimed. Furthermore, even if destruction of the structure produces loss of a specific function, it cannot be claimed that the function is "localized" completely in that structure. Nevertheless, in the following pages, we will make use of the expression "the function of an area" to indicate that the criterion of specific correlation has been met, and that this area is critical for the function described.

Names of behavioral tasks usually imply that there is a single dimension of behavior which is being assessed, and that differences in performance are due solely to differences between individuals on the indicated attribute. Occasionally, attempts to explain lesion effects in terms of other factors in the situation have gained the status of alternative theories of the nature of the changes in behavior. More often, such claims have been dismissed on the basis of control experiments, and we have not attempted to describe all of the pertinent experiments. We shall merely note here

TABLE 2-1

Organization and relationships of the thalamus

Input	Thalamic Nuclei	Output
	Internal Core	
	Extrinsic	
Reticular formation Hypothalamus	Central group (intralaminar, midline, etc.)	Limbic cortex
Hypothalamus	Anterior (dorsal, ventral, and medial)	Cingulate cortex
	Intrinsic	
Extrinsic thalamic nuclei Amygdala	Medialis dorsalis	Frontal cortex
	External Portion	
	Extrinsic	
Optic and acoustic pathways	Geniculates (medial and lateral)	Superior temporal cortex Occipital cortex
Cerebellum Somesthetic pathways of medial lemniscus	Ventral nuclei (anterior, lateral, and posterior)	Dorsal frontal cortex Pre- and postcentral cortex
	Intrinsic	
Extrinsic thalamic nuclei Sensory pathways (slow fibers)	Posterior group (including pulvinar)	Posterior association cortex, including secondary projection areas
	Outer Shell	
Midbrain reticular formation Other thalamic nuclei Sensory pathways (slow fibers)	Reticular nucleus	Widely over cortex

that in assessing the relationship between various lesions and the resulting deficits, among the parameters which have been considered are: level of motivation (Pribram, 1962*a*); postoperative interval (Orbach, 1956); intertrial interval (Riopelle and Churukian, 1958); type of task (Meyer, 1958); and cue variables (French, 1962; Warren and Harlow, 1952). Possible species differences will be briefly discussed in a later section; throughout this survey of neocortical function, we shall draw primarily upon studies in which monkeys were used as subjects.

"Function" of the Occipital Cortex. Klüver (1942) showed that total removal of the primary visual cortex of the monkey produced an animal which was unable to respond appropriately to any feature of the visual world except the total amount of light energy entering the eyes. His work, and that of others (Settlage, 1939) appears to support the hypothesis, clearly indicated by early anatomical and physiological studies (Poliak, 1932; Talbot and Marshall, 1941), that there is a fairly close relationship between specific parts of the retina (which receive stimuli from specific parts of the visual field) and specific parts of the visual cortex. Destruction of a part of the primary visual cortex produces a defect of the corresponding portion of the visual field. Although there is apparently some residual function in such areas (Cowey, 1962; Weiskrantz and Cowey, 1963), on casual examination a monkey may appear to be completely blind in the affected region. The loss of so much function after a cortical lesion clearly indicates the importance of the area, but there are many questions to be answered.

Does the area have nonvisual function? Apparently so. Orbach (1955) studied monkeys with occipital lesions on a variety of nonvisual tasks without disclosing any deficit in behavior until he followed a paradigm that Lashley (1943) had used in a similar study in rats. The monkeys were taught to run a maze, and then were blinded so that no visual cues could be used. This caused a loss in the accuracy of performance, but the animals eventually were able to relearn the maze, using nonvisual cues. Then, destruction of the "visual" cortex was imposed, and the animals *again* showed an impairment in behavior. A note of caution must be expressed; the lesions in this study probably involved part of an area of cortex which is important in accurate utilization of somesthetic information; perhaps the animals had difficulty using the tactual or kinesthetic cues which remained to them after they were blind.

Yet another question remains. Does destruction of part of this area affect at all the utilization of information from other parts of the field? A definite answer is not yet available from animal *ablation* studies. Such studies provide no evidence that the occipital cortex has any more than a relay function in vision.

We must turn away from simple ablation studies in order to find evidence concerning the *nature* of the participation of the occipital cortex in visually guided behavior. In some experiments, attempts have been made to disrupt the organization of the occipital cortex without destroying the tissue. Gold strips have been placed on the surface and gold pins inserted through it, in order to short-circuit electrical currents (Lashley, Chow, and Semmes, 1951). Cuts have been made through the depth of the cortex, and mica strips inserted, to block current flow (Sperry, Miner, and Myers, 1955; Sperry and Miner, 1955). These procedures were employed in order to disrupt any electrical fields that might be involved in the perception of figures, and the cuts might also destroy corticocortical connections within the visual areas. All such procedures produced little, if any, loss in accurate response to visual stimuli, but it has been noted that it was never demonstrated that these procedures actually caused any disruption in electrical fields (Brazier, 1961, p. 224).

When abnormal neural activity of the cortex is *actively* imposed by chemical stimulation, abnormal behavior can be demonstrated. The implantation of alumina cream over the cortex, or its injection within the cortex, is followed by abnormal, epileptic patterns in the electroencephalograph (EEG) recorded from that area, although there may be little or no actual tissue destruction. Monkeys with such *irritative lesions* of visual occipital cortex are deficient in the learning of a visual discrimination but show no loss in retention of a discrimination learned preoperatively (Kraft, Obrist, and Pribram, 1960). Electrical stimulation of the occipital cortex also has no apparent effect upon the performance of visual discriminations previously acquired (Chow, 1961*c*).

A difference between ablation and irritative lesions is seen in the experiment of Morrell, Roberts, and Jasper (1956), who used the technique of conditioning the EEG. A light may act as a US (unconditioned stimulus) since it produces a certain pattern of change in the EEG. Another signal, even another visual signal, which does not originally have this effect, may act as a CS (conditioned stimulus) so that after sufficient pairings with the US it will elicit the pattern of EEG changes, the CR (conditioned response). With such a procedure it was found that an alumina cream lesion in occipital cortex interfered with conditioning of the EEG to visual CSs but that with subsequent removal of the abnormal area, the CRs were easily obtained.

Stimulation of the cortex may be employed as a CS or US in a behavioral conditioning situation and some of the data from investigations of this sort provide evidence relating to the normal activity of the occipital area. It has been demonstrated that electrical stimulation of the cortex may serve as a CS in eliciting an avoidance CR in cats (Doty, Rutledge, and Larsen, 1956), and that this response will generalize to stimulation of the corresponding symmetrical point in the other hemisphere (Doty and Rutledge, 1959). However, Grosser and Harrison (1960) showed that similar generalization did not take place between two points on the same side of the rat's visual cortex if they were separated enough (0.5 mm or more) to avoid stimulation of common cortical neurons.

Electrical recordings of gross activity of this area of the cortex while the animal is engaged in discrimination learning have not proved very informative. Chow (1961*b*) employed a system in which the visual stimuli were presented in a flickering light. As is common, photic driving (response of the cortex at the rate of the imposed flicker) was recorded while the flicker was on. With repeated presentations of the stimuli, the amount of photic driving decreased. This too is common, and nothing about the pattern of driving appeared to characterize a state of learning even when the rate of the flicker was made part of the discriminative stimuli in a conditional reaction procedure.

Clear evidence that the occipital cortex does more than relay the information from the retina is given in the experiments of Hubel and Wiesel (1959). Microelectrodes which record the activity of single cortical cells disclose that some cells are stimulated maximally by light from a long, narrow area of the visual field. A given cell may respond to a vertically oriented shape; another to a horizontal or oblique set of points. Apparently the occipital cortex (or the optic-geniculate-occipital system) does effect a recoding of visual form input.

The Inferotemporal Area. The inferotemporal cortex also appears to be essential to normal visual function as measured in visual discrimination problems. After bilateral destruction of this area, monkeys will respond at chance level on problems learned preoperatively and learn new problems at a rate much slower than normal (Mishkin, 1954; Mishkin and Pribram, 1954; Wilson, 1957). The deficit appears to be specific to visual tasks, since delayed-response problems and somesthetic discrimination tasks are apparently undisturbed. The visual deficit can be displayed with a wide variety of visual stimuli: color, form, size, etc. The deficit also can be shown in successive discrimination problems (go-no-go problems with only one stimulus presented at a time) as well as in simultaneous discriminations (Pribram and Mishkin, 1955).

It has been suggested that this defect in behavior is due to some distortion of the visual field. Pasik, Pasik, Battersby, and Bender (1958) base such a conclusion upon the fact that their animals showed poorer performance postoperatively than preoperatively on a form discrimination, but not on a brightness discrimination. However, their inferotemporals did show a loss on the brightness problem relative to the control animals (with lesions in parietal cortex) so we must conclude that the inferotemporal lesion had an effect on brightness discrimination, although not a severe one.

Such an explanation might predict that the changes in performance after inferotemporal lesions would be similar to changes in performance after occipital lesions, although differing perhaps in magnitude. This is, however, not the case. Wilson and Mishkin (1959) compared monkeys with inferotemporal lesions and monkeys with lesions of that part of the occipital cortex which receives input from the middle of the visual field. On some tasks which were designed as measures of "sensory" function, such as patterned-string problems, the occipital group showed the largest deficits. On "learning" problems, such as learning set formation, the inferotemporal group performed most poorly. It cannot be claimed that there is a one-to-one relationship between the lesions made and the "function" disrupted, but there certainly are qualitative differences between the effects of the two lesions.

Further analysis of the nature of the inferotemporal deficit has been pursued. It has been shown that if a discrimination is overlearned before operation, it may be retained perfectly after the lesion is imposed, while problems learned just to criterion are lost (Orbach and Fantz, 1958; Chow and Survis, 1958). Furthermore, it has been suggested that the inability of monkeys to perform a discrimination accurately after operation does not prove that a specific "memory trace" has been completely destroyed. Monkeys that learned a problem preoperatively performed at chance immediately after operation. However, after additional training on new visual problems, the performance on the old problems appeared to show some recovery (Chow, 1952).

Other techniques have been employed to study the importance of this area in visual discrimination. Chow (1961*c*) delivered electric shocks to the inferotemporal cortex, presenting learning or retention trials on a successive visual discrimination to the monkey in the period of abnormal local EEG activity which follows such shocks. When the electrical stimuli were presented bilaterally, animals performed at a chance level in a learning task or on retention trials: during normal EEG changes they responded normally.

With the alumina cream technique for producing abnormal electrical activity, a different pattern of results appears (Stamm and Pribram, 1961). Animals with abnormal EEGs as a result of this procedure were much slower than normals in learning a visual discrimination, but they appeared to show slight impairment, if any, in retention of a preoperatively acquired habit. It may be noted, however, that these animals had apparently been overtrained on the discrimination by the time that the abnormal activity appeared.

Recording from this area during the course of learning has also been attempted. Chow (1961*b*) found that on some trials of a successive discrimination, the EEG was temporarily reduced in amplitude. This transient decrease was found only during the middle part of the acquisition of the problems. Stamm (1962) has found a new electrical response which occurs during the same part of the training procedure. These results are provocative, but it may be that these changes indicate only a state of the animal which is characteristic of the period during which increased attention to the problem is required and that they do not reflect the learning process itself.

Occipital-Inferotemporal Interaction. The participation of the occipital visual area in visually guided behavior is easily understood since visual input from the eye is relayed to the cortical area by the appropriate extrinsic nucleus of the thalamus, the *lateral geniculate*. But how do we explain anatomically the role of the inferotemporal cortex?

It is not very surprising to find that several experiments suggest that interaction between the inferotemporal and occipital areas must be possible in order that there be normal visual behavior. While a unilateral lesion of inferotemporal cortex produces practically no loss of visual ability, a deficit as great as that following a bilateral lesion appears when the unilateral lesion is combined with destruction of the occipital cortex on the other side, if, in addition, the neural fibers which connect the remaining inferotemporal and occipital cortices are cut (Mishkin, 1962).

In various experiments, possible afferent channels into the inferotemporal cortex have been investigated. This cortex receives fibers from the *pulvinar*, an intrinsic thalamic nucleus, and also receives cortico-cortical fibers from the ipsilateral preoccipital areas which themselves get excitation from the occipital visual areas. Attempts to produce a deficit in visual behavior of monkeys by lesions of the pulvinar have been unsuccessful (e.g., Chow, 1954). Some studies have suggested that preoccipital lesions produce a deficit in visually guided behavior (Ades and Raab, 1949); however, such a deficit might be due to the destruction of this area *per se*, and not to the presumed interruption of a pathway from occipital to temporal cortex. Furthermore, not all experiments involving ablation of preoccipital cortex have shown a resulting deficit (e.g., Lashley, 1948). Indeed, Chow (1961*a*) attempted to produce lesions of the pulvinar and of the preoccipital cortex within the same animal; even this did not impair the retention of visual discriminations. However, the lesions were not complete and further experiments suggest that the important connections between inferotemporal cortex and ipsilateral occipital cortex are indeed by way of cortico-cortical connections. For when the inferotemporal cortex was undercut, thus isolating it from subcortical centers, no loss of discrimination was noted. However, when the temporal cortex was crosshatched (destroying cortico-cortical fibers), a failure to retain visual habits ensued.

Other experiments, involving study of "split-brain" animals, when taken in conjunction with the conclusion that cortico-cortical connections exist between occipital and inferotemporal cortex, lead to a rather complex but not unbelievable picture of the "locus" of visual learning. In the intact animal visual information from either eye reaches the occipital cortex of both sides of the brain; by cutting the *optic chiasm* one prevents the crossing-over of fibers and each eye can project only to the cortex of the same side. However, there are direct connections between the two sides of the cortex, through the forebrain *commissures.*

As long as the optic chiasm is intact, a discrimination learned with input restricted to one eye can be performed without error when vision is restricted to the other eye even though the commissures are cut. Apparently, the memory trace is laid down on both sides and thus is available to either eye. Furthermore, if the optic chiasm is sectioned, habits learned with one eye will "transfer" to the other eye if the commissures are still present.

However, if the chiasm and the commissures have been cut, one has an animal in which there is apparent independence of the habits mediated by the two hemispheres: there is no transfer between eyes, and conflicting habits can be learned with the two sides. Especially interesting are the results obtained when animals are retested, after the commissures have been sectioned, on problems originally learned and successfully transferred after optic chiasm division. It appears that in this situation color discriminations may be performed well with either eye but form discriminations are retained only when tested with the eye which originally learned the problem. It has been concluded (Downer, 1962) that form discriminations which are learned after optic chiasm section give rise to memory traces in the ipsilateral hemisphere only, but as long as the commissures are intact, the other eye has "access" to that memory. For color, if the corpus callosum is intact, a problem learned through one eye produces traces on both hemispheres.

It has occasionally been suggested (Pribram, 1960*b*) that the output of the inferotemporal cortex is more important than its input, and attention has been turned to the fact that fibers from the temporal cortex terminate in the superior colliculus. This subcortical nucleus receives direct input from the eye and has reciprocal relationships with the occipital cortex. On these grounds alone we would be interested in the effects of its destruction upon visual behavior. At present, however, the evidence is unclear; lesions here have been reported to cause visual deficits in the cat but not in the monkey (Blake, 1959; Rosvold, Mishkin, and Szwarcbart, 1958).

In summary, then, we have two cortical areas that are important to visual behavior. They have somewhat different functions as indicated by the effects of selective ablation, and of stimulation of the areas. However, positive evidence of the nature of these functions from recording during learning is sparse and rather disappointing. The occipital area receives visual input directly from the thalamus, and appears to relay the information (transformed in some fashion, perhaps) through the preoccipital cortex to the inferotemporal area. Its exact path thereafter is open to conjecture.

Parietal Lobe. The parietal lobe is implicated in somesthetic function in humans and lower animals, although the mechanisms which serve various kinds of functions are obscure. As in the visual areas of the brain, the somesthetic primary projection area can be distinguished from an intrinsic cortical area which is also involved in behavior of that modality, but the modality specificity of the parietal intrinsic cortex, and the anatomical connections between sensory areas and "association" areas, are less definitely established than in the case of vision.

Primary projection areas have been identified by electrophysiological methods in man (Penfield and Rasmussen, 1950), and in monkey and other mammalian forms (Woolsey, 1947). These areas receive projections from extrinsic thalamic nuclei which relay impulses from somatosensory receptors on the body surface, and also presumably from cerebellar nuclei mediating such kinesthetic impulses (Terzuolo and Adey, 1960). Two projection areas for somatic representation have been delimited. The label S I is given to the larger area, the postcentral cortex; S II is found on the superior bank of the lateral fissure in the monkey (Woolsey, 1947).

Mountcastle (1961) and his co-workers have demonstrated that stimulation of a point on the skin provokes activity in a zone of cells in the postcentral cortex; more responses occur, with shorter latencies, from cells in the center of this active zone. Reciprocally, a response may occur in a cortical cell after stimulation of a rather wide area of skin, but it will follow most reliably from stimulation of a specific point in the center of this peripheral area. In addition, stimulation of skin surrounding the excitatory receptive field for a cortical cell will lead to inhibition of the response of the cell, and thus provide for more discrete patterns of activity in response to stimulation of neighboring points of the skin. Some cells are responsive to touch stimulation of the skin, and others, still within the postcentral area, are sensitive to kinesthetic impulses. The cells in S II are characterized by large and often bilateral receptive fields and often respond to painful stimuli.

Behavioral studies of the projection areas have centered around the assumption that they function in a manner similar to that of the visual projection areas. Deficits in dexterity, in ability to discriminate form tactually, and in nonvisual spatial orientation have been found after ablations in postcentral cortex (Cole and Glees, 1954). Orbach and Chow (1959) found impairment on a variety of tactual discriminations following ablation of S I in monkeys but no unique or additive effects from removal of S II.

Woolsey (1958) has summarized evidence that these sensory areas have some of the defining characteristics of motor areas, since electrical stimulation here can produce motor activity even after the frontal motor areas have been destroyed. Furthermore, electrical responses can be recorded in the frontal "motor" areas if one stimulates peripheral afferent nerves. However, the distribution and characteristics of the responses are consistent with the view that the frontal areas are principally motor, and the parietal areas principally sensory, and behavioral ablation studies reflect the same difference. Precentral lesions produce primarily motor symptoms (Cole and Glees, 1954). Kruger and Porter (1958) showed that while ablation of M I (see p. 56) as well as S I was necessary for permanent loss of a tactile discrimination, M I ablation alone did not produce a severe defect. While the postcentral extrinsic cortex may be considered to be motor as well as sensory in nature, it appears that stimulation of *all* sensory cortex can produce motor effects (Lilly, 1958), and there may prove to be nothing unique about the somatic system in this respect.

The *posterior parietal* cortex is intrinsic (association) cortex, according to electrophysiological and thalamic-degeneration studies. On the basis of its proximity to the postcentral area, and of some indication of anatomical connections with the postcentral area (Cole and Glees, 1954), the posterior-parietal area has been assigned a role in the acquisition or utilization of somesthetic information, although the exact nature of its role is not at all clear.

Lesions of the posterior parietal cortex have been reported to produce deficits on tactual discrimination tasks, coupled with disorders of reaching, jumping, and spatial orientation (Pribram and Barry, 1956; Wilson, 1957; Bates and Ettlinger, 1960). Some studies have reported losses in dexterity and accuracy of orientation without concomitant deficits on discrimination tasks (Pasik et al., 1958; Glees and Cole, 1954). Ettlinger and Kalsbeck (1962) interpret the difficulties shown by monkeys with this lesion as an impairment of position sense, since in almost all experiments some form of disorientation, visual as well as tactual, has been found.

Many of the studies of this area have been concerned with the hypothesis that its ablation is followed by deficits in "higher functions" of tactile learning or memory without the impairment of simple sensory functions which characteristically follows postcentral ablation. Blum (1951) felt that both sensory deficits and disturbances of more complex functions are produced by posterior-parietal ablation. Others have argued (Pasik et al., 1958) that the discrimination-learning deficits which are sometimes seen can be attributed completely to the tremor and incoordination which may be present. A recent study (Wilson, Stamm, and Pribram, 1960) suggests that performance on discrimination problems may be related to the ability of the animal to learn to make fine discriminations which are necessary in complex tasks, and that the postoperative impairment is thus based on less efficient utilization of tactual information.

The posterior parietal area shows strong laterality effects, unlike the presumably analogous area for vision, the inferotemporal cortex. Ettlinger and Kalsbeck (1962) have demonstrated that unilateral lesions produce retention deficits only when the stimuli are presented to the contralateral hand. The findings are consistent with the conclusion that there is no direct projection into S I of the ipsilateral body surface in monkeys, except for the face (see Rose and Mountcastle, 1959), and with the finding of a lack of intermanual transfer of a roughness discrimination after section of the forebrain commissures (Ebner and Myers, 1960; Myers, 1962).

One of the persistent difficulties in analyzing the effects of lesions has been the lack of comparability of the ablated area from experiment to experiment. This has been especially true in studies of parietal lobe function. The disturbances in orientation and coordination can be observed consistently in monkeys with lesions of the lateral part of the posterior parietal cortex, but in order for deficits on tactual learning tasks to appear, ablation of the medial surface seems to be necessary. The severe withdrawal responses noted by Denny-Brown (1960) may be dependent upon postcentral sectors which have been spared in the discrimination learning studies cited above, since they were designed to investigate "learning" apart from sensory defects.

Although one is tempted to assign different functions to the two areas we have considered, the more parsimonious view that the posterior parietal area acts only as a sort of accessory to the postcentral area in discrimination processes can still be maintained. It is noteworthy that no clear "double dissociation" (Teuber, 1955) has been demonstrated between the effects of lesions of the two areas; specifically, it has not been shown that ablation of the posterior parietal cortex affects abilities which are not impaired by postcentral ablation.

Superior Temporal Cortex. After peripheral stimulation of the *auditory system* in monkeys, electrical responses can be recorded over a wide area centering in the most superior part of the temporal lobe. Ablation in this region is followed by retrograde degeneration in the medial geniculate, the thalamic nucleus for audition, and in portions of several intrinsic nuclei. Details of these relationships are complex, and several different "auditory areas" have been proposed (Woolsey, 1961). Most of the electrophysiological as well as behavioral research has been done in cats, and only a brief summary will be offered here.

After all the areas of the cortex which receive projections from the medial geniculate nucleus have been destroyed, cats can still learn to respond to the onset of tones, and to make frequency and intensity discriminations, but the ability to discriminate between sounds differing only in localization, in duration, or in the temporal pattern of the constituent tones is impaired or lost. Neff (1961) suggests that the important difference between these groups of problems is to be found in the fact that for those which are not impaired the two stimuli excite at least partially different sets of neural units in the auditory system. For the discriminations that are impaired, the same neural units are excited by both stimuli, the two stimuli differing only in the temporal arrangement of excitation.

Additional studies (Goldberg and Neff, 1961) suggest that frequency discriminations may be dependent upon other areas of the cortex, but that intensity discriminations can still be made when no auditory information reaches the cortex. It should be noted that in most of the studies of cerebral mechanisms in audition, the focus of attention has been upon the animal's eventual capacity, and temporary loss of a habit after operation has not received as much attention as have, for example, comparable visual losses in experiments on the inferotemporal cortex.

Anterior to the areas of cortex implicated in auditory functions, largely buried within the lateral fissure, is an area which may deserve the title of primate *gustatory* cortex. In the case of this sensory system, the bulk of psychophysiological research has been done with rats; the effects of lesions in a comparable area of the cortex and in the thalamus have been studied and related to other behavioral and to electrophysiological data (Pfaffman, 1959). It is unfortunate that in both rats and monkeys, most of the lesion studies have used the preference method, in which the response measured is the animal's intake of solutions (usually of quinine) of different concentration, when they are paired with water. Obviously a postoperative increase in quinine intake may result either from a change in the discriminability of the fluids or from a change in the animal's tolerance of quinine. At any rate, it is true that lesions in the area indicated have been followed by increases in quinine rejection thresholds in the monkey (Pribram and Bagshaw, 1953). Although lesions in various other cerebral areas produce changes in eating behavior, it is presumed that they result from motivational changes rather than from sensory deficiencies (see Weiskrantz, 1960).

Olfactory deficits have also been produced by lesions of monkey temporal cortex. Lesions of either the temporal pole and superior temporal cortex, or of the inferotemporal ("visual") area, produce an impairment on olfactory discrimination problems using the differentiation procedure

(Brown, Rosvold, and Mishkin, 1963), but only with combined destruction of these areas did a deficit on simultaneous discriminations appear (Santibañez and Pinto-Hamuy, 1957). Based upon the foregoing results and upon the neural relationships of these structures to the *prepyriform cortex*, the primary receiving area for olfaction, Brown et al. suggest that a major focus for olfactory discrimination learning will be found in an area made up of the pole and the anterior part of the inferotemporal area.

Frontal Lobe Function. In the study of cortical function in monkeys, the question of the nature of the effects of destruction of the frontal lobe cortex has been at the same time one of the easiest and one of the most difficult to solve. Jacobsen and others demonstrated in the 1930s that bilateral destruction of the cortex anterior to the motor area produces a profound deficit in the ability of monkeys to perform adequately on the delayed-response and delayed-alternation tasks (Jacobsen and Haslerud, 1936; Jacobsen and Nissen, 1937). But a clear statement of the nature of that deficit, and of its relationship to other, more subtle changes which are seen in frontal animals, is still lacking.

Furthermore, a satisfactory explanation of these behavioral changes in terms of the neural connections of this area has not yet been provided.

Many authors have attempted to "explain" the delayed-response problem and thus to state which ability is lacking or impaired in the frontally operated monkey. The suggestion offered by Jacobsen is perhaps the most obvious one: that delayed response requires that the animal remember over a short period of time which of two or more cups was baited. Thus, inability to solve such a problem involves a "loss of immediate memory." Proponents of such a view have often emphasized that this memory must operate alone: there is no cue present at the time of choice which distinguishes the correct from the incorrect response, such as is available in a visual discrimination problem. This last point may be refuted without recourse to experiment (see Harlow, 1951). In responding to the usual spatial delayed response problem, the monkey has available to him those cues (visual, somesthetic, etc.) which distinguish the food cup on the right from the food cup on the left; he must remember from the baiting experience whether right or left is correct. This is similar to any discrimination problem: certain cues distinguish the two responses and the animal must remember from previous trials which cues indicate the rewarded response.

How then is a delayed response problem different from a discrimination problem? In a single discrimination problem the same cue always signals the presence of the reward; but in the delayed response, the bait (or other signal) and the reward are sometimes on the right and sometimes on the left. Thus, the delayed response may be considered as a series of two-trial problems, involving frequent reversals. The delayed response usually involves spatial features which distinguish between the cues and between the possible responses. These aspects—the two-trial nature, the number of reversals, and the spatial considerations—are among the characteristics which have been explored in further study of frontal lobe function.

Some studies have emphasized the possibility that frontal animals are particularly susceptible to interference from stimuli extraneous to the problem or from previous trials. Thus, on the one hand, Malmo (1942) showed that monkeys with such lesions improved in their ability to perform delayed response problems if they were kept in the dark during the delay interval; on the other hand, Spaet and Harlow (1943) demonstrated that one frontal animal performed much better on such problems when he received only one trial per day than when he received 25 trials per day.

However, neither of these forms of interference has been shown to account completely for the delayed response deficit of frontal animals, and it may well be that normal animals are aided as much as are operated animals by removal of such sources of interference (Wilson, Oscar, and Gleitman, 1963).

Another line of investigation has been directed to the possibility that frontal animals differ from normals in some way in their response to reward (Pribram, 1960*b*). In one experiment they appeared to be relatively less influenced by whether or not the preceding trial had been rewarded, in a noncorrection alternation situation (Wilson, 1962). However, studies which were planned to explore this idea further did not bear it out; frontals do not differ from normals in the asymptotic level of behavior in a probability learning situation, either under a noncorrection or a correction procedure (Pribram, 1962*b*; Wilson and Oscar, in press).

Other procedures have been used to study frontal lobe function, but the data do not assist much at the present time in the analysis of frontal activity. Thus, it appears that alumina cream implantation in this region produces a deficit in the learning of delayed alternation but does not affect retention of the alternation problem (Stamm and Pribram, 1960). Electrical stimulation at a high intensity causes a disruption of accurate delayed-alternation behavior, but, interestingly enough, stimulation at a moderate intensity has been reported to hasten the learning of alternation (Weiskrantz, Mihailovič, and Gross, 1960; Stamm, 1962).

A series of experiments by Mishkin and Pribram (1955, 1956; Pribram and Mishkin, 1956) stemmed from the finding that frontals performed much better on a go-no-go delayed response than on the conventional problem. In the go-no-go problem, the monkey must choose either to go or not go to a single centered food cup; this choice follows a delay period which begins after the monkey receives a cue indicating which behavior (go or no-go) will be correct. These experiments lead to the conclusion that the spatial nature of the cue is more important than the spatial nature of the response in making the conventional delayed-response problem particularly difficult for frontals.

However, it was demonstrated that spatiality, *per se*, is not a single, crucial characteristic which produces difficulty in frontal animals. This possibility and many others appear to be eliminated once again in a series of experiments (Brush, Mishkin, and Rosvold, 1961; Mishkin, Prockop, and Rosvold, 1962; Battig, Rosvold, and Mishkin, 1962), which has also provided a view of frontal-lobe function which is currently popular. In these experiments, frontal animals were worse than normals on visual discrimination problems presented under certain conditions, but performed as well as normals under different conditions. The pattern of results makes it difficult to conclude that the presence or absence of frontal deficit is highly correlated with any of the following variables: two trials versus twelve trials per problem, reversal versus nonreversal of reward-significance of the stimuli, spatial versus nonspatial cues.

The summary of this series of studies was presented in the form of a general hypothesis to describe the behavior of frontal monkeys. "Frontal lesions produce abnormal difficulty in suppressing whatever response normally prevails in a given situation" (Mishkin et al., 1962). This variation on an older view that frontal monkeys are lacking in ability to inhibit can subsume many of the known results. However, it does not convincingly account for the most obvious feature of the behavior of frontal animals, their loss of the ability to perform a delayed response. Mishkin has attempted to incorporate this symptom by suggesting that all monkeys tend to display spatial habits in a delayed response situation and that frontals have "abnormal difficulty" in suppressing the positional response in order to acquire the delayed response habit, but such an explanation would still be unable to account for the loss of delayed response ability when it has been *preoperatively* acquired.

The proponents of this hypothesis extended their investigations to an adjoining part of the brain, the cortex of the orbital (ventral) surface of the frontal lobe (Butter, Mishkin, and Rosvold, 1963). They found that although loss of this area causes *less* of a delayed-response deficit than does loss of the lateral frontal surface, it produces *more* of a change on tasks designed specifically to measure ability to suppress strong response tendencies. Thus, it appears that these two deficiencies are not basically the same.

Lesions in some other structures reliably produce deficits on problems which are sensitive to lateral frontal lesions, and such findings have been incorporated in some of the hypotheses of frontal-lobe function. Connections are found between the frontal cortex and the caudate nucleus, destruction or stimulation of which causes deficits in delayed-response performance (Rosvold and Delgado, 1956; Battig, Rosvold, and Mishkin, 1960). Konorski (1961) suggests that these regions share an important role in maintaining orientation in space, and that destruction of either disturbs recent memory for position, thus producing deficits on spatial delayed response. He believes that frontal cortex lesions produce a separate, often temporary, loss of inhibition which explains the deficits shown on nonspatial problems. Pribram (1958, 1960*a*) stresses the fact that the lateral frontal cortex and the limbic lobe both receive projections from nuclei of the internal core of the thalamus, and, finding that lesions of some limbic structures affect performance on delayed-response types of tasks, suggests that these tasks, like the "instinctive" activities more commonly described as limbic functions, involve sequential performance of a plan of activity.

If we bear in mind also the fact that in some experiments lateral frontal animals appear to be deficient in learning individual auditory or somesthetic problems (Gross and Weiskrantz, 1962; Ettlinger and Wegener, 1958), it seems that a solution may be found only by assuming two or more basic factors in the frontal lobe syndrome. We may conclude with a statement consistent with Mishkin's (1964) current view: perhaps inappropriate use of spatial cues describes part of this picture, but a more general difficulty in overcoming strong response tendencies is also involved. These may have some genetic relationship but be partially separable in neural locus.

LIMBIC AREAS AND RETICULAR FORMATION

Extremely drastic changes in the spontaneous behavior of animals can be produced by lesions in various parts of the limbic system. An untamed monkey, fearful and aggressive in turn before the operation, will become placid, curious, and almost friendly after destruction of the amygdala. After a lesion of the septal area a mild-mannered laboratory rat becomes aggressive and irritable. These effects and others are described most readily as changes in emotional behavior and several experiments have been done to explain them on this premise.

Some studies have involved measurement of "emotional behavior" in a social situation. Thus, it has been noted that lesions of the amygdala are sometimes followed by changes in a monkey's position in a social hierarchy (Rosvold, Mirsky, and Pribram, 1954), that cingulate cortex lesions do not impair the ability of monkeys to cooperate in a food-getting problem (Stamm, 1958), etc. Related to such studies are earlier demonstrations of the importance of limbic areas in "instinctive" behavior, e.g., cingulate lesions in rats cause an impairment in maternal behavior (Stamm, 1955).

The majority of the early attempts to analyze the effects of lesions in limbic areas used some form of avoidance conditioning, but the pattern of results is not completely clear or consistent. Thus, lesions of either the hippocampus, the amygdala, the frontotemporal cortex, or the cingulate cortex have produced faster extinction or slower acquisition of avoidance responses in monkeys (Weiskrantz, 1956; Pribram and Weiskrantz, 1957). On the other hand, apparently conflicting results are reported: rats form avoidance responses more rapidly after hippocampal or septal lesions (Isaacson, Douglas, and Moore, 1961; King, 1958).

The search for emotional-motivational consequences of limbic activation or destruction was stimulated partially by considerations of its anatomical position and connections. Papez (1937) set forth a "proposed mechanism of emotion," pointing out that the limbic areas provided a means of integration between the newer cortex ("higher processes") and the subcortical centers in the hypothalamus (autonomic responses). Furthermore, the circuit of connections (from anterior thalamus to cingulate cortex to hippocampus to hypothalamus, and back to anterior thalamus) could possibly be important in the mechanism of emotion, providing a means for sustained activity. While these relationships have continued to influence research and theory, it has become evident that alternative views of the role of the limbic areas should be considered, and that the specific functions of the separate structures must be examined.

An attempt has been made to subdivide the limbic system and to assign functions which still fit within the general categories of motivation and emotion. MacLean (1959) suggests that the amygdala, medial forebrain bundle, and frontotemporal cortex, along with some of the reticular areas, are part of a system concerned with "preservation of self," while another system (including the septal area, cingulate gyrus, and the hippocampus) works for "preservation of the species." He offers evidence based upon the effects of stimulation of these two systems; alimentary acts such as chewing and swallowing, and defensive behavior often follow stimulation of the first system, while pleasure reactions, grooming, and sexual behavior are obtained from stimulation of the second.

Another explanation of the differential effects of lesions in the limbic areas emphasizes the nature of motor patterns required in the solution of a problem. McCleary (1961) demonstrated that lesions of the subcallosal cortex of cats caused a loss on a *passive* avoidance problem but left intact an *active* avoidance response, whereas lesions of the cingulate area had opposite effects. This pattern is consistent with that provided by stimulation experiments (Kaada, 1951): stimulation of subcallosal cortex inhibits, while stimulation of cingulate area facilitates motor activity.

Meanwhile, evidence has accumulated which makes it difficult to summarize all of the effects of limbic lesions under the "emotional-motivational" heading. Monkeys with lesions of various limbic structures have been found deficient on several kinds of discrimination tasks; principally, but not solely, on the problems which are severely disrupted by lateral frontal cortex lesions (Orbach, Milner, and Rasmussen, 1960; Pribram, Wilson, and Connors, 1962). Analysis of these "nonemotional" changes has not yet proceeded very far, but one attempt to provide a specific hypothesis can be reported. Schwartzbaum and Pribram (1960) have hypothesized that amygdalectomy disturbs the ability to generalize. This change leads to an alteration in the reinforcing properties of new stimuli and thus may appear as an "emotional" or "motivation" change, although it is not basically such.

It appears then that it would be inappropriate to pin any simple label of "emotional area" upon the many structures of the limbic system; we will see later what roles some present-day psychological theorists assign to it. Additional evidence relative to limbic function has come from recording of brain electrical activity. We shall consider some of these results as part of a discussion of the reticular formation.

The midbrain reticular formation has been studied extensively in the last few years and destruction or stimulation of this area has been shown to have profound effects (see Magoun, 1958, for review). It receives fibers from all the sensory systems. These may come as collateral fibers from axons coursing in the specific sensory paths or as a separate set of fibers from a sensory receptor. Input from several senses may converge upon a single cell. This suggests that any possible information concerning the nature of the input is lost but this need not be so. The pattern of excitation may vary among the different inputs and thus may be maintained in the activity of the reticular receiving cell. The reticular area also receives input from the cerebral cortex, particularly from the frontal and sensori-motor cortex, and from the cortex of the limbic areas.

The outflow from the reticular area includes excitatory and inhibitory influences upon spinal motor activity, but it is its influence upon cortical activity that will concern us most. The midbrain reticular formation also influences activity in the thalamic nonspecific system, consisting of the central group and the reticular nucleus of the thalamus. Stimulation here produces cortical activity, but the cortical effect of stimulation of part of this system is more localized than the effect of midbrain system activation. Excitation of these systems, the midbrain and the thalamic "arousal" systems, can illustrate the control they have over the electrical activity of the cortex and the behavior of the organism.

The normal EEG of an awake but unaroused animal presents a rhythmical wave of moderate intensity with a frequency which is characteristic of the species. Activation of the reticular area leads to the same change as that which is typically produced by an external stimulus, a desynchronization of the resting rhythm and the production of a nearly "flat" record. Electrical stimulation of the reticular area will awaken a sleeping animal and higher intensities produce apparent fear, agitation, and attempts to escape. Destruction of the reticular formation usually produces a comatose animal which can not be aroused behaviorally by external stimuli, and whose EEG can only briefly be provoked into showing the arousal pattern of desynchronization.

The reticular system is thought to play yet another major role in controlling neural activity. A mechanism for explaining attention and similar psychological processes has been seen in the fact that stimulation of the reticular formation changes the intensity of the neural responses to stimuli, as measured in the classical sensory paths. Furthermore, similar changes in the response to a stimulus are seen as a result of behavioral manipulation. (In the commonly cited experiment of Hernández-Peón, Scherrer, and Jouvet (1956), the electrical response to a sound measured in the dorsal cochlear nucleus of the cat was greatly diminished by the presentation of a visual stimulus, two mice in a bottle.) It has been suggested (Hugelin, Dumont, and Paillas, 1960) that the change in the auditory response may be the result of even more peripheral events—the contraction of muscles in the middle ear which reduce the effective intensity of the sound—but such a decrease in response has been demonstrated at the cortical level in cats with these muscles cut (Moushegian, Rupert, Marsh, and Galambos, 1961).

The behavioral event just noted, the shift in attention, may be considered to be part of the process of learning. One of the most recent trends in neuropsychological investigations has been the study of electrical events correlated with learning. There has been great variety in the procedures used, areas studied, and the results obtained, but some conclusions seem to emerge from several studies of conditioning.

The presentation of a novel stimulus leads to widespread changes (including desynchronization) in the EEG. With repeated presentations, these effects diminish in degree and sharpen in location. This decrease in electrical change is correlated with a decrease in behavioral responsiveness and is specific to the stimulus employed. Novel stimuli will cause the reappearance of the activation pattern (Sharpless and Jasper, 1956).

If reinforcement (a US) is added to the procedure, following the original stimulus (now the CS) on each trial, there is a reinstatement of the arousal reaction to the CS. This precedes the appearance of a behavioral conditioned response. Furthermore, the electrical response to the CS may change from a widespread desynchronization to a response localized to the site of the US (Morrell and Jasper, 1956).

Changes are also noticed in certain subcortical centers and the sequence of these changes has led some to suggest that the midbrain and thalamic reticular areas are involved in the formation of connections at different stages of learning (Gastaut, 1958). Slow waves appearing in the hippocampus at an early stage of learning have been variously interpreted, e.g., as related to the execution of a planned sequence (Adey, Walter, and Hendrix, 1961) or as indicative of the formation of temporary connections and the concomitant inhibition of diffuse discharge provoked by the midbrain reticular formation (Grastyán, 1961).

Particularly interesting are the experiments in which flickering lights are used as stimuli. It is known from earlier investigations that such a stimulus may impose the frequency of its flicker upon the electrical response of the nervous system; presumably, the areas of the brain showing such a response are those involved in the behavior resulting from the stimulus, and thus in the learning process. Activity in various areas has been traced and it has been shown that in a trained animal, the frequency of activity in nonspecific systems is that of the stimulus appropriate to the response being made regardless of which stimulus is actually presented (John and Killam, 1960). The frequency-specific responses are also seen during interim, nonstimulated periods, as learning progresses.

It must be emphasized that these very interesting experiments and all studies of electrical activity changes during behavior are "correlational" studies and subject to the limitations of these procedures. The necessary events in learning may be taking place elsewhere or be of a completely different nature. The electrical changes recorded may be only a reflection of such a process. Indeed, the "correlation" is not even perfect. As noted above, characteristic electrical changes precede the appearance of a behavioral CR; such "signs of learning" remain intact in extinction after the overt response has disappeared. Chow, Dement, and John (1957) have used a transfer situation to demonstrate that conditioning of the EEG may take place without any sign of appropriate behavioral response to the CS. However if we view learning as an intervening variable which is only imperfectly manifested by any given response measure, it may eventually be demonstrated that some aspect of cerebral electrical activity does indeed correspond to learning so defined. It may be more convenient to adopt a physiological definition of learning, rather than to pursue a search for the physiological correlates of each specific behavioral indicator.

HYPOTHALAMUS AND REINFORCEMENT

Stimulation or destruction of parts of an extensive system running through the subcortical forebrain and midbrain leads to changes in behavior. Many of these effects are apparently related to the physiological functions of motivation or reward, but not all are easily subsumed under such headings.

Three major streams of investigation have contributed to knowledge of this system. W. R. Hess (1954) and others have shown that stimulation of subcortical centers, principally in the hypothalamus, produces changes in behavior, including a large number of autonomic responses. Destruction of certain points leads to an increase in consummatory behavior (most clearly in the case of eating), whereas destruction of other points causes a decrease; the opposite pattern of results may be produced by stimulation of these points. Finally it has been shown that stimulation in certain parts of this system will cause positive reinforcement of preceding behavior, whereas stimulation of other points is negatively reinforcing.

Positive reinforcement of preceding behavior, displayed dramatically in the self-stimulation experiments (Olds and Milner, 1954), is produced most readily by stimulation in a system which involves the lateral hypothalamic area, part of the midbrain, and a tract known as the *medial forebrain bundle* (Olds, 1962). A secondary system appears to be more dorsal, including various limbic areas and parts of the thalamus and caudate nucleus. The reinforcing effects of stimulation of these various structures may differ in more than intensity, but so far little attention has been paid to such differences. Some attempts have been made to determine the general characteristics of this central rewarding effect, and to determine its relationship to a more conventional reward and to the animal's drive state.

Although the effects of ESB (electrical stimulation of the brain) in a reward area are probably not exactly the same as those of a more conventional reward, many alternatives to the hypothesis that ESB is truly reinforcing have been eliminated. It has been demonstrated that rats will learn a maze for ESB, showing improvement between days and between trials within a day (Olds, 1956). Stimuli associated with ESB will come to have secondary rewarding properties (Stein, 1958). Clearly the changes in behavior involved here cannot be thought of as simple motor automatisms triggered by direct neural stimulation.

Some studies have led to the suggestion that responses reinforced by ESB show unusually rapid extinction (Seward, Uyeda, and Olds, 1959). This appears to be true of ESB only under those conditions of electrode placement and current which are not very rewarding, since ESB in other loci, for which animals will work on a schedule with a high ratio of response to reward, also leads to slower extinction (Brodie, Malis, Moreno, and Boren, 1960).

Some differences still seem to obtrude. Rats can learn to space their responses by a required interval of time when food is given for each properly timed response; when ESB in certain neural structures is made the reward in this situation, performance deteriorates (Brady and Conrad, 1960). And, in a runway situation, animals perform better with massed trials than with spaced ones when ESB is given as a reward (Seward, Uyeda, and Olds, 1960), although apparently not when food or water is used (Cotton and Lewis, 1957).

The lateral hypothalamic portion of this positive reward area appears from separate evidence to be intimately involved in eating and drinking behavior (Anand and Brobeck, 1951). Electrical or chemical stimulation of the area will produce eating in an apparently satiated animal. This excessive eating cannot be explained as a nonspecific increase in ongoing activity nor as a general increase in the tendency to gnaw on all objects. Animals will perform instrumental acts at an increased rate in order to get food. After destruction of this area, animals will stop eating and drinking, and consequently, die. This is not as simple as it may seem, however. If animals are maintained alive by tube feeding, they recover eating and drinking over a variable period of time according to a rather complex series of steps (Teitelbaum and Epstein, 1962). Motivation for food may still be impaired even after apparently normal eating and drinking have reappeared.

Other findings in addition to the overlap of the areas involved point to a relationship between these feeding areas and the reward area. Rats will work for ESB whether they are hungry or not, but only with certain placements will they work harder if hungry than if not hungry (Margules and Olds, 1962). This situation arises with electrodes in the lateral hypothalamic area, or with electrodes nearby and a current of the intensity necessary to achieve stimulation of the lateral hypothalamic area.

Although the areas concerned appear to be more widespread, it has also been possible to delimit areas in which self-stimulation rates decrease markedly after castration, and return after androgen injections (Olds, 1958).

Quite near the areas discussed above are others, the stimulation of which appears to have negatively reinforcing effects upon behavior (Delgado, Roberts, and Miller, 1954). This system appears to encircle the major reward system and includes parts of thalamic and midbrain nonspecific systems. Animals will perform responses that lead to escape from such stimulation although demonstration of true avoidance of brain stimulation is rare.

Some of the evidence for escape without avoidance can be explained by assuming that the stimulation from which this phenomenon arises is eliciting both positive and negative effects (Roberts, 1958; Stein, 1962). This finding is obtained with many placements, and usually takes the form of noting that for a short train of pulses, stimulation is rewarding but with greater duration of stimulation, the ESB becomes punishing. It has been suggested that this happens when the electrode points are located in a positive region but are near a negative region. The neurons of the positive area are stimulated as soon as the ESB begins. With continuation of the stimulus, physiological spread takes place into areas which are punishing; this effect overcomes the positive effect of the early part of the stimulation.

Interaction between the positive and negative systems has been investigated. Olds and Olds (1962) found that a continuous train of negatively reinforcing stimulation given while the rat is being tested for self-stimulation at positive points causes retardation of response rates, but continuous positive stimulation enhances the speed with which rats escape from negatively reinforcing stimuli. These findings, and others, lead to the suggestion that activity in negative areas acts by inhibiting activity in positively rewarding areas, while increased activity in positive areas enhances all behavior, including escape from negative stimulation (Olds, 1962).

There are ventromedial hypothalamic areas, destruction of which leads to hyperphagia, i.e., increased food intake (Brobeck, Tepperman, and Long, 1943), and stimulation of which is reported to produce a reduction in eating (Smith, 1961). The effect of lesions in these "satiety areas" is as complex as that of the lesions which produce aphagia. At least two effects have been studied: increased intake of easily obtained, highly palatable food, and increased finickiness in the choice of foods; it appears that the lesions related to these effects can be somewhat differentiated although they overlap (Graff and Stellar, 1962).

The lateral and ventromedial hypothalamic areas are considered to be particularly critical in the integration of information concerning the state of deprivation or food intake of the organism; hypotheses and data concerning the nature of such information (neural, chemical, thermal) are summarized by Rosenzweig (1962) and Teitelbaum (1961). Lesions of both of these areas produce the same effects on food intake as lesions of the feeding area alone, consistent with the view that the feeding area is nearer to the final common path and is normally under the inhibitory influence of the satiety area (Stellar, 1954). This view finds further support in the fact that lesions of the satiety center which increase eating also augment self-stimulation rates in the feeding areas, and that stimulation of the satiety centers has the additional effect of reducing feeding center self-stimulation (Hoebel and Teitelbaum, 1962).

Stimulation of the ventromedial area also produces negative reinforcement according to Olds (1962). The interrelationships appear, therefore, to be consistent if not immediately intuitively understandable. Within the areas in which stimulation produces positive reinforcement, there is an area in which stimulation produces eating. Within the area of negative reinforcement, there is an area where stimulation causes a cessation of eating. To account for these relationships Olds presents a model of the reinforcement mechanism according to which neural activity from drive and from reward are normally projected to the same area, although with different functions. ESB, "being something of a bludgeon, has the effects of both" (p. 594).

Miller (1961) would offer a different interpretation, and emphasize different facts. Thus it is noted that rats will learn to turn *off* stimulation in the lateral hypothalamic (feeding) area, and the fact that the onset of stimulation in this area is reinforcing is explained as due to reduction of some other drive by general effects of the stimulation. Conversely it is pointed out that stimulation in the ventromedial area may be momentarily *positively* reinforcing, consistent with a drive-reduction theory explanation of its effects on food intake. On the other hand, it is possible that the unpleasant negative effects of ventromedial stimulation are of primary importance, that the cessation of eating results secondarily and is not due to a true decrease in the hunger drive (Krasne, 1962).

A complete reinterpretation of many of the experiments cited in the foregoing discussion may be necessary in the light of a recent study. Reynolds (1963) suggests that the production of a lesion by passing direct current through the brain may have the effect of *stimulating* structures surrounding the locus of the lesion, since this commonly used procedure produces local hemorrhage and leaves particles of the metallic electrodes in the brain. When lesions of the ventromedial hypothalamus were produced by radio-frequency current (which is free of these side effects), hyperphagia did not ensue. The conclusion is that the hyperphagia seen in other experiments is due to stimulation of the lateral hypothalamus, that the ventromedial nucleus has no role in control of food intake.

Some experiments have been undertaken to determine whether reinforcing stimulation affects behavior motivated by more conventional drives. Olds and Olds (1961) have found that intermittent delivery of a train of positive stimuli retards learning of a simple reversal problem for food reward. They suggest that this may be explained as a result of partial reinforcement of both correct and incorrect responses, and that an influence of stimulation upon learning per se need not be invoked. However, stimulation of negatively rewarding areas had no effect upon learning scores. Other studies also suggest some involvement of these areas in learning mechanisms. Thus, Mahut (1962) has shown that stimulation of the posterior hypothalamus and the nonspecific nuclei of the thalamus at intensities which are not great enough to be reinforcing can disrupt learning. Furthermore, lesions of similar areas in the cat and in the rat have been followed by deficiencies in learning. If the effects upon performance of ESB are to be described in psychological terms, it is doubtful that an explanation in terms of reinforcement alone will be sufficient.

MOTOR SYSTEMS

It is interesting that very little physiological information about the motor systems has been incorporated into psychological theory. In spite of the influence of behaviorism, most of what is considered to be psychophysiology has been concerned with the physiological basis of "mind" or sensory experience and feelings. This is largely a matter of definition: knowledge concerning the relationship between structure and motor activity does exist, but it is not generally considered to be psychological subject matter.

The advances in knowledge about the sensory systems have had a parallel in advances in knowledge about the motor system. The primary projection areas of the sensory systems were discovered, and the association areas defined by exclusion. On the motor side, a similar situation existed. A cortical system for motor activity was discovered which has the sort of spatial arrangement which provides the illusion of an explanation. These cortical areas contribute a large part of the fibers of the anatomically defined *pyramidal* system. Motor mechanisms which lie outside this system are then, by definition, *extrapyramidal,* and separate functions for the two systems have been sought. As with the projection area-association area distinction it has become apparent that the two systems are intimately interrelated, and definition of separate functions is thus far of limited usefulness (Patton and Amassian, 1960; Jung and Hassler, 1960).

The pyramidal tract is defined as those fibers which originate in the cortex and, as they pass to the spinal cord, make up the pyramids, two cone-shaped eminences on the anterior surface of the medulla. The pyramidal tract arises partially from the motor area of the frontal lobe, but also from the postcentral parietal cortex, and presumably from the posterior parietal, occipital, and temporal lobes as well (Terzuolo and Adey, 1960).

When different parts of the precentral motor cortex (M I) are stimulated, movements of different parts of the musculature are seen. Analysis of the effects of stimulation of the cortex and of the pyramidal tracts further downstream indicates that the cells which are connected to a single muscle are closely grouped in the cortex, but there are such complex interconnections of cortical cells that stimulation in the cortex produces an organized movement pattern which may involve several muscles. The parts of the body thus represented fall into an orderly sequence on the cortex; each side of the body is represented on the opposite side of the brain, with upper parts of the body lower on the cortex, and with the more medial parts of the body toward the front of the cortical area.

But movements may be elicited by stimulation of many parts of the cortex; indeed of almost all of the exposed cortex (Lilly, 1958). There is a supplementary motor area in the medial frontal cortex (M II), and motor pathways arise in the "sensory" areas S I and S II. Movements may also be produced by stimulation of posterior parietal, temporal, and occipital points. Some of these areas appear to function via M I, but some are at least partially independent; some operate through fibers of the pyramidal tract, some through the extrapyramidal system.

The pyramidal system has been traditionally regarded by physiologists as concerned with voluntary movement, as opposed to reflex movement, which is said to be under the control of the extrapyramidal or spinal systems. This statement is obviously difficult to prove, but to the extent to which it may be tested by using human cases, or by framing reasonable definitions of "voluntary" for use in animal studies, it appears to be an oversimplification at the very least.

Lesions of the precentral motor cortex or section of the pyramidal tract produce a flaccid paralysis of the affected parts of the opposite side of the body. Following precentral lesions recovery of function recurs after a period of time which depends partially upon the amount of tissue originally removed. Subsequent operations upon adjoining cortex (Cole and Glees, 1954) or motor cortex of the other hemisphere (Pribram, Kruger, Robinson, and Berman, 1955/6) may cause a renewed defect. Even when paralysis of a hand is not complete, a monkey shows a great increase in the amount of time necessary to perform skilled acts, such as opening a problem box, but the sequence of necessary subacts may still be correctly exhibited. Thus it may be concluded that a lesion in this area does not produce a loss due to the excision of the locus of a habit, although it may clearly disturb the skilled performance of acts.

The cortical portion of the extrapyramidal system arises from essentially the same cortical areas as the pyramidal tract, but before terminating on cells in the spinal cord, it synapses with cells of the basal ganglia or the brain-stem reticular formation. The interconnections of the basal ganglia (the caudate nucleus, putamen, and globus pallidus) with cortical areas, the thalamus, the cerebellum, the reticular area, and other midbrain structures are complex; we may simply note that the final outflow to the spinal cord has cells of origin in the reticular formation or the red nucleus of the midbrain.

In humans with various diseases involving abnormal motor activity (chorea, athetosis, etc.), pathological changes are seen in the basal ganglia. Stimulation of some of these structures can produce motor activity or an apparent inhibition of motor activity, and experimental lesions (particularly when combined with cortical lesions) may give rise to disorders of movement (Jung and Hassler, 1960). These findings have led to the general view that the major activity of these structures is in controlling motor behavior and coordination, and to the further suggestion that they fulfill primarily an inhibitory role.

As we have noted earlier, recent studies have shown that lesions of the caudate nucleus are followed by other changes, not clearly motoric in nature. Monkeys appear to lose interest in their environment (Dean and Davis, 1959) and perform poorly on delayed response and delayed alternation problems (Rosvold, Mishkin, and Szwarcbart, 1958). Rosvold and Mishkin have emphasized the relationship of the caudate to the frontal cortex, as noted above. Buchwald and his co-workers (Buchwald, Rakic, Wyers, Hull, and Heuser, 1962) have postulated a more complex inhibitory system, the "caudate loop," involving the nonspecific nuclei of the thalamus as well. This system is thought to be concerned with the integration of input to the brain and to be of importance to all "higher processes." Thus while the involvement of the basal ganglia in certain disorders of posture and movement is certain, their role in behavior is not restricted to the control of motor activity.

One summary is offered by Denny-Brown (1960), who conceives of several levels of integration with final cortical dominance. The motor cortex selectively modifies the final response pattern in cooperation with sensory input. The triggering action of tactual input releases the appropriate response; the function of the cortex is therefore one of suppression of inaccurate, and facilitation of appropriate patterns of action. At lower levels of neural integration, responses are modified and selected in grosser ways. Thus, the level of integration of motor responses in terms of what stimuli will release them is a function of finer and finer differentiation by higher nervous system structures.

The picture of motor organization that emerges is one of different degrees of differentiation and patterning. The reflex activity of the spinal cord is integrated into patterns of postural organization, and movements and voluntary acts are superimposed on the on-going body tonus with its built-in feedback to maintain certain levels of excitation. This system is in turn modified and acted upon by the afferent systems, which also contribute to the central level of excitation and the resulting responses. Centers of motor function are thought of as being arranged in the same sort of hierarchical system as are the sensory systems. Each level in the system contributes organization and patterning to the activity by virtue of its connections with other parts of the brain.

STRUCTURAL BASIS OF LEARNING

We have attempted to survey the present state of neuropsychological knowledge in a geographic fashion. Some of the experiments cited have provided us with knowledge about the possible *mechanisms* underlying psychological processes, but most of them have been concerned primarily with the *locus* of the process.

Presumably, the cellular and synaptic mechanisms discussed earlier provide us with the basic mechanisms involved in nervous system activity and thus in psychological processes. At the present time studies are increasing in which an explanation is sought for changes in nervous system response to a given input. Prominent among these are studies relating to the structural basis of learning, and a few of these must be briefly mentioned.

Rusinov (1953) demonstrated that many interesting effects follow the local application of anodal (positive) current to a region of the cortex. If an exteroceptive stimulus is presented while such a current is being applied to a portion of the motor cortex, movements of the limb corresponding to the polarized cortical region may be produced, although neither external stimulus nor anodal current alone produces the effect. A "dominant focus" thus established may affect behavior for up to twenty minutes after the source of the current is removed. Some pertinence of these findings to the phenomenon of learning is suggested by other studies. A tone was presented several times during the period of polarization and produced a motor response; during the twenty minutes that followed termination of the current, the tone still produced the response although a light did not.

When potassium chloride is applied to the cortex, waves of *spreading depression* spread from the site of application over the hemisphere (Bureš, Burešová, and Záharóva, 1958). Spreading depression results in the temporary loss of reflexes and learned responses, and it is characterized also by a *negative* potential at the area affected. Morrell and Naitoh (1962) have shown that direct application of a surface-negative polarizing current to the visual cortex causes a decrement in a conditioned avoidance response to light in rabbits, although polarization of the motor cortex has no effect.

Another line of investigation has been concerned with the possibility that changes produced by learning are to be found in the molecular structure of the cell. Hydén has shown that various kinds of stimulation of the nervous system generate increases of ribonucleic acid content of the cells. Training of rats on a difficult balancing task produces an increase in RNA in cells of the vestibular nuclei (Hydén and Egyházi, 1962). An increased amount of RNA is also found in a cortical area which has been made abnormally excitable by bombarding it with stimulation from an overactive area of cortex (Morrell, 1961).

Intercellular chemical mechanisms in learning have been studied by Rosenzweig, Krech, and Bennett (1960), who have tested various possible consequences of the hypothesis that the acetylcholine-cholinesterase system which is involved in transmission at some synapses is of importance in learning. They have demonstrated a complex relationship between the amount of cholinesterase and acetylcholine available in rats of different strains and their behavior in learning tasks. Furthermore, they have found an influence of behavior (environmental complexity) upon a biochemical measure, their index of cholinesterase activity.

Olds' (1962) microelectrode studies of single cells may also be cited here. The procedure consists of recording activity from a single neuron and then rewarding the animal after each spontaneous response of the cell. The "reward" is ESB in a positively reinforcing area. From some cells this procedure produces an increase in the response rate, which may even convert intermittent firing into continuous activity; such cells are found in the cingulate and hippocampal regions but not in neocortical areas. Some questions of control remain unsettled but this appears to be a fruitful area of research.

Evidence concerning the possible nature of the physiological mechanisms underlying learning and memory can also be obtained from studies utilizing techniques which are less molecular than those cited above. There has been a reawakening of interest in consolidation theories of learning and memory; such theories state that some process inaugurated by exposure to a learning trial continues for a period of time after the trial is terminated, and that optimal memory for the trial requires that this process not be interrupted. This perseverative process is generally considered to be some form of temporary neurophysiological activity which produces a more impervious change in the nervous system if allowed to run its course. An equivalent theory can be offered as a descriptive theory of behavior without physiological correlates, and evidence from behavioral studies can be brought to bear upon it, but much of the recent work in this area has involved physiological interruption of perseveration and has been stimulated by hypotheses concerning the physiological nature of the perseverative process, and of the permanent neural change.

Thus it has been demonstrated that electroconvulsive shock given to rats after each trial on a learning task will have a deleterious effect upon performance, an effect which is more severe the shorter the interval between trial and shock (Duncan, 1949). It is presumed that the massive electrical discharge of the seizure interrupts the perseverative neural process and cuts short some fixation process which is taking place. At least two alternative explanations of this ECS effect have been proposed, however. Coons and Miller (1960) have suggested that ECS is unpleasant, and that what appears to be amnesia for the correct response actually may be an avoidance of the consequences of the response, since it has been followed on previous trials by ECS. According to Adams and Lewis (1962), the convulsion produced by ECS becomes conditioned to the stimuli of the experimental apparatus, and by competing with the "correct" response in the experimental situation, produces the decrement in performance. The apparently conflicting results of the experiments cited by the various authors can be understood, plausibly if unparsimoniously, by concluding that all three factors may play a part. At any rate, the consolidation theory is supported further by the finding that post-trial ECS can block the *disruption* of a response. A rat which has learned to bar-press for food, and then on one trial is given a painful foot-shock through the bar, will on a subsequent test day refrain from pressing the bar. An ECS immediately after the foot-shock should enhance this tendency not to press according to either the Coons-Miller or Adams-Lewis theories, but in fact rats so treated show a continuation of bar-pressing on the subsequent day, apparently due to forgetting of the foot-shock (Heriot and Coleman, 1962).

Another body of evidence relating to consolidation theory comes from studies of the effects of various drugs upon learning. It may be noted that procedures which would control for analogues of the Coons-Miller or Adams-Lewis theories of ECS, i.e., theories emphasizing the possible role of the drug as a reinforcing agent or as a stimulus producing a conflicting response, have not always been employed.

The injection of some convulsants (e.g., pentylenetetrazol) and anaesthetics (e.g., pentobarbitol) after a learning trial produces apparent amnesia for the trial (Pearlman, Sharpless, and Jarvik, 1961), whereas the posttrial injection of certain stimulants (e.g., picrotoxin) increases the rate at which learning is shown (Breen and McGaugh, 1961). There is also some evidence that surgical anaesthesia in humans produces retrograde amnesia, providing a link between the experimental animal data and the large body of clinical reports of such an amnesia following loss of consciousness.

In interpreting the improvement in learning from posttrial injections of drugs, it has been assumed that the pharmacological agents enhance the degree of perseveration set up by each learning trial. More detailed explanations of the nature of such enhancement will vary with the exact pharmacological action of each drug: one suggestion (Breen and McGaugh, 1961) is that such a drug may act by slightly inhibiting cholinesterase activity, thus increasing the effective amount of acetylcholine present.

Studies of these kinds will undoubtedly continue to increase in number, and lead to the solution of some of the basic questions of neuropsychology. Indeed, as the questions of mechanisms are answered, the questions of locus may seem less and less important.

HUMAN DATA

The data presented thus far have been based upon studies using animals of many different mammalian species. In most of the studies of neocortical function, experiments done with monkeys have been cited, but in most of the work on subcortical mechanisms, rats, cats, and dogs have served as subjects. It might be desirable in making such a survey to restrict discussion at first to a single species. Unfortunately, there are not sufficient data on any one species to give an adequate picture of brain-behavior relationships. It seems likely that many of the conclusions presented here will have to be modified when more is known about species differences in neural function.

However, one approach is to take any apparent difference in function of a brain area as a challenge, on the assumption that a structure which can be identified in two species *does* underlie similar behavior in both. Then, the task would be to discover the common element in the apparently dissimilar behavior patterns of the two species (Pribram, 1958). For this reason, as well as because of its intrinsic interest, we turn to the study of cortical function in humans.

Most of the information on human brain mechanisms came originally from the study of individual patients who were seen because of some abnormality of behavior and subsequently studied at autopsy where injury due to disease or accident was described. Now several additional and in general better sources of information are possible: studies of the effects of electrical stimulation during operation (Penfield and Rasmussen, 1950); study of the effects of removal of tissue at surgery (Milner, 1958); and study of the effects of accidental injury in samples chosen on the basis of injury, not on the basis of the effect of injury (Teuber, Battersby, and Bender, 1960; Semmes, Weinstein, Ghent, and Teuber, 1960).

In his studies of brain-stimulation in conscious patients, Penfield was able to delimit cortical areas for somatosensory and motor function, topographically organized along the central sulcus much as in other animals. Stimulation of occipital areas caused visual sensations, and in similar fashion, auditory function was "localized" in the temporal lobe, and speech in various areas of frontal and temporal cortex. Ablation of these areas supported the notions of localization.

More detailed studies have not changed the main outline but have added facts which do not fit an extreme localization theory any more easily than they do a holistic theory of brain function. While recent work (Teuber, 1959) has confirmed the existence of circumscribed sensory defects after lesions of the primary projection areas, these same studies suggest that impairment on complex perceptual responses of various types seems to be at least partially independent of the locus of lesion. Even within the primary projection areas, defects in function cannot be ascribed wholly to sensory losses of any simple kind. For example, after penetrating wounds of the occipital lobe, visual field defects are found much as would be expected, but in addition are found changes in critical flicker frequency, dark adaptation, and other visual functions in the "intact" portions of the visual field. Similarly, lesions in pre- and postcentral areas may give rise to such effects as a higher two-point threshold on the contralateral side, which is understandable on the basis of projections to the cortex from the body surface, but difficulty in discriminating objects tactually also results and appears with the use of either hand in people who show the threshold change on only one side.

A still more general level of functioning can be described in which deficits are noticed regardless of locus. Lesions anywhere in the cortex will affect complex tactual tasks or complex visual tasks such as finding a hidden figure (Teuber, 1959). Wolff and his co-workers (1958) emphasized similar results: performance on tests requiring memory, abstraction, tolerance for frustration, etc., was impaired, and increasingly so the larger the lesion.

The question of the existence and nature of functions specific to association areas has been raised many times. Penfield assigned "interpretive" function to the areas of cortex adjoining the primary projection areas, and demonstrated a degree of modality specificity. The visual abnormalities arising from temporal lobe lesions were fitted into this description (Penfield, 1958). Milner (1958, 1962) has demonstrated a dissociation of temporal lobe function between the hemispheres. Left temporal lobe excision produces difficulty in the recall of verbal material while disease or destruction of the right temporal lobe appears to produce nonverbal perceptual defects such as failure to grasp a visual anomaly, or poorer auditory discrimination.

Subcortical temporal lesions have produced even more striking results (Milner, 1958). Bilateral hippocampal removal produces a profound memory loss which has been compared to the difficulty shown by a frontal monkey on delayed response. This comparison becomes even more interesting when it is noted that the monkey retains perfectly an overlearned discrimination, and the human shows no impairment on tests of general intelligence.

A summary of posterior area deficits in humans suggests that there are some specific functions which may be attributed to these areas. Some of these functions can be correlated with primary sensory defects but are not entirely explained by them. Some of the differences in function may thus be described in terms of sensory modality, but others require the use of more abstract distinctions such as verbal versus nonverbal.

The frontal association areas have traditionally been the locus of the higher mental processes such as abstraction, foresight, etc. Halstead (1947), for example, differentiated between subjects with frontal lesions, and control subjects and those with nonfrontal lesions on his tests of these functions. Contrary evidence is given by Teuber (1959), who finds that tests which appear to measure abtraction as it is usually defined do not distinguish frontals. Penfield and Rasmussen (1950) characterized the frontal areas as "dispensable cortex" except for speech areas. While it may be dispensable, Penfield and Rasmussen note, nevertheless, that after frontal lobe lesions there is "impairment of mental processes necessary for planned initiative." The subtlety of this defect contrasts remarkably with the reliability and magnitude of the deficit that monkeys with frontal lesions show on delayed response problems. The bridge between the defect in humans and the impairment in monkeys is still being sought.

However, differences have been found on some tasks between patients with frontal lesions and others. Unlike Teuber, Milner (1964) found that frontals were impaired on a card-sorting task. The perseverative errors which were made could not be attributed to deficits on sensory, motor or attentional factors. She found further that subjects with lateral frontal lesions showed more perseverative errors than orbitofrontals, a finding which is at variance with the results of Butter, Mishkin, and Rosvold (1963) in monkeys. In another test, in which subjects were required to follow a path through a maze, the frontal subjects apparently could not resist making errors even when they could tell they were wrong. However, Milner suggested that perseveration was a symptom of the deficit, not a cause.

The negative results on tests of presumably higher mental functions which Teuber has reported in the past are balanced by tests on which he has uncovered symptoms peculiar to frontal lesions (Teuber, 1964). Frontals are retarded in visual searching, presumably because of improper use of the afferent feedback from motor activity which normally controls the execution of responses. Frontals have difficulty in finding the vertical when postural and visual cues offer conflicting information, and in touching parts of the body corresponding to those indicated on a diagram. These symptoms lead to the suggestion that the function of the frontal mechanism is the presetting of responses. This activity is conceived of as a high-level motor compensatory function which allows for the prediction of events in a certain order coupled with the ability to see one's self as an agent in relation to this order.

This hypothesis about frontal lobe function may be able to subsume the kinds of changes in behavior which previous investigators have characterized as changes in cognition and affect. The difficulties shown by lobotomized subjects have been related by Greenblatt and Solomon (1958) to reduction in drives, inability to be affected by past experience, and inability to "elaborate or sustain experience." If one is willing to conceive of these defects as failures of motor presetting mechanisms which ordinarily direct behavior over time, whether or not frontal subjects show "stimulus-bound behavior," "inability to profit from past experience," etc., may be found to depend upon specific aspects of the situation studied. The fact that many of the studies of frontal-lobe function have used subjects previously psychotic or neurotic makes interpretation of behavior after lesions even more difficult. However, with an hypothesis such as Teuber's, it seems possible that a rapprochement with subhuman data and an answer to the problem of frontal lobe function may be found.

THREE RECENT PHYSIOLOGICAL THEORIES

Historical Background. The main problem for early students of neuropsychology was to discover the nature of the central events which intervene between stimulation and response. One obvious approach was to seek evidence for localization of function; we have summarized above the recent research in this field. Historically, however, the fruitfulness of such attempts has been severely challenged by those who have denied or minimized the importance of functionally differentiated units, at least in the cerebral cortex. (See Krech, 1962, for a summary.) The history of neuropsychology during the nineteenth and early twentieth centuries has often been seen as a controversy between the "localizers" and the "holists."

Pavlov belongs among the localizers in view of his theory of psychophysiology, a theory which involves a picture of neural function with great influence upon subsequent views of behavior and physiology. His transcortical reflex theory (1927) assumes that stimuli, interoceptive and exteroceptive, are conducted along discrete paths to discrete terminal points in the brain. The brain is thought of as a complicated mosaic, made up of structurally different sensory and motor areas. Structure rather than mechanism is the central explanatory concept. Stimuli which excite particular cortical points will be affected by excitatory and inhibitory processes, making possible different central excitation from identical stimuli. In Pavlov's theory, no association areas need be postulated since the association of cortical receptor and motor areas mediates reflex action. This is possible because the receptor system (or *analyser*) includes the sense organ, afferent tracts, projection areas, and connections with motor cortex. Conditioned reflexes, which are the basis of all learned activity, are formed when excitation from a new stimulus occurs simultaneously and contiguously with a focus of strong excitation which is already present. Unconditioned stimuli arouse such centers of activity, and the reflex response is mediated by neural paths which have innately lowered resistance to impulses from the appropriate point. The path of the new stimulus is thus facilitated by its association with the unconditioned stimulus.

More recently, Konorski (1950) has extended a similar explanation to include varieties of conditioning which were not discussed by Pavlov. Additional physiological mechanisms are required in order to explain Konorski's "second type of conditioning" (instrumental conditioning based on reinforcing stimuli), and the kind of behavior which has been characterized as "insightful" (involving associations between neutral stimuli). The second type of conditioning involves background activity in the motor cortex which arises from a conditioned stimulus signaling a positive or a negative reinforcement. If a response is performed which leads to avoidance of a negative reinforcement or to delivery of a positive reinforcement, connections are formed between the centers of activity representing the movement and the conditioned stimulus. The *mechanism* of conditioning is the growth and multiplication of synapses. In this case, the connections are made in the motor cortex, between motor neurons and the axons of cells which originate in the area of the conditioned stimulus.

Konorski's analysis of intelligent behavior leads him to distinguish between the "gnostic" and the "affective" aspects of stimulation. In simple conditioning, connections are formed between the gnostic (sensory) center of the conditioned stimulus and the affective center of the unconditioned stimulus, but insightful behavior depends upon connections between the gnostic centers of two stimuli. Thus, associations can be formed between neutral stimuli, associations which may be described as expectancies, and displayed later in "insightful behavior." These mechanisms are presumably more highly developed in primates than in lower mammals.

Lashley is generally considered as a major opponent of localization theories, although he himself demonstrated the dependence of some functions upon specific cortical areas. He objected to reflex theory (1930), primarily because of his assumption that the plasticity of behavior must be subserved by a flexible neural organization. His interference theory (1942) was developed in order to specify a neural substrate for learning and memory without making use of notions such as transcortical reflexes or topographical representation of sensory excitation. It seemed reasonable to Lashley that memory or learning is a reduplication of a stimulus pattern throughout a whole functional area rather than a specific set of connections. In brief, Lashley proposed a translation of the spatial attributes of a stimulus into a modification of the spatial patterns of postural organization by means of such interference patterns at each level of activity in the nervous system.

Some of Lashley's more recent ideas (1958) did not emphasize the interference pattern *per se*. He expanded on the importance of on-going central activity in determining the effect of a stimulus. Low level tonic excitation of memory traces becomes the basis for cerebral organization. Both Lashley and Pavlov argued against the existence of separate centers for higher mental processes. Their theories agree that the process is the same no matter what level of the nervous system or what species is being analyzed: the process which is postulated is very different, however, for Lashley and for Pavlov.

In 1949, Hebb presented the first version of a physiological theory which tries to salvage the idea of association by discarding the outmoded physiological theory on which it was previously based, and by utilizing new physiological facts to provide a respectable base.

Hebb's Neuropsychological Theory.[2] Hebb, like Lashley before him, explicitly rejects the concept of transcortical reflexes, and its corollary assumption of the sensory control of experience and behavior. But he also rejects Lashley's interference theory of the nature of the central event. In their place, he proposes a central semiautonomous process. This is made up of cell assemblies, units of neural activity which correspond to sensory events, and phase sequences, series of cell assemblies ordered in time which correspond to perception and thinking. On-going central activity is the physiological referent for attention, set, or any other process which acts selectively on stimulus input. The association of assemblies with each other and with central representations of motor activities is the structural basis for the translation of patterns of sensory input into patterns of motor activity.

---

[2] This discussion is based primarily on the following references: Hebb (1949, 1955, 1958, 1963).

Cell assemblies and phase sequences serve as physiological explanations of psychological processes. Hebb denies that these constructs are really physiological since they do not derive directly from physiological data; their physiological appeal lies in the fact that they do not blatantly contradict facts. Furthermore, Hebb says that they are too complex to be tested directly by physiological methods; only in the behavior of the whole organism does it make sense to talk about phase sequences. But if we accept the standard levels of analysis, the cell assembly *is* a physiological construct since it is presumed to underlie behavior. Let us first examine the way in which neural activity is organized into cell assemblies, according to the theory.

The assumption of linear transmission of impulses from sensory to motor areas is not consistent with either the physiological or psychological data, and Hebb therefore makes use of the reverberating circuits described by Lorente de Nó (1938). Such a system, made up of groups of neurons which are arranged in closed, self-exciting circuits, is the basis of the cell assembly. Reverberating systems are set up in such closed circuits as a result of repeated afferent stimulation. As one neuron fires another, this increases the probability that it will excite the same neuron again. This is explained as a consequence of decreased synaptic resistance; perhaps due to growth of synaptic knobs which increase the area of contact between one neuron and another, but in any case, because of some structural change. Consequent firing of the same group of cells is facilitated with each occurrence of the stimulus which originally excited the system.

These circuits are set up in the association cortex. In vision, for example, it is assumed that fibers from the primary projection area go to preoccipital areas, the "visuopsychic areas" and here, closed circuits are stimulated and formed into cell assemblies (see Fig. 2-4). Although a given receptor cell will be fired by many stimuli, each stimulus will excite a unique pattern of receptor cells. It is assumed that two or more afferent impulses are usually necessary in order to fire a third cell upon which they converge. Successive application of this principle of convergence can lead to the excitation in association areas of quite different populations of cells by stimuli which originally excited widely overlapping groups of receptor cells.

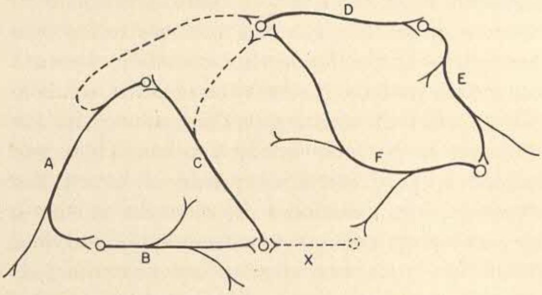


Fig. 2-4. Cortical circuits, their establishment and interconnection. If A, B, and C are all excited by the same sensory event, then A will deliver impulses to B at the moment that B is firing, and the synapse A-B will be strengthened. Eventually stimulation of A, B, or C alone could set up reverberations within this circuit. Broken lines show possible connections between this group of cells and the D-E-F circuit, which might permit them to function as a unit. (From Hebb, 1958, p. 104)

Milner (1957) pointed out that certain subsidiary assumptions about the formation of cell assemblies must be made if the system is to work, physiologically. The most important one is the postulation that inhibitory cells are also fired when excitatory cells are excited. If every cell excited every other cell with which it is contiguous, excitation would snowball. The more activity in the brain, the more likely it would be that all of the cells would be fired, resulting in a seizure rather than a cell assembly. It is also useful to assume that the inhibitory fibers which are fired by a particular neuron have connections mainly with excitatory neurons other than the one which fires them. This arrangement provides for greater inhibition of these other cells. A group of cells which are firing will tend to keep themselves firing but will not allow the excitation to spread all over the cortex, and thus it will provide the pattern of excitation which makes up a cell assembly.

Perceptual learning begins with the organization of cell assemblies corresponding to the elements of a figure. Each of these assemblies includes a motor component, a tendency to fixate on other parts of the figure, i.e., on other elements. Successive activity in the various cell assemblies will lead to their association, the tendency for one assembly to be followed by another. Thus, perceiving a figure is a consequence of having first organized into assemblies the elements making it up (including reflex eye movements), and then establishing temporal overlap between the individual assemblies so that each assembly facilitates all the others. If perception is organized in this way, it is necessary for the organism to have had experience with the stimulus components before it can perceive the total figure. The components will be of characteristic number and kind for each figure. For example, anything with three angles and three sides will be seen as similar, since any such figure will activate the common assembly which corresponds to "triangle."

The notion that perception depends upon some prior simpler process is consistent with Senden's (1960) reports of the course of development of vision in the congenitally blind who first gain sight in adulthood. The idea is supported further by recent studies of stabilized images (Hebb, 1963). Under normal conditions, the eyes are constantly in motion; if this motion is prevented, figures in the visual field fade in a few seconds. But such figures fragment: some portions of the figure may disappear independently of the rest. The fact that a figure fragments suggests that it must be made up of components which are either aroused or not. Perception of the figure depends upon discrete activations and is not a unitary event. Parts of a figure which are meaningful, or which the observer is attending to, are perceived longer.

For activity is already under way in the nervous system before a given stimulus is presented, and such on-going activity will influence the subsequent course of neural events. Attention is the central facilitation of a sensory event, i.e., a cell assembly is already active by virtue of central processes at the same time that it is aroused by a stimulus. Meaning is specified as an interconnecting set of assemblies which can remain active longer than any of its components. The factors of meaning, attention and set can now be seen to contribute to the stability of some parts of a figure since they represent central facilitation of sensory effects.

A phase sequence is a series of cell assemblies ordered in time. The complete act of perception involves the activation of a phase sequence, as each of the associated cell assemblies corresponding to the elements of the figure is activated in turn. A more complex and extended phase sequence underlies the course of thinking. The duration and content of thought will be determined by both sensory and central events, since the assemblies which make up a phase sequence may be determined either by an external stimulus, or by a central facilitation, or by both (see Fig. 2-5). In thinking, as in perceiving, the reverberatory nature of the neural networks will insure that activity persists for a short period of time beyond the duration of the stimulus. This activity represents

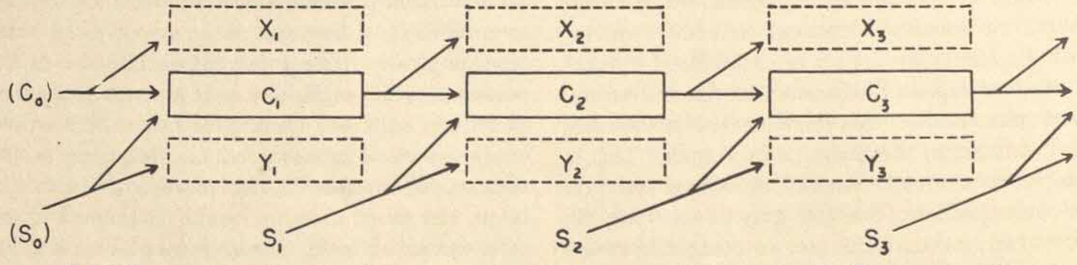


Fig. 2-5. Sensory and cortical influences upon the course of thinking. C_1, C_2, C_3 are assemblies which are active at successive moments in time, since each receives excitation from both sensory input and the previously active assembly. The selective central influence upon thought that stems from the nature of the preceding thought may be called *attention*. *X* and *Y* receive excitation from only central and sensory events, respectively, and thus remain subliminally excited. (From Hebb, 1958, p. 106)

transient memory for the idea or for the figure perceived. Permanent memory depends upon the sort of structural changes described above in the formation of the cell assembly. These changes will lead to alterations in the ease of passage of excitation from one cell assembly to another, i.e. to the stronger association of already organized cell assemblies. Such changes are the physiological substrate of associative learning.

The distinction between perceptual and associative learning is used by Hebb to explain developmental and species differences. Early learning in the human is slow and continuous since sensory elements must be combined through repetition before perception can occur. Until there has been a history of sensory stimulation, the autonomous slow wave activity of the normal resting cortex predominates. In the first stages, learning consists of perceptual learning; the organization of this on-going activity into cell assemblies and phase sequences which can then be easily activated by a sensory event. Learning can be seen as the process by which the same central effect is elicited by a stimulus, repeatedly, bringing central activity under sensory control. Later learning consists of associating already organized systems into higher-order ones, increasing the probability that a given assembly will excite another.

This description of the learning process as consisting of two stages, one slow and continuous, the second efficient and fast, leads to predictions about phylogenetic differences as well as maturational ones. Since learning in its initial stages consists of organizing the association cortex into assemblies, it follows that species with less association cortex have less to be organized, and also less structure with which to build higher-order systems. In these species, initial learning should take less time, but the degree of complexity of final learning will be limited. In man, in contrast, the early stages of perceptual learning are slower and it takes longer for the fast learning characteristic of the species to be achieved, but the final possibilities are also much greater.

Hebb's treatment of motivation is based upon the presumed role in behavior of the diffuse activating systems of the brain. The brain is aroused by diffuse bombardment through these systems in an amount generally proportional to the total amount of sensory input. It is suggested that this is analogous to a drive state, since activation is necessary in order that the cue value of specific stimuli be utilized, i.e. for effective perception, thinking and behavior. When the arousal system is not functioning, the organization of the brain in terms of cell assemblies and phase sequences gives way to the slow-wave, hypersynchronous activity characteristic of sleep. For this reason the central state is labeled *semi*autonomous: neural activity of the central state can never be completely independent of afferent input. Cortical function will be facilitated by a moderate amount of diffuse stimulation, but intense stimulation will disorganize central firing, and with disruption of assembly systems the cue function of a sensory event is lost. Here we have the hypothesized physiological basis for emotional disruption: intense stimulation producing disorganization of central activity and behavior.

From these conclusions some predictions about the nature of reinforcement follow. What is reinforcing will depend upon the level of arousal of the system; at low levels of arousal, increases in activation will be reinforcing; at high levels of arousal, reduction in activation will be reinforcing. These predictions follow since only with a moderate amount of activation will phase sequences run off smoothly and thus be facilitated, becoming more likely to occur in the future.

Let us look at the kinds of sensory-central interactions that are logically possible. The assembly aroused by a sensory event may be independent of the dominant on-going activity in the brain. If there is an enduring system which maintains its independence in spite of sensory input, Hebb would define this as *intention:* the central activity will guide behavior. If sensory states and central states set up incompatible phase sequences, or if incomplete sequences are initiated, we have a basis for conflictful disruption of behavior. When sensory processes actively support central activity, it produces the kind of reinforcement which attention produces. Time relationships give another dimension to these processes. If a phase sequence is aroused by central facilitation before it is aroused by a sensory event, we get an expectancy. If the sensory event facilitates the same sequence, the expectancy is confirmed: this is cognitive reinforcement. Voluntary behavior will be a function of expectation, set and present stimulation. If central facilitation arouses a sequence which has been aroused before by a sensory event, you get recall. Thus memory will involve the same kinds of activity that took place originally in perception; perception, it would appear, automatically involves recognition.

Hebb's theory thus relies on an on-going central process which interacts with sensory events in various ways in the mediation of responses. The model of the brain which it presupposes is in line with current data. A two-process theory of behavior is assumed whereby the midbrain structures contribute to the level of arousal of the organism while the sensory tracts carry information, which in turn contributes to the level of arousal. Hebb's theory is characterized by an emphasis upon the development of perception from component sensation already organized by repeated stimulation. His discussion of the development of the central semiautonomous process makes no sharp distinction between innate and learned factors. Cell assemblies must develop from something, and this history will include maturation, early experience, metabolic, and biochemical variables. The argument about whether perception is learned or not depends upon the definition of learning employed; stimulation is certainly necessary to the development of normal perception.

Hebb's theory is specific, in a way that Lashley's is not, about the way in which stimulus-response relationships are possible. Hebb says they are *associated,* i.e., some structural change is involved. But this is an association of systems and circuits, rather than of discrete sensory and motor elements, and it is this difference that Hebb uses to explain the plasticity of behavior. If one could follow the effect of a stimulus until its "final outcome" is observable, one would observe stimulation impinging upon the central network of assemblies and phase sequences. The stimulation would excite one or another sequence of activation, depending upon the nature of the on-going assemblies and the level of arousal of the organism. One part of the sequence of activity may facilitate a cell assembly involving motor cells, thus producing an overt response. Motor equivalence will depend upon the existence of *expectancies* of the results of responses which alternate with perceptions of the present position of the organism.

In conclusion Hebb provides a physiological mechanism for explaining perception, learning, motivation, emotion, memory, attention, reinforcement, expectancy, voluntary behavior, and sleep. The fact that these are all functions of particular aspects of the semiautonomous central process does not make the theory less convincing.

Pribram and the Neural Regulation of Behavior.[3] When we turn to a consideration of Pribram's theory, it must be noted that it has developed not as a single theory but as a series of statements about processes underlying behavior on several levels. One subtheory, or set of statements, concerns the organization of the nervous system. Another set of statements attempts to correlate this organization with various psychological processes which are inferred from behavior. At another level, the molar behavior of individuals in an environment is dealt with as a function of the preceding sets of statements. Two points may be noted about this way of proceeding. First, that the kinds of categories which are familiar to behavior theorists do not always emerge; motivation, learning, emotion, perception, etc., terms for which Hebb was seeking a physiological referent, are replaced by new psychological constructs which organize these processes differently. Secondly, although Pribram has drawn his psychological constructs from physiological models, every level of theory is influenced by behavioral as well as by physiological data, and even the most global level of psychological analysis is bound up with neurological variables.

The heart of Pribram's statement at all levels lies in the way he views the brain. No less than Hebb, Pribram has insisted that psychologists give up the kind of old-fashioned physiological thinking which is obviously inadequate for explaining the structural basis of behavior. These ideas include not only simple reflex theory, but also the higher-lower view of structure and function which has dominated previous theorizing.

The Victorian world view presupposed a gradient from lowest to highest. The addition of complicated and higher structures to the nervous system explained the appearance of complicated and higher function in higher animals. Evolution was thought to be a direct line of development from simple brains and simple behavior, sometimes called instinctive, to complicated brains which mediate complicated behavior, sometimes called learned or variable. The assumption that simple behavior was innately determined by structures that were phylogenetically old, while complicated or variable behavior was mediated by structures that arose later in evolution is paralleled by assumptions about processes in the individual. The same dichotomy is preserved in the distinction between new parts of the brain, which subserve intellectual functions, and old parts of the brain which subserve motivational-emotional functions. It was presumed that the higher centers influenced lower centers in the same perfect way that the upper classes restrained the lower classes in society. Furthermore, there were higher intellectual functions and lower intellectual functions, so that levels of complexity of structure were also thought to exist in the higher levels of neural organization. The portion of the brain which had traditionally been reserved for the highest level of ideation was the neocortex, with the cortex of the frontal lobes being the highest of all.

---

[3] Sources for this discussion are: Pribram (1958, 1960*a*, 1960*b*, 1962*a*); Miller, Galanter, and Pribram (1960).

Pribram sees many difficulties associated with theories based on this differentiation. The dichotomy between old and new behavior processes cannot be maintained any more easily than that between old and new parts of the brain. Both "instinctive" and learned behavior, when analyzed, depend upon both old and new structures and upon both fixed and variable patterns of response. The assignment of function to a particular structure raises still other difficulties. Finally, the notion of a direct line of development, with new features added along the way, is not an adequate account of the differences in behavior to be found in animals with different kinds of brains.

The comparative-evolutionary view of brain-behavior relationships dominated the interpretation of data from human clinical material as well as from animal experiments. It is in an attempt to substitute an entirely different way of organizing the data that Pribram offers his theory based on the conception of the concentric brain.

The evolution of brain and behavior according to this view is a matter of increasing differentiation of both structure and function. Analysis in terms of horizontal levels is supplanted by a model of the brain which supposes that it is organized concentrically. An *internal* core of structures surrounded by an *external* system can be distinguished at all levels of vertebrate phylogeny. Both parts increase in complexity as evolution proceeds. One does not look for new functions appearing with new structures, but rather for differences in the way behavior is organized as a function of varying degrees of differentiation in the nervous system.

The systems in mammalian brain which are designated *internal* are found close to the central canal; they include the arousal systems, the hypothalamus, parts of the thalamus, the limbic areas, and the frontal association cortex. These structures are alike in being made up of short-fibered units with many interconnections. They are multisynaptic, and graded responses with slow transmission are the rule. In contrast, the *external* parts of the brain include other sectors of the thalamus, the projection areas of the sensory and motor systems, and the posterior association areas of the cortex. These structures are, for the most part, characterized by long fiber tracts, fewer synapses, and larger fibers. This produces fast, efficient, impulse propagation as opposed to the diffuse excitation in the internal core system.

Both internal and external thalamic sectors can be subdivided in another classification. Depending upon whether a particular thalamic nucleus receives extrathalamic input or not, it has been described as *extrinsic* or *intrinsic*. Since both internal and external parts of the thalamus have both intrinsic and extrinsic nuclei, a four-way classification is possible (see Fig. 2-6). For example, within the external section, extrinsic nuclei include the

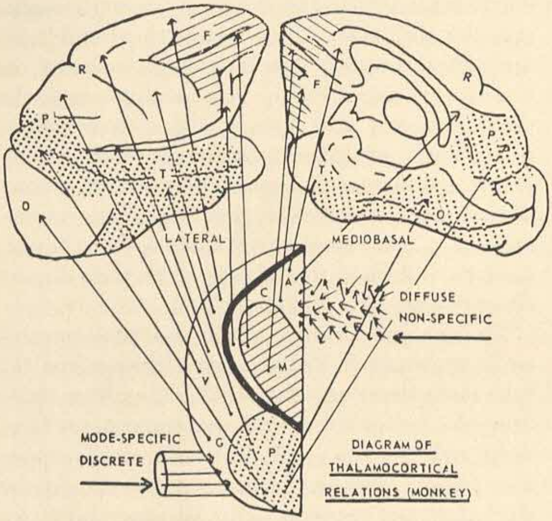


Fig. 2-6. Pribram's outline of the thalamo-cortical relationships in the monkey, and of the subdivisions of cerebral cortex. The broad black band in the thalamus represents the division between the internal core which receives nonspecific, diffuse input and the external portion which receives discrete, modality-specific projection tracts. The stippled and cross-hatched areas represent the intrinsic systems; the remainder of the figure shows the extrinsic systems. (From Pribram, 1958, p. 150)

thalamic way stations for the sensory tracts, such as the medial and lateral geniculates; intrinsic nuclei, such as the pulvinar, were distinguished on the basis of the belief that they had no such input from fiber tracts from the periphery. The projections from the thalamus are such that intrinsic and extrinsic cortical and subcortical structures can also be described. Thus, limbic areas of the brain, while internal, can be described as extrinsic by virtue of the extrathalamic input to the diencephalic nuclei which project to them. (See Table 2-1.) Pribram acknowledges that this is a simplified schema, but it serves to organize a search for communalities of function.

He finds that internal parts of the brain have to do with the excitability of the central nervous system while the external sectors are involved in transmission and differentiation of sensory input. The former are nonspecific diffusely active systems, whereas the latter are modality-specific impulse-propagating systems. The extrinsic parts of both systems are alike in that they are made up of specific afferents: sensory fiber tracts in the external sector, input from the hypothalamus and brain-stem arousal systems in the internal sector. The intrinsic systems, on the other hand, are said not to have any large amount of input from outside the thalamus. These relationships and communalities comprise the physiological theory, or first level of organization. This proposal about the organization of the nervous system is based on many kinds of criteria: electrophysiology, comparative histomorphology, retrograde degeneration studies, cytoarchitecture, strychnine neuronography, and experimental analyses of behavior. Now these parts of the brain must be related more specifically to behavior.

The *external extrinsic* systems are perhaps most easily understood. Pribram has characterized the external systems as composed of long fiber tracts from the periphery. These sensory systems have been well studied, and thalamo-cortical projection systems mapped in some detail. While the mechanisms of receptor activity, transmission, interaction, and final consequences are still unknown, the fact that these fiber tracts can be traced from sense organ to topological arrangement on the cortex makes the problem of assigning function to these areas seem less difficult than for other parts of the brain.

The *internal* core structures which are also *extrinsic* by virtue of their input via the thalamus are also concerned with information, but in this case it appears to be information about the internal state of the organism rather than about the external world. In the dichotomies between old, instinctive behavior and new, variable behavior, central core structures such as the limbic areas had been assigned motivational-emotional functions. Much had been made of the fact that these structures are phylogenetically older. Pribram characterizes these structures in another way: as systems which regulate and are sensitive to the level of excitability of the nervous system. Therefore, their function will be highly dependent upon the state of the organism and upon environmental variables which contribute to the state of excitability of the organism. What is common to the patterns of behavior which depend on these structures is not their instinctual or simple quality, but the fact that they all involve sequences of responses which are ordered in time. They can be grouped together structurally and functionally because mating, maternal behavior, fighting, eating, etc., all involve series of responses which are dependent upon the completion of acts in certain orders, and upon the effect of particular responses on the level of arousal of the organism.

The kinds of processes which are mediated by the internal core system are labeled "dispositions" by Pribram. They involve behavior which is regulatory and which therefore must depend upon the internal state. The regulation depends partly upon the slowly adapting receptors which are located around the third and fourth ventricles, largely in the hypothalamus, and partly upon the diffuse activating systems of the brain stem and thalamus. It is known that the receptors in the central core are sensitive to changes in the internal state which have to do with appetitive-consummatory functions such as concentration of O_2, blood sugar, electrolytes, and certain hormones. Departures from some predetermined level will set up dispositions in the organism, tendencies to respond in such a way as to achieve the proper level. Since the organism is then disposed to respond in some way, the concept includes an energizing function, and also directional and equilibratory functions. The fact that the central receptors are selectively responsive to various states of imbalance provides the cue function which is necessary to explain the direction of behavior. These receptors are called homeostats by Pribram to emphasize their sensitivity to states of arousal. Homeostats, together with the activating systems, provide the mechanisms for the basis of dispositional behavior. The diffuse systems determine the general state of excitability of the organism, with the homeostats signaling departures from some level of excitability.

However, the level of excitability can and will be changed by the activating systems, and thus, the homeostats are said to be *biased* or preset to particular levels. Thus, the activation of behavior will depend upon the general level of excitability of the organism as well as the specific stimulating effects from internal receptors. Furthermore, the reciprocal relationship between sensory input and the brain stem activating system provides another source of arousal.

The theory has now provided for the energizing of behavior, for the direction of behavior, and for the adaptiveness of behavior. Motivation, in this theory, is not always a matter of tissue deprivation, and not all responses will reduce the level of excitation or provide reward. What will set up a disposition to respond in a certain way will depend upon the setting of the homeostat, the particular level of arousal of the organism, and contributions to this momentary state of arousal from extraorganismic events.

The necessity of including feedback processes in sensing mechanisms such as the hypothesized homeostat led Pribram and others (Miller, Galanter, and Pribram, 1960) to suggest the substitution of a new unit of analysis for the traditional *S-R* reflex arc. Since the only determinant of response in the traditional reflex arc is whether the stimulus is intense enough to cross the threshold, this does not account for the widely shown efferent control of receptor activities. It is proposed that in the initiation of responses the input is tested against some centrally maintained level of activity, and response will follow if there is a discrepancy between the two. Incongruity between input and on-going excitation is the important determinant of behavior. The response will effectively bring the organism to the state that is being tested for. Thus, the organism controls the effect of the stimulus by feedback from the response to the receptor. This has been labeled the TOTE unit to distinguish it from the simple S-R reflex. The sequence of Test, Operate, Test, Exit is thought to describe the activity of the sensing mechanism upon the input, and the response resulting from the results of the test for incongruity between states.

This paradigm serves to explain the simplest kinds of sequences characteristic of homeostatic activity. In the systems which test for internal states of the organism, differences in level of excitability can be envisaged as the effective stimulus which continues effective until incongruity vanishes. But in the sequences of behavior which develop from dispositions of the organism which are more complicated than simple vegetative functions, it may be necessary to think of the TOTE unit as a unit of control, or information, rather than as a loop of neural impulses.

The homeostats do not act in an isolated manner, however. The system is made up of an interconnected series of homeostats such that feedback from each one regulates the excitability of the whole system. Pribram makes use of Ashby's (1960) term *ultrastable system* to describe the fact that the whole system will return to a predetermined level of excitability because of feedback from individual homeostats. The mechanism as a whole is sensitive to local changes because these represent fluctuations in the general state of excitability.

This interconnected ultrastable system, organized around the sets of homeostats, is identified anatomically with the limbic system. Pribram rejects the suggestion that these structures mediate only old, simple, instinctual behavior as contrasted to new, complicated, variable, learned behavior mediated by neocortical structures. In Pribram's view, limbic area structures regulate sequences of behavior which are based on dispositions of the organism. While dispositions may be produced as a consequence of excitation of the central core homeostats, the important feature of the activity of this system is the fact that these responses are sequentially arranged. Dispositions may involve responses that are unlearned or learned, but they will demand the completion of one unit of behavior before the next unit can be initiated. This is true of the responses that are seen in sequences of behavior having to do with fighting, fleeing, mating, eating, etc. The loss of memory and of ability to carry out a plan that follows limbic area destruction in humans is taken as evidence for the disruption of a system necessary for the execution of sequential responses leading to a preset state of the organism.

The two extrinsic systems, in summary, have different connections with afferents. These afferents, in the external portions of the forebrain, result in transmission of sensory input; in the internal portion, they produce graded potential changes which result in changes in excitability.

However, the theory, as presented thus far, is incomplete both physiologically and functionally. We must also consider those parts of the brain which Pribram labels intrinsic. The *intrinsic* systems, both *external* (posterior) and *internal* (frontal) are alike in their physiological aspects: both are randomly organized neural networks with graded response mechanisms, inhibitory mechanisms and variable interconnections and patterns of firing. Their functions will be limited by these properties and by the properties of the stimulation which is given.

While the intrinsic systems are alike in their structure and general function, they differ in accordance with their participation in the lateral system on the one hand, and the internal core system on the other. The *external* intrinsic system is related to the sensory projection systems which respond to certain properties of stimulation. The *internal* intrinsic system is related to the limbic system which responds to changes in levels of excitation determined by central core mechanisms.

It has been held that brain function consists of afferent impulses which bear sensory information from stimuli, efferent impulses which give rise to muscular and glandular effects, and in between, structurally and functionally, associations which connect the two. Association areas were also called upon to associate stimulus elements so that higher functions could be derived from lower. The obvious loci for association areas were the large, electrically silent regions of the brain which lie outside the sensory and motor projection areas. Transcortical reflexes were presumably propagated through frontal and posterior association areas, subserving learning, perception, thinking, and other intervening processes.

Pribram rejects this view, citing the following reasons: First, afferent impulses terminate in the "motor" as well as the "sensory" cortex, and efferent fibers originate in the sensory as well as the motor cortex. Secondly, attempts to disturb association processes by isolating intrinsic and extrinsic areas have not been successful. Thirdly, attempts to trace transcortical paths of excitation by anatomical or electrical means have also been unsuccessful. Finally, it is theoretically difficult to conceive of perception, learning, and other processes as transcortical associative mechanisms. While Hebb's theory is an attempt to do just this, it is clear that the semiautonomous central process is a different mechanism than a transcortical reflex.

In Pribram's theory, the intrinsic systems, which are the classical association areas relabeled, are indeed necessary for higher function, and do act upon stimulation relayed from primary sensory pathways. But the way in which this is thought to happen makes necessary a quite different view of brain function. Rather than assuming that the intrinsic areas act upon stimulation received via transcortical connections from the extrinsic areas, he suggests that the intrinsic system sends efferents to some subcortical locus where they act upon input to the extrinsic system. For example, in the posterior intrinsic system, the inferotemporal cortex might be efferently connected with the superior colliculus and intrinsic efferents would modify the incoming afferent impulses which terminate in the striate cortex. This efferent activity is called "partitioning of sets." Efferent activity determines the fate of sensory input by limiting the range of possibilities to which the input can be assigned. It provides patterning of sensory input. The function of the intrinsic sectors, therefore, is to provide a representation of classes of stimulation which determines the effectiveness of incoming stimulation. In addition, since this activity of the intrinsic cortex can be modified by experiential factors, some afferent input must be hypothesized, but its nature has not been specified in the theory.

In the external sector, modality-specificity is claimed for different parts of the posterior intrinsic system, since its relationship to mode-specific primary projection areas is stressed. While the extrinsic projection areas are necessary for responding to the presence or absence of stimuli, the intrinsic areas make possible differential responses and the perception of invariances in stimulation. Partitioning, the efferent activity of the intrinsic cortex, results in the comparison of stimulus input with on-going excitation, and the stimulus is responded to on the basis of how it departs from this neural representation. Every stimulus is modified by, and in turn, modifies, the activity which is the basis of partitioning. Thus there is efferent control over what is perceived.

With repeated stimulation, the class of representations which will be responded to in the same way becomes delimited. The organism will continue to respond to different objects in the environment until his responses do not change the representation. He is then making as fine a discrimination as the situation requires.

The physiological referent for a representation of classes of stimulation is not specified further, but it is clear that the intrinsic areas do not exist in this theory merely to propagate impulses from sensory areas. Rather, they are operating on the input in such a way that finer and finer discriminations are possible, and learning what is the relevant aspect of a stimulus is provided for. The partitioning mechanism, which programs, or patterns, stimulation in accordance with its own activity, could not be an accumulation of all the inputs in the organism's history against which each new input is checked for divergences and similarities. It is more feasible logically and physiologically to think of the programming mechanism as a coded representation such as a cell assembly (Hebb, 1949) or an adaptation level (Helson, (1948). Both of these concepts fulfill the requirements which are necessary for partitioning of stimulation to occur since they provide an on-going process which is sensitive to stimulation but not completely determined by it, and which modifies the effect of stimulation as a function of its own activity.

The intrinsic area of the internal core system, the frontal area, is said to be necessary to the ordering of sequences of responses toward which the organism is disposed. These dispositions have been related to changes in the state of the organism. We have already discussed the lower levels of this hierarchical organization; the highest level is located in the frontal cortex, which serves as an "association area" for limbic cortex. It is necessary to arrange and utilize information from lower levels so that dispositional responses can be carried out over time. These organized, ordered dispositions are labeled "intentions," or "plans." Instructions to behave in a way which will return the organism to a predetermined state result from the partitioning of input by the frontal area. Information is selected by the efferent activity which interacts with it in the same way as in the posterior system, but the incongruities which the organism abolishes in this case are differences in level of arousal rather than differences in pattern of excitation. Frontal cortex is necessary for the presetting of dispositions to respond. When the organism modifies and carries out responses in such a way that a sequence of acts is produced which ends the disparity between the present state and what the organism is disposed to do, the representation will be stabilized.

Modifiable neural representations are the basis, then, of higher functions. Some representations involve invariant properties of the environment; these are based on sensory input to adjoining posterior areas. Some are based on properties of the internal state; the behavior and results of behavior of the organism are incorporated into these representations. When the organism notices incongruities, this initiates intentions. Obviously, both perceptual and dispositional incongruities will contribute to the initiation of intentions. But intentions are independent of immediate stimulus variables. The organism with a frontal lobe mechanism can identify, predict, and initiate various outcomes on the basis of the representation built up from previous outcomes. In Pribram's terms, the posterior intrinsic mechanisms provide the information; the frontal intrinsic mechanisms provide the strategy to attain the outcome which is desired.

Pribram has attempted to show that intention, or will, may be used as a label for the kind of behavior which is characteristic of a certain level of neural organization. The hierarchical organization of the internal core, with its multilinked circuits controlling excitability gradients, allows for the building up of response sets. The flexibility of the representation is mediated by limbic structures such as the amygdala. Habituation, or the building up of representations, will include responses as well as stimuli, so that motor representations will be altered in the face of incongruity, as well as perceptual representations. Thus the posterior intrinsic system is sensitive to differences between past and present receptor stimulation, while the frontal intrinsic system is sensitive to differences between past and present states of the organism.

These differences in function are displayed by the results of ablation studies. Pribram's use of "plans" as a construct implies that an orderly execution of sequential responses is possible; frontal lesions produce failures of the necessary working memory. "Images" are coded representations of classes of stimuli; posterior lesions produce failures of memory storage. In this case, the organism has lost the finer differentiations which can be made between stimuli, and invariances represent larger classes of stimulus transformations. In this view, memory is not a unitary process, but one which is intrinsic to different parts of the brain. Memory function cannot be limited to the cortex because if subcortical components of a system are disturbed, the hierarchical relationship will be disrupted and the control of responses will be lost.

Motivational-emotional processes are also seen to be nonunitary in character. The motivational-like behavior which is characteristic of the internal core systems is dissociable from the perceptual-like preference or evaluative behavior which is dependent upon the external systems. Thus, an organism will respond in such a way as to avoid shock because he is disposed to execute the responses which will accomplish this. But he may prefer a certain stimulus object because a preference for this stimulus has been incorporated into the representation through prior experience.

Reinforcement is also seen to be of two kinds in this theory. The two kinds again correspond to the two divisions of the brain which have produced the other functional distinctions. The organism is reinforced, or gratified, when he can do what he intends to do. No further modification of the "plan" is necessary. The organism is fully instructed. This kind of reinforcement is a function of the frontal systems. Processes in the posterior systems, on the other hand, are reinforced, or satisfied, when no further modification of the "image" is necessary. The organism is fully informed. So the theory takes cognizance of cognitive reinforcement as well as primary reinforcement. All reinforcement depends upon unique specification of corrective change of the representation, either by current receptor stimulation or by current organismic states.

The promise of a new way to look at behavior has been demonstrated. The problem is to find what function can be demonstrated for particular systems and structures, although behavioral data are always in the forefront. The theory suggests that both intentional and discriminative behavior are subserved by both old and new structures, that both are determined by certain constraints such as the structures involved, the kind of experience provided, the situational variables involved in testing, etc. Therefore, neither structure nor environmental events alone will completely define the resulting processes. The kinds of distinctions which he finds meaningful are derived from certain properties of the central nervous system. New categories of behavior become necessary which cut across traditional ones.

It is made clear that this theory does not attempt to be rigorous in terms of all the data. Distinctions are blurred or sharpened, some structures are left without a function. Its great value lies in its utility as a framework for ordering a large mass of data, and as a guide for designing new experiments free from the ambiguities which older conceptions of the brain invited.

Arnold's Physiological Theory of Emotion.[4] In her analysis of emotion, Arnold states that complete understanding of any psychological phenomenon is possible only when the physiological mechanisms can be specified. For this reason, she presents a physiological theory of emotional behavior. However, her theory provides a function for all parts of the brain, since in order to describe the processes which underlie emotional behavior, she must make use of mechanisms which mediate behavior in general. For, in her view, "motivation-emotion" states are intimately involved in all behavior, with the brain acting as an interrelated structure.

Arnold begins with an analysis of experience and behavior; many classical psychological categories, such as emotion, perception, and memory, are included. Her analysis of these processes leads to some new neural categories, or, at least, some new suggestions about how the structures of the brain might be interrelated, in order to provide a mechanism for the functions studied. Defining emotion as the felt impulse to action after an organism has perceived and evaluated a stimulus, leads to the search for brain processes which could mediate such felt impulses to action. This involves specifying the antecedents: perceiving and appraising; and the consequences: the exciting of motor pathways which results in behavior.

The kinds of mechanisms which are looked for will be a function of how these psychological processes are defined. In Arnold's theory, perceiving involves the registration of stimulation, identification of stimuli, appraisal, and recall. Registration occurs in the association areas. Recognition, or identification of the stimulus, is mediated in secondary sensory areas next to the primary sensory areas. Appraisal is a momentary process, which gives rise to tendencies to approach or to avoid the stimulus situation.

---

[4] Discussion of Arnold's theory is based on Arnold (1960).

In this theory, a total sequence of behavior is comprised of the following steps. Given adequate stimulation from the environment, or from internal stimuli: stimulation is registered, identified, appraised, and recalled; various possible responses and outcomes of responses are imagined and appraised; various action patterns are initiated and appraised; and finally, a response is made, and its result appraised.

The first steps, from registration of stimulation through appraisal and recall, are mediated by the *hippocampal memory circuit*. Possible actions and their consequences, or expectancies based on sensory and motor memories, are served by the *amygdala imagination circuit*. Finally, the impulses to action are initiated by the hippocampus, which in turn activates cerebellar, frontal, and prefrontal structures which make up the *action circuit*. The importance of circuits as physiological substrates becomes clear when it is noted that appraisal is an integral part of perceiving, imagining, and responding, as outlined above. It is necessary to postulate mechanisms which allow for transformation of input and action patterns in terms of the organism's evaluation of similar patterns of activity. Each part of the sequence is influenced by the appraisal mechanism before the next step follows.

Let us consider in more detail the mechanisms proposed by Arnold, starting with the *hippocampal circuit* (see Fig. 2-7). Appraisal is based upon the stimulus characteristics of the situation and memory for previous situations. Impulses from sensory and association areas are relayed to limbic areas where appraisal takes place. The appraised stimulation excites the hippocampus, which relays impulses through the fornix and midbrain to thalamic nuclei and association areas. Recall of a particular pattern of excitation thus depends upon a cycle of neural activity which includes an evaluation of the stimulus. Appraisal is equally dependent upon memory since recall of similar situations is an important component of the appraisal process.

The memory circuit is thus thought to repeat the final path of the original sensory input. That is, after traveling the hippocampal circuit, stimulation reaches the thalamus again and is projected

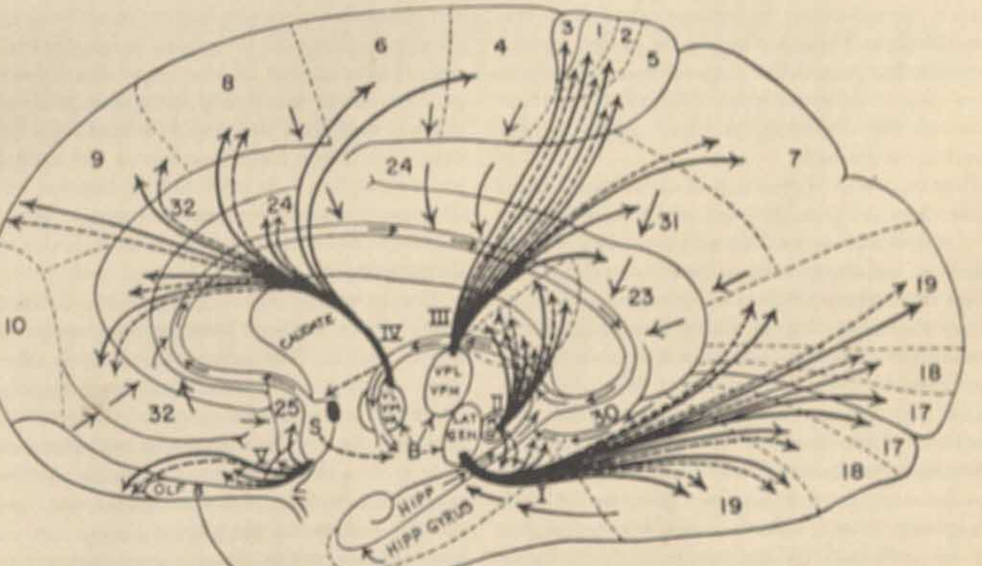


Fig. 2-7. The memory circuit reception and registration, recognition and recall. I visual system, II auditory system, III somesthetic system, IV motor system, V olfactory system, A cortical auditory area, B brain stem, HIPP hippocampus, LAT GEN lateral geniculate nucleus, MG medial geniculate nucleus, OLF B olfactory bulb, S septal area, VA, VM, VL portions of ventral nucleus of thalamus, VPL, VPM ventroposterior nucleus of thalamus. (From Arnold, 1960, Vol. 2, p. 69)

to the appropriate cortical area. But the path is not exactly the same: it is suggested that different sectors of the specific thalamic nuclei involved are excited when the stimulation comes from the hippocampal circuit rather than from afferent sensory tracts. This distinguishes the behavioral processes of identifying stimulation and recalling stimulation. Recall consists of a pattern of impulses arriving a second time in association areas after transmission through the memory circuit. It is the hippocampus which presumably supplies the patterning which preserves the appropriate sequences in memory.

The appraisal process which takes place in limbic areas contiguous to sensory and association areas is based on modality-specific regions which correspond to the mode-specific projection and association areas. Arnold divides the limbic lobe into sectors, on the basis of neuronographic data, which serve the appraisal of various kinds of stimulation. The limbic areas are delineated as follows: hippocampal gyrus for appraisal of visual and auditory stimuli; posterior cingulate gyrus for somesthetic stimuli; prepyriform and subcallosal gyri for olfactory stimuli; and posterior insula for gustatory stimuli. Stimulation giving rise to motor images corresponding to perceptions will be discussed below. These are appraised in the anterior cingulate gyrus and the anterior insula. Each of these limbic areas, when stimulated, relays impulses to the hippocampus where further activation is accomplished.

Direct afferent stimulation of these limbic areas is also possible. Arnold makes use of the discovery of a secondary sensory fiber system (see p. 41) which, in her interpretation, projects to the limbic cortex through midline and intralaminar nuclei of the thalamus. Simple evaluations of pleasantness versus unpleasantness can be mediated by the thalamic part of the system, but appraisal, which takes account of previous experience, is a function which depends upon limbic structure excitation. Furthermore, the hippocampal circuit may be active even if memory for similar patterns of impulses is not involved, since the hippocampus can be activated by excitation in other limbic areas.

Corresponding to sensory memory, perception, and appraisal, the theory proposes motor counterparts. Motor recall is localized in the dorsolateral frontal cortex, and frontal association areas are therefore analogous to posterior association areas in subserving recall (recognition plus appraisal based on experience) of motor memories. As stated above, motor images, or patterns of movements, which can be described as felt impulses to action, are appraised in the anterior cingulate gyrus of the limbic lobe.

These patterns of motor excitation are relayed through the hippocampal circuit in the same way as sensory impulses, so that motor images no less than sensory ones can be ordered, evaluated, and recalled. In summary, the recall of both sensory and motor memories is based on a circuit relaying through the hippocampus which terminates in the path of the original afferent input.

The second stage in sequences of neural activity consists of a process which Arnold labels imagination. If memory traces are aroused in new combinations in order to produce a meaningful structure, a process is involved which is mediated by the *amygdala circuit* (see Fig. 2-8). The sensory and motor images thus produced by selective rearrangement can be identified with expectancies, i.e., the organism projects images of what a situation will be like, or what outcome a response will effect, or what responses are possible in the situation. This is accomplished by reorganizing sensory and motor memories residing in sensory and motor association areas. This requires an anatomical circuit which receives afferent input from these association areas via limbic structures, and which projects excitation to association areas and limbic areas after some transformation of the excitation. Since imagination is activated in response to any situation demanding a new response, this circuit is also excited by internal need states, and a hypothalamic input is postulated.

The locus for the rearrangement of impulses which is the basis of planning and imagining is the amygdala. The amygdala receives afferents from association areas through limbic areas which, in turn, excite intrinsic thalamic nuclei which project to the frontal and posterior association areas. Thus, imagining possible *actions* is based on excitation of the amygdala, the medialis dorsalis nucleus of the thalamus, and the dorsolateral frontal cortex. *Sensory* imagination is based on afferents from limbic areas to the amygdala, pulvinar and posterior nuclei of the thalamus, and the posterior association cortex.

But images are also appraised, which involves the appraisal circuit described above. Motor images, or possible actions, are appraised when the

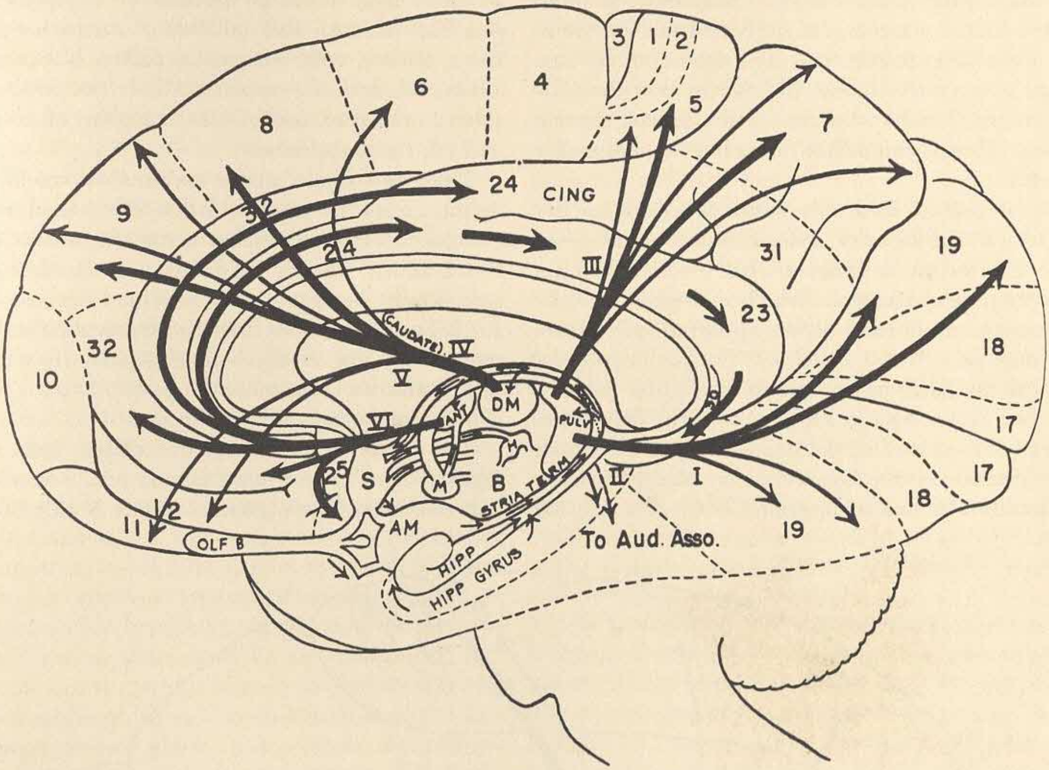


Fig. 2-8. The imagination circuit and the recall of success and failure. Imagination of various modalities: I visual, II auditory, III somesthetic and gustatory, IV motor, V olfactory, VI recall of success or failure, AM amygdala, ANT anterior thalamic nuclei, B brain stem, CING cingulate gyrus, DM dorsomedial thalamic nucleus, H habenula, HIPP hippocampus, M mammillary body, OLF olfactory bulb, PULV pulvinar, S septal area, STRIA TERM stria terminalis. (From Arnold, 1960, Vol. 2, p. 72)

outcome of the contemplated action is recalled and evaluated. This is accomplished by excitation of the hippocampus by the amygdala; the hippocampus activates a circuit through the hypothalamus and anterior thalamic nucleus, to the cingulate gyrus (frontal limbic area) where motor images are appraised. Sensory images, or perceptions of the consequences of actions, are similarly appraised in the appropriate limbic area through the hippocampal circuit. Thus, the amygdala, when activated, produces impulses to imagine what will happen, and impulses to respond in certain ways. These are evaluated by the appraisal system, just as are sense impressions and actions. The sequence of activity thus far can be summarized as follows: after perception, what was perceived and whether it was liked or disliked is recalled by means of the hippocampal circuit. The tendency to imagine consequent sensory experiences and possible responses relevant to the situation is mediated by the amygdala circuit. Appraisal of experience and imagination is served by the appraisal system, which includes midbrain structures and the limbic cortex. Any object that is perceived and appraised will lead to the recall of appropriate memories and will induce sensory and motor images. If sensory stimulation is very intense, evaluation will be mediated by the thalamus before stimulation reaches the limbic cortex. In this case, feeling and immediate action will result before the impulses to recall and imagine (provided by the hippocampal and amygdala circuits) are activated.

It has been assumed that every appraisal sets up impulses to action. These can be inhibited until the recall and appraisal of the impulse has been completed as described above. When this process has taken place, the initiation of response depends upon another circuit which comprises the final stage in the sequence of activity from reception of stimulation to response. The *action circuit* (see Fig. 2-9) is based on relays from the hippocampus to the mammillary bodies via the fornix, then to the midbrain, cerebellum, frontal lobe, and motor cortex.

The frontal lobe has been specified in this theory as the locus of motor memories. These can be described as a sense of what is to be done, or patterns of movement. Which movement will be initiated will depend on the appraisal system, but it must be initiated somehow. The problem of the translation of sensory patterns into motor patterns is met again. Arnold suggests that the hippocampus is the center for the transformation of sensory information into motor actions, or, simply, sensory impulses into motor impulses. Choice of action will depend upon repeated rounds of appraisal of possible actions, and possible consequences of those actions, with recurrent recalling of relevant situations, and the appraisal that was made of them in the past, and of different plans of action, and their past appraisals.

Thus, the hippocampus induces the recall of memories and the impulse to act. When the hippocampal mechanism reaches a certain level of activity, it will initiate the action pattern. But the action must be organized somewhere because the frontal cortex receives fully patterned impulses for movement. The cerebellum, classically described as a center for organization of motor patterns, fulfills the same purpose in this theory.

The impulses for action are relayed from the hippocampus to the cerebellum, which receives afferent sensory input, and therefore is able to set

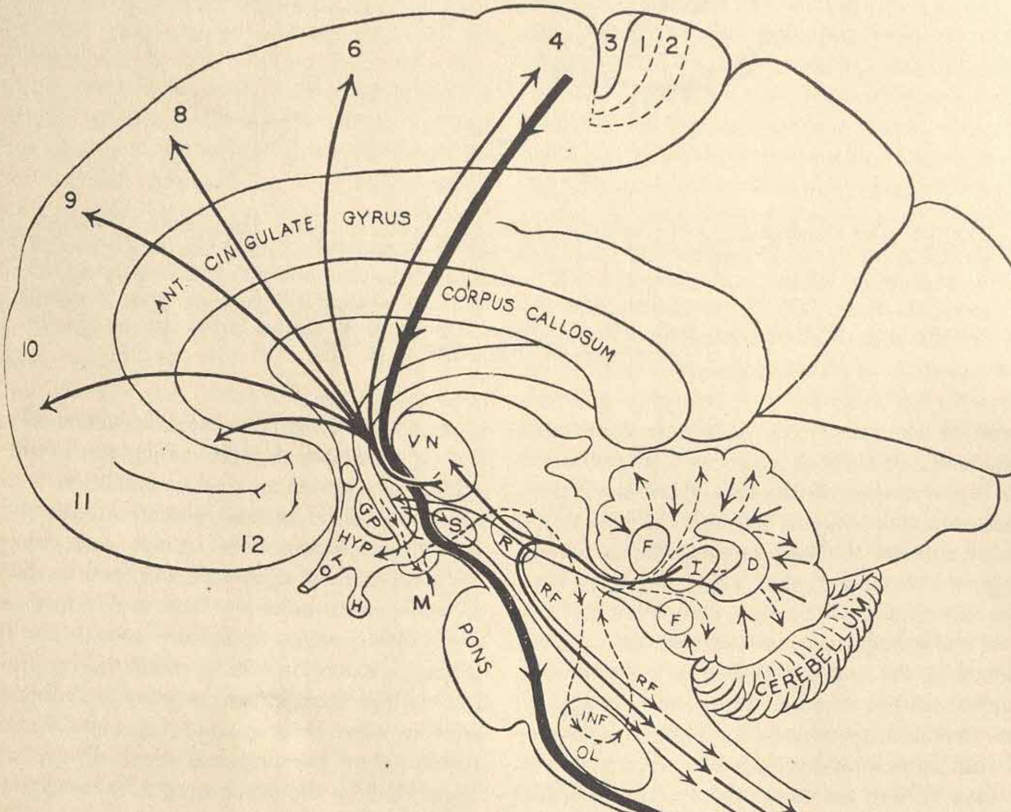


Fig. 2-9. The action circuit. D, F, I nuclei of the cerebellum, GP globus pallidus, H pituitary gland, HYP hypothalamus, INF OL inferior olive, M mammillary body, OT optic tract, R, S midbrain nuclei, RF reticular formation, VN ventral thalamic nucleus. (From Arnold, 1960, Vol. 2, p. 83)

up the delicate adjustments that make up patterns of movement. This action pattern is relayed via the ventral nuclei of the thalamus to the frontal lobe and also via the globus pallidus to the extrapyramidal pathways and to the hypothalamus. The cerebellum also sends descending relays to the spinal cord, as part of the descending reticular system. The cerebellum permits avalanche conduction of impulses, so that the action pattern is amplified as well as relayed from the hippocampus.

After the action pattern is organized, it must still be initiated before overt movement is possible. Intentions, or felt impulses to action, are experienced when the prefrontal areas are active, and they activate patterns of impulses in the frontal motor areas. This results in the experience of an urge to a specific coordinated action. As described above, this will be appraised in the frontal limbic area, and it is dependent upon a connection with the appraisal system after the pattern of movement has been fully organized in the cerebellum. That is, there must be an appraisal of the specific action pattern after it has been set up in the cerebellum, as well as an appraisal of the impulse to action which occurs before the pattern has been organized in the cerebellum. This post-cerebellar connection with the appraisal system is presumably mediated in the thalamus. The cerebellum projects to the midline thalamic nuclei, which relay to the hypothalamus and then the cingulate gyrus. Unlocalized feelings of pleasure or pain can be mediated by the thalamic portion of the system, but the evaluation of the appropriateness of a particular action depends upon the limbic cortex. This final appraisal will activate the hippocampus and the action circuit which finally excites the frontal areas and motor cortex, It follows that motor cells do not determine the specific muscular pattern; while they mediate the final position of the effectors, the pattern of contractions has been previously set up in the cerebellum.

Corollary to the action pattern are involuntary autonomic changes, including characteristic emotional expressions, endocrine changes, and background movements of the whole body. This is possible because the action circuit relays to both pyramidal and extrapyramidal tracts. The latter mediate body tonus changes via a system from the premotor area to the spinal motor tracts through the globus pallidus, and autonomic changes by a relay to the hypothalamus. These are the effects which investigators of emotion have hitherto considered in an isolated way.

The theory has described a sequence of physiological events underlying psychological processes from perception through appraisal to action. These events are subserved by a series of relays from cortical to subcortical structures, and from sensory cortex to motor cortex by way of subcortical structures. The symmetry of the theory is evident when it is noted that there is registration of actions as well as sensations, and there is motor imagination as well as images of sensory experience. There is appraisal of both motor events and sensory events. Impulses to action are felt when relays from the hippocampus and cerebellum arrive in premotor areas. Actions are initiated when combined impulses from the cerebellum and premotor cortex activate the motor cortex. Frontal areas, which register impulses to action, make it possible for motor images to be appraised and recalled in the same way that sense data are.

The problem of how actions are inhibited can be handled without the addition of any new mechanisms. If the pattern of action is to be stopped, a new appraisal of the situation is necessary. This will occur in the same way that any motor pattern is appraised: by perception of the situation and its appraisal, and motor memory of the action and its appraisal. If the evaluation is such that it sets up tendencies to withdraw, the on-going response will cease. Action patterns may also be inhibited by surprise or trauma. This suggests that disorganization of the circuits is another cause of cessation of action. In general, one activity is stopped, and another behavior sequence is started because the first one is appraised as no longer suitable. Eating, for example, will stop when afferent impulses from internal receptors and from external stimuli are appraised as unpleasant or unsuitable. This depends on activity in the anterior cingulate gyrus, hippocampus, cerebellum, frontal area, and motor cortex. The same relays and structures are involved as in initiation of action; only a new sequence is involved.

In summary, Arnold's theory proposes that the brain is made up of systems subserving perceptual, memory, appraisal, and motor functions, which are outgrowths of lower functional systems, mediating the same processes on a grosser level. Primitive sensation and feeling are served by thalamic structures, but for the continuous rounds of recalled perceptions and appraisals, cortical structures are necessary. The bridge between sensory input and action is in the hippocampus.

Describing the memory, appraisal, imagination, and action circuits separately and piecemeal can not do justice to this conception of brain function. If these processes can be envisaged as occurring as rounds of activity, each round activating the next until some level of activity in the hippocampus is reached which triggers the motor cortex to effect predetermined patterns of action, the theory may be seen to have great explanatory and heuristic value. A solution to the problem of how sensory information is transformed into responses has been proposed by Arnold in anatomical detail not attempted previously.

SUMMARY OF THEORIES

Let us now see how these three theorists treat some of the classical problems in psychology. It might seem surprising that they have made use of constructs such as plans, images, intention, and evaluation. But they have clearly felt the need to discuss behavior in molar terms and have been willing to postulate structures and mechanisms which could serve such processes, rather than to limit physiological explanations to central states presumed to underlie discrete sensations or responses. While none of the theorists would claim to have solved the problems of physiological psychology, each has made an effort to show that it should be possible to describe these activities of the organism without resorting to the *deus ex machina* of some neurophysiologists (Eccles, 1953). All would reject the sort of simple determinism that claims that every action, whether voluntary or not, is the result of some one stimulus. Yet all would hope that the kind of behavior which includes the goals of the organism as well as compulsions to action, can ultimately be explained within the framework of a physiological theory. We will now turn to the communalities of thinking which characterize these current attempts to define the physiological basis of behavior.

First, all are united in rejecting a simple notion of transcortical reflexes as the bridge between sensory and motor impulses. All go beyond Lashley in trying to specify anatomically or physiologically what the bridging mechanism might be like. Some sort of association areas, distinct from primary projection areas, which act upon sensory input, are made use of. In each theory, some modality-specificity is assumed for association areas as well as primary sensory areas.

The idea of an activating system, distinct from a system for receiving or transmitting information, is incorporated in all of the theories. Hebb and Pribram accept the concept of midbrain and thalamic ascending and descending systems very much in the form in which the original discoveries were made. Arnold proposes the appraisal system as prior, and includes the formations which have been called arousal systems within it. In her theory, as well, this system when activated leads to impulses to action.

The importance of motor memory, and the presetting of motor response patterns, is emphasized in all of these theories. Pribram's intentions, Hebb's expectancies, and Arnold's motor images all are pointed to as determinants of response which are as important as the nature of the sensory input, or memory. Pribram and Arnold localize registration and control of patterns of response in the frontal cortex. While Hebb's theory is not anatomically definite on this point, the cell assemblies representing motor patterns of response will obviously include cells found in motor areas. The importance of sequences of behavior is stressed by all theories: the guiding of one set of responses by the outcome of previous responses is a necessary assumption. More simply, the central control of behavior is stressed rather than control by sensory input.

What Hebb labels a cell assembly, and Pribram calls a representation or image, corresponds to Arnold's notion of recognition or recall processes. It is presumed that these act upon sensory input in some way, and this is construed in each theory as efferent control of sensory information.

Arnold and Pribram stress the hierarchical nature of brain processes. While the fundamental processes are the same, higher centers control activity in lower centers in continuing cycles of testing, or appraising, providing feedback from the monitoring process to neural activity.

Experience is seen as an important determinant of the on-going central processes which determine the fate of the input. The building up of cell assemblies, or neural representations, or appraised perceptions, depends upon prior stimulation and appreciation of its outcome. Sensation, or the arrival of sensory input to cortical projection areas is a different mode of response from perception, which involves organization of the input, although Pribram (if restricted to these terms) might suggest that perception precedes sensation!

Motivation, or drive, is also handled in similar fashion in these theories. Both motivation and reinforcement are thought to have both cognitive and vegetative aspects. Drives and reinforcement will depend upon the level of arousal of the organism. Central core mechanisms in all theories are important in motivational aspects of behavior, but the influence of cortical activity upon this kind of behavior points to the importance of sensory control of drives.

Memory is not a unitary affair in these theories. Not only is it modality-specific in its sensory manifestations, but the importance of memory for motor patterns as well is stressed. In all cases, it is treated as a reactivation of neural patterns, and some neural patterns may represent serial order as well as specific information. Learning, in each theory, appears to be a special case of brain function, in which the same outcome is achieved from the interaction of sensory input and on-going central states. Pribram and Arnold point to subcortical mechanisms as the important ones in the actual learning process, as distinct from transformations of input which are presumably cortical.

One could show, for each of these theories in turn, that there are experimental findings which are difficult to explain. Such a procedure would have only limited usefulness, since none of the theories is so rigidly constructed that its continued life would be greatly threatened thereby. It is hoped that uncritical summaries will permit an estimate of the possible usefulness of these different points of view.

By stressing the common aspects of these theories, we can arrive at a representation of how the organism is thought to function on a physiological level. In general, there are sensory systems, motor systems, and activating systems in the brain: each part reciprocally incorporates the activity of the other parts into its own activity. These sectors are organized in levels in a hierarchical system. Mutual facilitation of these systems is mediated by subcortical structures such as the hippocampus, in such a way that patterns of neural organization can be formed and transformed into behavior.

APPROACHES TO PHYSIOLOGICAL PSYCHOLOGY

The theories just presented exemplify three ways of approaching the problem of constructing a theory of physiological psychology. Hebb and Arnold are alike in that both begin with categories which serve to organize behavior, and then attempt to suggest the organismic correlates of such behavior. Beyond this, they differ greatly. The behavioral data which Hebb considers in his theory cover a wide range of situations and require that the theory encompass observations and provide explanations concerning processes such as perception, learning, motivation, intelligence, emotion, and development. Yet in the explanation of a given behavioral event, Arnold offers a much more complex analysis of the physiological events involved. Arnold pays little attention to problems at the cellular level but brings together experiments on various aspects of localization, and illustrates strikingly the detail which can be offered in making such explanations on the basis of available evidence. Hebb sees the major task not as constructing such geographical correlations but in providing explanations of the cellular mechanisms that are active in behavior. His approach, and his revisions of theory in the light of physiological facts, make his work the outstanding example of a physiologically sensitive approach to neuropsychological theory.

Pribram can be contrasted with both Hebb and Arnold. His theory begins with an analysis of structure rather than function. The physiological features and microscopic anatomy of various structures are considered as well in framing hypotheses about function, so that Pribram uses what is known of possible mechanisms and anatomical relationships to provide a theory of psychology.

Is either of these ways of proceeding to be preferred? Several alternative conclusions may be offered:

1. Psychologists are interested in behavior: physiological psychology properly will be a study of the structural correlates of behavior. Therefore, we must begin with *behavior*.
2. Psychologists are interested in behavior: physiological psychology will provide information about behavior only if we start with *physiology* and *anatomy* and find the behavioral correlates of structure. The behavioral categories that emerge may be new and useful.
3. Unless one doubts the unity of science, it does not matter at which level we begin. If we find the appropriate constructs to start from at either the behavioral or the structural level, we have some chance of finding the corresponding units in the other domain.

OTHER THEORETICAL APPROACHES

Many others have proposed neuropsychological hypotheses to cover narrower fields of inquiry. We can note again the distinction between theories which begin with behavior and those that begin with structure. Stellar's (1954) theory of motivation can be considered a psychology-oriented approach as are the more general views of Hebb and Arnold. Of course, the ideas spring from a study of both brain and behavior, but the theory is about the physiology of motivation, not the psychology of the hypothalamus. On the other hand, a restricted theory such as any of the various "theories of frontal lobe function" is just that: an attempt to determine the role in behavior of this structurally defined unit.

Our earlier discussion of the nature of theories of physiological psychology leads us to omit from consideration some treatments of behavior which others might include. As we have tried to indicate, we see no reason for thinking that the mathematical biophysical approach of Rashevsky and his coworkers (1948), for example, is more physiological than the theories of Hull (1943) or Bush and Mosteller (1955). Rashevsky begins with some specifications of the properties of neurons, but Hull clearly was influenced by the physiology of his day, and Bush and Mosteller state that a "variety of physiological models might provide" the physical basis for their assumption that organisms behave as if they possessed probability mechanisms. However, authors of these points of view would probably be equally unconcerned if their physiological assumptions were proved incorrect, as long as the behavioral predictions were supported. By contrast, the theories we have discussed, and the more specific physiological theories mentioned above, are presented with the belief that the brain-behavior correlations are an integral part of the hypotheses being proposed.

RECURRENT ISSUES IN PHYSIOLOGICAL PHYCHOLOGY

Questions still unanswered may be more evident than are conclusions in our summary of data. Nevertheless, it is clear that remarkable strides have been made in the field in the last fifteen years. Have these experimental advances served to settle any of the issues upon which opinions have been militantly divided? Let us consider two of these classical problems.

Mass Action Versus Localization. Many treatments of the question of cerebral function suggest that these labels summarize diametrically opposed points of view, and that resolution of this controversy would be the most important advance in the study of neuropsychology. Neither of these is true.

"Mass action" refers to the principle that all parts of a certain structure or area are of importance in the service of some specific function, and that the adequacy of this function will be directly related to the amount of such tissue available. But this fact may exist along with localization of function. Mass action *will* be seen if one defines function grossly enough and studies a large enough area. Mass action *may* be seen within some structure which has been found to have a specific function.

Those who are remembered because of their mass-action point of view have delimited the areas within which they thought such a concept applied, and they have thus by exclusion indicated the extent to which they believed in localization. On the other hand, the most prominent localizers, the phrenologists, believed in mass action within the areas which corresponded to each trait; and modern students of precisely defined projection systems are not surprised at overlap between the receptive fields of neurons, which provides the basis for a mass-action relationship between structure and function.

Teuber's results indicate, furthermore, that it is inappropriate to expect either mass action or localization to serve as an all-inclusive principle. Blindness of part of the visual field may arise only after lesions of the corresponding part of the visual projection field; some visual functions, such as the critical flicker frequency, are altered after lesions anywhere in the visual system, while yet other kinds of behavior involving vision, such as the ability to discover hidden figures in drawings, are affected by any cortical destruction.

Yet, evidence has been presented in the preceding pages which indicates the accuracy of some claims of localization in several mammalian species, both in various parts of the brain, and within parts of the cerebral cortex. It must be remembered that evidence of mass action is obtained as a negative result. Evidence of localization impresses us more, and probably properly so. Demonstration of localization of function requires accurate identification of both a structure and a function. Some of the controversies that involved mass action versus localization are better understood as disagreements over unity versus multiplicity of an alleged function rather than as arguments about structure-function relationships per se.

We may also question the importance of localization of function as the term is generally used. Contiguous localization may appear to be a sufficient condition, but it is hardly a necessary one, for allowing the assertion that a certain behavioral function exists. The phrenological trait of "amativeness" may vary reliably among people as an independent dimension of personality even if it were to be found to be based not on a given area but upon a system of neurons widely dispersed in the brain.

Thus, demonstrations of localization serve to confirm hypotheses about the organization of both structure and function, and their correlation. But localization studies as such give us no information about mechanism, about what is going on, rather than where. Recent trends in investigation have turned toward the study of mechanism, to what appears to be the preferred goal of neuropsychology. The fact that the primary projection area for vision is located in one sector in the occipital cortex has been convenient for studies of ablation effects, but (particularly if brain activity consists solely of synaptic connections between individual neurons) we could claim that the unitary nature of occipital visual cortex should be of interest only to the anatomist. Psychophysiologists want to know what takes place at each cell, whether they are clustered together or randomly dispersed throughout the brain.

The Physiological Nature of the Behavioral Unit. Displeasure with the concept of the *S-R* "cortical reflex" is almost universal among those who present theoretical discussions of brain activity. The objections and alternatives are worth some notice.

The objection that one does not have a *simple* chain of sensory neurons to projection area, followed by a link to association area, to motor area, and to a set of motor neurons: this in itself is not evidence against the accuracy or adequacy of an explanation in terms of *S-R* terminology. For even if the set of intermediate steps is more complex and involves more loci than are indicated above, it might still be possible to follow through the brain the excitation set off by one stimulus. The further objections to the cortical reflex approach are three-fold:

1. The organism will differ from time to time, and the state and nature of the organism will determine the fate of the stimulus. This argument incorporates also the fact that the brain is active even before a stimulus is presented; and it includes further the fact that even without a stimulus, responses will be emitted. Blind attempts to follow *S-R* chains would lead to unexplained divergences in the patterns of excitation following a given stimulus unless the moment-to-moment differences in the organism were considered. If this were the only objection, one could adopt the *S-O-R* outline, and the problem would be resolved.

2. The *S-R* (or *S-O-R*) concept does not accurately reflect the actual unit of physiological activity. Specifically, Pribram, in espousing the TOTE unit, would feel that a complex sequence of afferent and efferent activity would be interposed between the initial stimulus and the eventual response; some of the intercalated efferent activity might actually be motor activity, some of it would be efferent activity which changes the state of receptors and thus controls input. The individual chain from an afferent impulse to the first consequent efferent impulse is only *part* of the real unit of physiological activity and behavior.

Livingstone (1962) incorporates these arguments in the proposal that it is not appropriate to talk either of linear cause-effect relationships (*S-R* arcs) or of interactions between such sequences. Instead, realizing the complex interdependence of the neural systems before and after a stimulus is presented, he suggests we speak of *transactions*, and he hopes an appropriate mathematical tool will be developed for description and prediction of such events.

3. The third objection is perhaps the most serious one in that it calls into question the tacit assumption that all brain activity worth noting is composed of responses of individual cells. We have mentioned above some of the evidence that artificially imposed direct current fields may affect behavior; it has been shown directly that potential differences arising from neural structures do exist between various cortical points, and between cortex and subcortex, and that they are affected by sensory stimulation; it has also been shown that such fields can change the excitability of neurons (Köhler, 1951; O'Leary and Goldring, 1959). It does not necessarily follow that in the

normal organism these potential fields have any significance (see discussion in Brazier, 1961, pp. 224-232), but if field forces are truly of importance then the *S-R* concept is clearly wrong as a physiological model. Furthermore, localization studies would recover some of their importance, since the geographical relationships of cells would be as important as their synaptic connections.

SUMMARY

We have surveyed theories and data in physiological psychology in the hope that some notion of the complexity of the field has been conveyed. Even when the focus is restricted to neuropsychology, it is clear that many fields of inquiry have contributed to the knowledge we have of the physiological bases of behavior.

It is obvious that new data and new theories are necessary, and will be forthcoming. We think it is less obvious but equally true that in the future all psychology will be physiological psychology, and the excitement of understanding as well as predicting behavior will be shared by all psychologists.

REFERENCES

Adams, H. E., and Lewis, D. J. Electroconvulsive shock, retrograde amnesia, and competing responses. *J. comp. physiol. Psychol.*, 1962, **55**, 299-301.

Ades, H. W., and Raab, D. H. Effect of preoccipital and temporal decortication on learned visual discrimination in monkey. *J. Neurophysiol.*, 1949, **12**, 101-108.

Adey, W. R., Walter, D. O., and Hendrix, C. E. Computer techniques in correlation and spectral analyses of cerebral slow waves during discriminative behavior. *Exp. Neurol.*, 1961, **3**, 501-524.

Anand, B. K., and Brobeck, J. R. Hypothalamic control of food intake. *Yale J. Biol. Med.*, 1951, **24**, 123-140.

Arnold, Magda. *Emotion and Personality.* New York: Columbia University Press, 1960. 2 vols.

Ashby, W. R. *Design for a Brain: The Origin of Adaptive Behavior.* (2d ed.) New York: John Wiley & Sons, Inc., 1960.

Bates, J. A. V., and Ettlinger, G. Posterior biparietal ablations in the monkey. *A.M.A. Arch. Neurol.*, 1960, **3**, 177-192.

Battig, K., Rosvold, H. E., and Mishkin, M. Comparison of the effects of frontal and caudate lesions on delayed response and alternation in monkeys. *J. comp. physiol. Psychol.*, 1960, **53**, 400-404.

———, ———, and ———. Comparison of the effects of frontal and caudate lesions on discrimination learning in monkeys. *J. comp. physiol. Psychol.*, 1962, **55**, 458-463.

Beach, F. A. Neural and chemical regulation of behavior. In H. Harlow and C. N. Woolsey (eds.), *Biological and Biochemical Bases of Behavior.* Madison: University of Wisconsin Press, 1958, pp. 263-284.

Blake, L. The effects of lesions of the superior colliculus on brightness and pattern discriminations in the cat. *J. comp. physiol. Psychol.*, 1959, **52**, 272-278.

Blum, J. S. Cortical organization in somesthesis: Effects of lesions in posterior associative cortex on somatosensory functions in Macaca mulatta. *Comp. Psychol. Monogr.*, 1951, **20**, 219-249.

Bodian, D. The generalized vertebrate neuron. *Science*, 1962, **137**, 323-326.

Brady, J. V., and Conrad, D. Some effects of brain stimulation on timing behavior. *J. exper. Anal. Behavior.*, 1960, **3**, 93-106.

Brazier, M. A. B. (ed.) *Brain and Behavior.* Vol. 1. Washington, D.C.: Amer. Inst. Biol. Sci., 1961.

Breen, R. A., and McGaugh, J. L. Facilitation of maze learning with posttrial injection of picrotoxin. *J. comp. physiol. Psychol.*, 1961, **54**, 498-501.

Brobeck, J. R., Tepperman, J., and Long, C. N. H. Experimental hypothalamic hyperphagia in the albino rat. *Yale J. Biol. Med.*, 1943, **15**, 831-853.

Brodie, D. A., Malis, J. L., Moreno, O. M., and Boren, J. J. Nonreversibility of the appetitive characteristics of intracranial stimulation. *Amer. J. Physiol.*, 1960, **199**, 707-709.

Brown, T., Rosvold, H. E., and Mishkin, M. Olfactory discrimination after temporal lobe lesions in monkey. *J. comp. physiol. Psychol.*, 1963, **56**, 190-195.

Brush, E., Mishkin, M., and Rosvold, H. E. Effects of object preferences and aversions on discrimination learning in monkeys with frontal lesions. *J. comp. physiol. Psychol.*, 1961, **54**, 319-325.

Buchwald, N. A., Rakic, L., Wyers, E. J., Hull, C., and Heuser, G. Integration of visual impulses and the "caudate loop." *Exp. Neurol.*, 1962, **5**, 1-20.

Bureš, J., Burešová, O., and Záharóva, A. Conditioned reflexes and Leaõs spreading depression. *J. comp. physiol. Psychol.*, 1958, **51**, 263-268.

Bush, R. R., and Mosteller, F. *Stochastic Models for Learning.* New York: John Wiley & Sons, Inc., 1955.

BUTTER, C. M., MISHKIN, M., AND ROSVOLD, H. E. Conditioning and extinction of a food-rewarded response after selective ablations of frontal cortex in rhesus monkeys. *Exp. Neurol.*, 1963, **7**, 65-75.

CHOW, K. L. Conditions influencing the recovery of visual discriminative habits in monkeys following temporal neocortical ablations. *J. comp. physiol. Psychol.*, 1952, **45**, 430-437.

———. Lack of behavioral effects following destruction of some thalamic association nuclei in monkey. *A.M.A. Arch. Neurol. Psychiat.*, 1954, **71**, 762-771.

———. Anatomical and electrographical analysis of temporal neocortex in relation to visual discrimination learning in monkeys. In J. F. Delafresnaye (ed.), *Brain Mechanisms and Learning*. Springfield, Ill.: Charles C Thomas, Publisher, 1961, pp. 507-525. (*a*)

———. Changes of brain electropotentials during visual discrimination learning in monkey. *J. Neurophysiol.*, 1961, **24**, 377-390. (*b*)

———. Effect of local electrographic afterdischarges on visual learning and retention in monkey. *J. Neurophysiol.*, 1961, **24**, 391-400. (*c*)

———, DEMENT, W. C., AND JOHN, E. R. Conditioned electrocorticographic potentials and behavioral avoidance response in cats. *J. Neurophysiol.*, 1957, **20**, 482-493.

———, AND SURVIS, J. Retention of overlearned visual habit after cortical ablation in monkey. *A.M.A. Arch. Neurol. Psychiat.*, 1958, **79**, 640-646.

CLARK, G. Area 3, a critical evaluation of cytoarchitectonic studies. *J. comp. Neurol.*, 1962, **119**, 21-24.

COLE, J., AND GLEES, P. Effects of small lesions in sensory cortex in trained monkeys. *J. Neurophysiol.*, 1954, **17**, 1-13.

COONS, E. E., AND MILLER, N. E. Conflict versus consolidation of memory traces to explain "retrograde amnesia" produced by ECS. *J. comp. physiol. Psychol.*, 1960, **53**, 524-531.

COTTON, J. W., AND LEWIS, D. J. Effect of intertrial interval on acquisition and extinction of a running response. *J. exp. Psychol.*, 1957, **54**, 15-20.

COWEY, A. Visual field defects in monkeys. *Nature*, 1962, **193**, 302.

DAVIS, R. C. Experiment and theory in physiological psychology. In S. Koch (ed.), *Psychology: A Study of a Science*. Vol. 4. New York: McGraw-Hill Book Company, Inc., 1962, pp. 242-279.

DEAN, W. H., AND DAVIS, G. D. Behavior changes following caudate lesions in rhesus monkeys. *J. Neurophysiol.*, 1959, **22**, 525-537.

DELGADO, J. M. R., ROBERTS, W. W., AND MILLER, N. E. Learning motivated by electrical stimulation of the brain. *Amer. J. Physiol.*, 1954, **179**, 587-593.

DENNY-BROWN, D. Motor mechanisms—introduction: the general principles of motor integration. In J. Field (ed.), *Handbook of Physiology: Neurophysiology*. Vol. 2. Washington, D.C.: Amer. Physiol. Soc., 1960, pp. 781-796.

DEUTSCH, J. A. *The Structural Basis of Behavior*. Chicago: University of Chicago Press, 1960.

DIAMOND, I. T., AND CHOW, K. L. Biological psychology. In S. Koch (ed.), *Psychology: A Study of a Science*. Vol. 4. New York: McGraw-Hill Book Company, Inc., 1962, pp. 159-241.

DOTY, R. W., AND RUTLEDGE, L. T. "Generalization" between cortically and peripherally applied stimuli eliciting conditioned responses. *J. Neurophysiol.*, 1959, **22**, 428-435.

———, ———, AND LARSEN, R. M. Conditioned reflexes established to electrical stimulation of cat cerebral cortex. *J. Neurophysiol.*, 1956, **19**, 401-415.

DOWNER, J. L. DEC. Interhemispheric integration in the visual system. In V. B. Mountcastle (ed.), *Interhemispheric Relations and Cerebral Dominance*. Baltimore: John Hopkins Press, 1962, pp. 87-100.

DUNCAN, C. P. The retroactive effect of electroshock on learning. *J. comp. physiol. Psychol.*, 1949, **42**, 32-44.

EBNER, F. F., AND MYERS, R. E. Inter- and intrahemispheric transmission of tactile gnosis in normal and corpus callosum-sectioned monkeys. *Fed. Proc.*, 1960, **19**, 292.

ECCLES, J. C. *The Neurophysiological Basis of Mind: The Principles of Neurophysiology*. Oxford: Clarendon Press, 1953.

ENGLISH, H. B., AND ENGLISH, A. C. *A Comprehensive Dictionary of Psychological and Psychoanalytical Terms*. New York: Longmans, Green & Co., Inc., 1958.

ETTLINGER, G., AND KALSBECK, J. E. Changes in tactile discrimination and in visual reaching after successive and simultaneous bilateral posterior parietal ablations in the monkey. *J. Neurol. Neurosurg. Psychiat.*, 1962, **25**, 256-268.

———, AND WEGENER, J. Somaesthetic alternation, discrimination, and orientation after frontal and parietal lesions in monkey. *Quart. J. exp. Psychol.*, 1958, **10**, 177-186.

FLECHSIG, P. E. Brain physiology and theories of volition. In G. von Bonin (transl.), *Some Papers on the Cerebral Cortex*. Springfield, Ill.: Charles C Thomas, Publisher, 1960, pp. 181-200.

French, G. M. Spatial discontiguity in monkeys with lesions of the frontal cortex. *Science,* 1962, **135,** 728-729.

Gastaut, H. The role of the reticular formation in establishing conditioned reactions. In H. H. Jasper et al. (eds.), *Reticular Formation of the Brain.* Boston: Little, Brown & Company, 1958, pp. 561-589.

Glees, P., and Cole, J. Comparisons of anterior parietal lesions (areas 3, 1, 2) with posterior parietal lesions (areas 5 and 7) in trained monkeys. *C. R. V^e Inter. Neurol. Congr.,* 1954, **3,** 362-367.

Goldberg, J. M., and Neff, W. D. Frequency discrimination after bilateral section of the brachium of the inferior colliculus. *J. comp. Neurol.,* 1961, **116,** 265-290.

Graff, H., and Stellar, E. Hyperphagia, obesity, and finickiness. *J. comp. physiol. Psychol.,* 1962, **55,** 418-424.

Grastyán, E. The significance of the earliest manifestations of conditioning in the mechanism of learning. In J. F. Delafresnaye (ed.), *Brain Mechanisms and Learning.* Springfield, Ill.: Charles C Thomas, Publisher, 1961, pp. 243-263.

Greenblatt, M., and Solomon, H. C. Studies of lobotomy. *Res. Publ. Assoc. nerv. ment. Dis.,* 1958, **36,** 19-34.

Gross, C. G., and Weiskrantz, L. Evidence for dissociation of impairment on auditory discrimination and delayed response following lateral frontal lesions in monkeys. *Exp. Neurol.,* 1962, **5,** 453-476.

Grosser, G. S., and Harrison, J. M. Behavioral interaction between stimulated cortical points. *J. comp. physiol. Psychol.,* 1960, **53,** 229-233.

Halstead, W. C. *Brain and Intelligence.* Chicago: University of Chicago Press, 1947.

Harlow, H. F. Primate Learning. In C. P. Stone (ed.), *Comparative Psychology.* (3d ed.) Englewood Cliffs, N.J.: Prentice-Hall, Inc. 1951, pp. 183-238.

Hebb, D. O. *The Organization of Behavior.* New York: John Wiley & Sons, Inc., 1949.

———. Drives and the C.N.S. (conceptual nervous system). *Psychol.* Rev., 1955, **62,** 243-254.

———. *A Textbook of Psychology.* Philadelphia: W. B. Saunders Company, 1958.

———. The semiautonomous process: Its nature and nurture. *Amer. Psychologist,* 1963, **18,** 16-27.

Helson, H. Adaptation-level as a basis for a quantitative theory of frames of reference. *Psychol. Rev.,* 1948, **55,** 297-313.

Heriot, J. T., and Coleman, P. D. The effect of electroconvulsive shock on retention of a modified "one-trial" conditioned avoidance. *J. comp. physiol. Psychol.,* 1962, **55,** 1082-1084.

Hernández-Peón, R., Scherrer, H., and Jouvet, M. Modification of electric activity in cochlear nucleus during attention in unanesthetized cats. *Science,* 1956, **123,** 331-332.

Hess, W. R. *Diencephalon: Autonomic and Extrapyramidal Functions.* New York: Grune & Stratton, Inc., 1954.

Hoebel, B. G., and Teitelbaum, P. Hypothalamic control of feeding and self-stimulation. *Science,* 1961, **135,** 375-377.

Hubel, D. H., and Wiesel, T. N. Receptive fields of single neurons in the cat's striate cortex. *J. Physiol.,* 1959, **148,** 574-591.

Hugelin, A., Dumont, S., and Paillas, N. Tympanic muscles and control of auditory input during arousal. *Science,* 1960, **131,** 1371-1372.

Hull, C. L. *Principles of Behavior: An Introduction to Behavior Theory.* New York: D. Appleton-Century Company, Inc., 1943.

Hydén, H., and Egyházi, E. Nuclear RNA changes of nerve cells during a learning experiment in rats. *Proc. Nat. Acad. Sci.,* 1962, **48,** 1366-1373.

Isaacson, R. L., Douglas, R. J., and Moore, R. Y. The effect of radical hippocampal ablation on acquisition of avoidance response. *J. comp. physiol. Psychol.,* 1961, **54,** 625-628.

Jacobsen, C. F., and Haslerud, G. M. Studies of cerebral function in primates. *Comp. Psychol. Monogr.,* 1936, **13,** No. 63.

———, and Nissen, H. W. Studies of cerebral function in primates: IV. The effects of frontal lobe lesions on the delayed alternation habit in monkeys. *J. comp. Psychol.,* 1937, **23,** 101-112.

Jessor, R. The problem of reductionism in psychology. *Psychol. Rev.,* 1958, **65,** 170-178.

John, E. R., and Killam, K. F. Electrophysiological correlates of differential approach-avoidance conditioning in cats. *J. nerv. ment. Dis.,* 1960, **131,** 183-201.

Jung, R., and Hassler, R. The extrapyramidal motor system. In J. Field (ed.), *Handbook of Physiology: Neurophysiology.* Vol. 2. Washington, D.C.: Amer. Physiol. Soc., 1960, pp. 863-927.

Kaada, B. R. Somato-motor, automatic, and electrocorticographic responses to electrical stimulation of "rhinencephalic" and other structures in primate, cat, and dog. *Acta physiol. scand.,* 1951, **24,** Suppl. 83.

King, F. A. Effects of septal and amygdaloid lesions on emotional behavior and conditioned avoidance responses in the rat. *J. nerv. ment. Dis.,* 1958, **126,** 57-63.

KLÜVER, H. Functional significance of the geniculo-striate system. *Biol. Sympos.*, 1942, **7**, 253-299.

KÖHLER, W. Relational determination in perception. In L. A. Jeffress (ed.), *Cerebral Mechanisms in Behavior.* New York: John Wiley & Sons, Inc., 1951, pp. 200-230.

KONORSKI, J. Mechanisms of learning. *Sympos. Soc. exp. Biol.*, 1950, **4**, 409-431.

———. The physiological approach to the problem of recent memory. In J. F. Delafresnaye (ed.), *Brain Mechanisms and Learning.* Springfield, Ill.: Charles C Thomas, Publisher, pp. 115-132.

KRAFT, M. S., OBRIST, W., AND PRIBRAM, K. H. The effect of irritative lesions of the striate cortex on learning of visual discriminations in monkeys. *J. comp. physiol. Psychol.*, 1960, **53**, 17-22.

KRASNE, F. B. General disruption resulting from electrical stimulus of ventromedial hypothalamus. *Science,* 1962, **138**, 822-823.

KRECH, D. Cortical localization of function. In L. Postman (ed.), *Psychology in the Making.* New York: Alfred A. Knopf, Inc., 1962, pp. 31-72.

KRUGER, L., AND PORTER, P. A behavioral study of the functions of the Rolandic cortex in monkeys. *J. comp. Neurol.*, 1958, **109**, 439-469.

LASHLEY, K. S. Basic neural mechanisms in behavior. *Psychol. Rev.*, 1930, **37**, 1-24.

———. The problem of cerebral organization in vision. *Biol. Sympos.*, 1942, **7**, 301-322.

———. Studies of cerebral function in learning. XII. Loss of the maze habit after occipital lesions in blind rats. *J. comp. Neurol.*, 1943, **79**, 431-462.

———. The mechanism of vision: XVIII. Effects of destroying the visual "associative areas" of the monkey. *Genet. Psychol. Monogr.*, 1948, **37**, 107-166.

———. Cerebral organization and behavior. *Res. Publ. Assoc. nerv. ment. Dis.*, 1958, **36**, 1-18.

———, CHOW, K. L., AND SEMMES, J. An examination of the electrical field theory of cerebral integration. *Psychol. Rev.*, 1951, **58**, 123-136.

———, AND CLARK, G. The cytoarchitecture of the cerebral cortex of Ateles: A critical examination of architectonic studies. *J. comp. Neurol.*, 1946, **85**, 223-306.

LILLY, J. Correlations between neurophysiological activity in the cortex and short-term behavior in the monkey. In H. Harlow and C. N. Woolsey (eds.), *Biological and Biochemical Bases of Behavior.* Madison: University of Wisconsin Press, 1958, pp. 83-100.

LIVINGSTONE, R. B. How man looks at his own brain: An adventure shared by psychology and neurophysiology. In S. Koch (ed.) *Psychology: A Study of a Science.* Vol. 4. New York: McGraw-Hill Book Company, Inc., 1962, pp. 51-99.

LORENTE DE NÓ, R. Analysis of the activity of the chains of internuncial neurons. *J. Neurophysiol.*, 1938, **1**, 207-244.

MCCLEARY, R. A. Response specificity in the behavioral effects of limbic system lesions in the cat. *J. comp. physiol. Psychol.*, 1961, **54**, 605-613.

MACLEAN, P. D. The limbic system with respect to two basic life principles. In M. A. B. Brazier (ed.), *The Central Nervous System and Behavior.* Transactions of the Second Conference. New York: Josiah Macy, Jr. Foundation, 1959, pp. 31-118.

MAGOUN, H. W. *The Waking Brain.* Springfield, Ill.: Charles C Thomas, Publisher, 1958.

MAHUT, H. Effects of subcortical electrical stimulation on learning in the rat. *J. comp. physiol. Psychol.*, 1962, **55**, 472-477.

MALMO, R. B. Interference factors in delayed response in monkeys after removal of frontal lobes. *J. Neurophysiol.*, 1942, **5**, 295-308.

MARGULES, D. L., AND OLDS, J. Identical "feeding" and "rewarding" systems in the lateral hypothalamus of rats. *Science,* 1961, **135**, 374-375.

MEYER, D. R. Some psychological determinants of sparing and loss following damage to the brain. In H. Harlow and C. N. Woolsey (eds.), *Biological and Biochemical Bases of Behavior.* Madison: University of Wisconsin Press, 1958, pp. 173-192.

MILLER, G. A., GALANTER, E., AND PRIBRAM, K. H. *Plans and the Structure of Behavior.* New York: Holt, Rinehart, and Winston, Inc., 1960.

MILLER, N. E. Implications for theories of reinforcement. In D. E. Sheer (ed.), *Electrical Stimulation of the Brain.* Austin: University of Texas Press, 1961, pp. 575-584.

MILNER, B. Psychological defects produced by temporal lobe excision. *Res. Publ. Assoc. nerv. ment. Dis.*, 1958, **36**, 244-257.

———. Laterality effects in audition. In V. B. Mountcastle (ed.), *Interhemispheric Relations and Cerebral Dominance.* Baltimore: Johns Hopkins Press, 1962, pp. 177-195.

———. Some effects of frontal lobectomy in man. In J. M. Warren and K. Akert (eds.), *The Frontal Granular Cortex and Behavior.* New York: McGraw-Hill Book Company, Inc., 1964, pp. 313-334.

MILNER, P. M. The cell assembly: Mark II. *Psychol. Rev.*, 1957, **64**, 242-252.

MISHKIN, M. Visual discrimination performance following partial ablations of the temporal lobe: II. Ventral surface vs. hippocampus. *J. comp. physiol. Psychol.*, 1954, **47**, 187-193.

———. A possible link between interhemispheric integration in monkeys and cerebral dominance in man. In V. B. Mountcastle (ed.), *Interhemispheric Relations and Cerebral Dominance.* Baltimore: Johns Hopkins Press, 1962, pp. 101-107.

———. Perseveration of central sets after frontal lesions in monkeys. In J. M. Warren and K. Akert (eds.), *The Frontal Granular Cortex and Behavior.* New York: McGraw-Hill Book Company, Inc., 1964, pp. 219-241.

———, AND PRIBRAM, K. H. Visual discrimination performance following partial ablations of the temporal lobe: I. Ventral vs. lateral. *J. comp. physiol. Psychol.*, 1954, **47**, 14-20.

———, AND ———. Analysis of the effects of frontal lesions in monkey: I. Variations of delayed alternation. *J. comp. physiol. Psychol.*, 1955, **48**, 492-495.

———, AND ———. Analysis of the effects of frontal lesions in monkey: II. Variations of delayed response. *J. comp. physiol. Psychol.*, 1956, **49**, 36-40.

———, PROCKOP, E. S., AND ROSVOLD, H. E. One-trial object-discrimination learning in monkeys with frontal lesions. *J. comp. physiol. Psychol.*, 1962, **55**, 178-181.

MORRELL, F. Lasting changes in synaptic organization produced by continuous neuronal bombardment. In J. F. Delafresnaye (ed.), *Brain Mechanisms and Learning.* Springfield, Ill.: Charles C Thomas, Publisher, 1961, pp. 375-392.

———, AND JASPER, H. H. Electrographic studies of the formation of temporary connections in the brain. *EEG clin. Neurophysiol.*, 1956, **8**, 201-215.

———, AND NAITOH, P. Effect of cortical polarization on a conditioned avoidance response. *Exp. Neurol.*, 1962, **6**, 507-523.

———, ROBERTS, L., AND JASPER, H. H. Effect of focal epileptogenic lesions and their ablation upon conditioned electrical responses of the brain in the monkey. *EEG clin. Neurophysiol.*, 1956, **8**, 217-236.

MOUNTCASTLE, V. B. Duality of function in the somatic afferent system. In M. A. B. Brazier (ed.), *Brain and Behavior.* Vol. 1. Washington, D.C.: Amer. Inst. Biol. Sci., 1961, pp. 67-93.

MOUSHEGIAN, G., RUPERT, A., MARSH, J. T., AND GALAMBOS, R. Evoked cortical potentials in absence of middle ear muscles. *Science*, 1961, **133**, 582-583.

MYERS, R. E. Discussion in V. B. Mountcastle (ed.), *Interhemispheric Relations and Cerebral Dominance.* Baltimore: Johns Hopkins Press, 1962, pp. 117-129.

NEFF, W. D. Neural mechanisms of auditory discrimination. In W. A. Rosenblith (ed.), *Sensory Communication.* New York: M.I.T.-John Wiley & Sons, Inc., 1961, pp. 259-278.

OLDS, J. Runway and maze behavior controlled by basomedial forebrain stimulation in the rat. *J. comp. physiol. Psychol.*, 1956, **49**, 507-512.

———. Effects of hunger and male sex hormone on self-stimulation of the brain. *J. comp. physiol. Psychol.*, 1958, **51**, 320-324.

———. Hypothalamic substrates of reward. *Physiol. Rev.*, 1962, **42**, 554-604.

———, AND MILNER, P. Positive reinforcement produced by electrical stimulation of septal area and other regions of rat brain. *J. comp. physiol. Psychol.*, 1954, **47**, 419-427.

———, AND OLDS, M. E. Interference and learning in paleocortical systems. In J. F. Delafresnaye (ed.), *Brain Mechanisms and Learning.* Springfield, Ill.: Charles C Thomas, Publisher, 1961, pp. 153-187.

———, AND ———. Approach-avoidance interactions in rat brain. *Amer. J. Physiol.*, 1962, **203**, 803-810.

O'LEARY, J. L., AND GOLDRING, S. Changes associated with forebrain excitation processes: d.c. potentials of the cerebral cortex. In J. Field (ed.), *Handbook of Physiology: Neurophysiology.* Vol. 1. Washington, D.C.: Amer. Physiol. Soc., 1959, pp. 315-328.

ORBACH, J. Non-visual functioning of occipital cortex in the monkey. *Proc. Nat. Acad. Sci.*, 1955, **41**, 264-267.

———. Immediate and chronic disturbances on the delayed response following transection of frontal granular cortex in the monkey. *J. comp. physiol. Psychol.*, 1956, **49**, 46-51.

———, AND CHOW, K. L. Differential effects of resections of somatic areas I and II in monkeys. *J. Neurophysiol.*, 1959, **22**, 195-203.

———, AND FANTZ, R. L. Differential effects of temporal neo-cortical resections on overtrained and non-overtrained visual habits in monkeys. *J. comp. physiol. Psychol.*, 1958, **51**, 126-129.

———, MILNER, B., AND RASMUSSEN, T. Learning and retention in monkeys after amygdala-hippocampus resection. *A.M.A. Arch. Neurol.*, 1960, **3**, 230-251.

PAPEZ, J. W. A proposed mechanism of emotion. *A.M.A. Arch. Neurol., Psychiat.*, 1937, **38**, 725-743.

PASIK, P., PASIK, T., BATTERSBY, W. S., AND BENDER, M. B. Visual and tactual discriminations by macaques with serial temporal and parietal lesions. *J. comp. physiol. Psychol.*, 1958, **51**, 427-436.

PATTON, H. D., AND AMASSIAN, V. E. The pyramidal tract: Its excitation and functions. In J. Field (ed.), *Handbook of Physiology: Neurophysiology.* Vol. 2. Washington, D.C.: Amer. Physiol. Soc., 1960, pp. 837-862.

PAVLOV, I. P. *Conditioned Reflexes.* New York: Oxford University Press, 1927.

PEARLMAN, C. A., JR., SHARPLESS, S. K., AND JARVIK, M. E. Retrograde amnesia produced by anesthetic and convulsant agents. *J. comp. physiol. Psychol.*, 1961, **54**, 109-112.

PENFIELD, W. Functional localization in temporal and deep sylvian areas. *Res. Publ. Assoc. nerv. ment. Dis.*, 1958, **36**, 210-226.

———, AND RASMUSSEN, T. *The Cerebral Cortex of Man: A Clinical Study of Localization of Function.* New York: The Macmillan Co., 1950.

PFAFFMAN, C. The sense of taste. In J. Field (ed.), *Handbook of Physiology: Neurophysiology.* Vol. 1. Washington, D.C.: Amer. Physiol. Soc., 1959, pp. 507-534.

POLIAK, S. The main afferent fiber systems of the cerebral cortex in primates. *U. Calif. Publ. Anat.*, 1932, **2**, 1-370.

PRIBRAM, H., AND BARRY, J. Further behavioral analysis of the parieto-temporal-preoccipital cortex. *J. Neurophysiol.*, 1956, **19**, 99-106.

PRIBRAM, K. H. Comparative neurology and the evolution of behavior. In A. Roe and G. G. Simpson (eds.), *Behavior and Evolution.* New Haven: Yale University Press, 1958, pp. 140-164.

———. A review of theory in physiological psychology. *Ann Rev. Psychol.*, 1960, **11**, 1-40. (*a*)

———. The intrinsic systems of the forebrain. In J. Field (ed.), *Handbook of Physiology: Neurophysiology.* Vol. 2. Washington, D.C.: Amer. Physiol. Soc., 1960, pp. 1323-1344. (*b*)

———. Interrelations of psychology and the neurological disciplines. In S. Koch (ed.), *Psychology: A Study of a Science.* Vol. 4. New York: McGraw-Hill Book Company, Inc., 1962, pp. 119-157. (*a*)

———. Personal communication, 1962. (*b*).

———, AND BAGSHAW, M. Further analysis of the temporal lobe syndrome utilizing frontotemporal ablations. *J. comp. Neurol.*, 1953, **99**, 347-375.

———, AND KRUGER, L. Functions of the "olfactory brain." *Ann. N.Y. Acad. Sci.*, 1954, **58**, 109-138.

———, ———, ROBINSON, F., AND BERMAN, A. J. The effects of precentral lesions on the behavior of monkeys. *Yale J. Biol. Med.*, 1955/56, **28**, 428-443.

———, AND MISHKIN, M. Simultaneous and successive visual discrimination by monkeys with inferotemporal lesions. *J. comp. physiol. Psychol.*, 1955, **48**, 198-202.

———, AND ———. Analysis of the effects of frontal lesions in monkeys. III. Object alternation. *J. comp. physiol. Psychol.*, 1956, **49**, 41-45.

———, AND WEISKRANTZ, L. A comparison of the effects of medial and lateral cerebral resections on conditioned avoidance behavior of monkeys. *J. comp. physiol. Psychol.*, 1957, **50**, 74-80.

———, WILSON, W. A., JR., AND CONNORS, J. Effects of lesions of the medial forebrain on alternation behavior of rhesus monkeys. *Exp. Neurol.*, 1962, **6**, 36-47.

RASHEVSKY, N. *Mathematical Biophysics.* (Rev. ed.) Chicago: University of Chicago Press, 1948.

REYNOLDS, R. W. Ventromedial hypothalamic lesions without hyperphagia. *Amer. J. Physiol.*, 1963, **204**, 60-62.

RIOPELLE, A. J., AND CHURUKIAN, G. A. The effect of varying the intertrial interval in discrimination learning by normal and brain-operated monkeys. *J. comp. physiol. Psychol.*, 1958, **51**, 119-125.

ROBERTS, W. W. Both rewarding and punishing effects from stimulation of posterior hypothalamus of cat with same electrode at same intensity. *J. comp. physiol. Psychol.*, 1958, **51**, 400-407.

ROBERTSON, J. D. The membrane of the living cell. *Scient. American*, 1962, **206**, 64-72.

ROSE, J. E., AND MOUNTCASTLE, V. B. Touch and kinesthesis. In J. Field (ed.), *Handbook of Physiology: Neurophysiology.* Vol. 1. Washington, D.C.: Amer. Physiol. Soc., 1959, pp. 387-429.

———, AND WOOLSEY, C. N. Organization of the mammalian thalamus and its relationships to the cerebral cortex. *EEG clin. Neurophysiol.*, 1949, **1**, 391-404.

ROSENZWEIG, M. R. The mechanisms of hunger and thirst. In L. Postman (ed.), *Psychology in the Making.* New York: Alfred A. Knopf, Inc., 1962, pp. 73-143.

———, KRECH, D., AND BENNETT, E. L. A search for relations between brain chemistry and behavior. *Psychol. Bull.*, 1960, **57**, 476- 492.

ROSVOLD, H. E. Neural structures involved in delayed response performance. In J. M. Warren and K. Akert (eds.), *The Frontal Granular Cortex and Behavior.* New York: McGraw-Hill Book Company, Inc., 1964, pp. 1-15.

———, AND DELGADO, J. M. R. The effect on delayed-alternation test performance of stimulating or destroying electrically structures within the frontal lobes of the monkey's brain. *J. comp. physiol. Psychol.*, 1956, **49**, 365-372.

———, Mirsky, A. F., and Pribram, K. H. Influence of amygdalectomy on social behavior in monkeys. *J. comp. physiol. Psychol.*, 1954, **47**, 173-178.

———, Mishkin, M., and Szwarcbart, M. K. Effects of subcortical lesions in monkeys on visual-discrimination and single-alternation performance. *J. comp. physiol. Psychol.*, 1958, **51**, 437-444.

Rusinov, V. S. An electrophysiological analysis of the connecting function in the cerebral cortex in the presence of a dominant area. *Communs. XIX Intern. Physiol. Congr., Montreal,* 1953, 719-720. Cited in Morrell and Naitoh, 1962.

Santibañez, G., and Pinto-Hamuy, T. Olfactory discrimination deficits in monkeys with temporal lobe ablations. *J. comp. physiol. Psychol.*, 1957, **50**, 472-474.

Schwartzbaum, J. S., and Pribram, K. H. The effects of amygdalectomy in monkeys on transposition along a brightness continuum. *J. comp. physiol. Psychol.*, 1960, **53**, 396-399.

Semmes, J., Weinstein, S., Ghent, L., and Teuber, H.-L. *Somatosensory Changes after Penetrating Brain Wounds in Man.* Cambridge: Harvard University Press, 1960.

Senden, M. v. *Space and Sight.* Glencoe, Ill.: Free Press, 1960.

Settlage, P. H. The effect of occipital lesions on visually-guided behavior in the monkey. *J. comp. physiol. Psychol.*, 1939, **27**, 93-131.

Seward, J. P., Uyeda, A., and Olds, J. Resistance to extinction following cranial self-stimulation. *J. comp. physiol. Psychol.*, 1959, **52**, 294-299.

———, ———, and ———. Reinforcing effect of brain stimulation on runway performance as a function of interval between trials. *J. comp. physiol. Psychol.*, 1960, **53**, 224-228.

Sharpless, S., and Jasper, H. H. Habituation of the arousal reaction. *Brain,* 1956, **79**, 655-680.

Skinner, B. F. Are theories of learning necessary? *Psychol. Rev.*, 1950, **57**, 193-216.

Smith, O. A., Jr. Food intake and hypothalamic stimulation. In D. E. Sheer (ed.), *Electrical Stimulation of the Brain.* Austin: University of Texas Press, 1961, pp. 367-370.

Spaet, T., and Harlow, H. F. Problem solution by monkeys following bilateral removal of the prefrontal areas: ii. Delayed reaction problems involving use of the matching-from-sample method. *J. exp. Psychol.*, 1943, **32**, 424-434.

Sperry, R. W., and Miner, N. Pattern perception following insertion of mica plates into visual cortex. *J. comp. physiol. Psychol.*, 1955, **48**, 463-469.

———, ———, and Myers, R. E. Visual pattern perception following subpial slicing and tantalum wire implantations in the visual cortex. *J. comp. physiol. Psychol.*, 1955, 48, 50-58.

Stamm, J. S. The function of the median cerebral cortex in maternal behavior of rats. *J. comp. physiol. Psychol.*, 1955, **48**, 347-356.

———. Personal communication, 1958.

———. Personal communication, 1962.

———, and Pribram, K. H. Effects of epileptogenic lesions in frontal cortex on learning and retention in monkeys. *J. Neurophysiol.*, 1960, **23**, 552-563.

———, and ———. Effects of epileptogenic lesions of inferotemporal cortex on learning and retention in monkeys. *J. comp. physiol. Psychol.*, 1961, **54**, 614-618.

Stein, L. Secondary reinforcement established with subcortical stimulation. *Science,* 1958, **127**, 466-467.

———. An analysis of stimulus-duration preference in self-stimulation of the brain. *J. comp. physiol. Psychol.*, 1962, **55**, 405-414.

Stellar, E. The physiology of motivation. *Psychol. Rev.*, 1954, **61**, 5-22.

Talbot, S. A., and Marshall, W. H. Physiological studies on neural mechanisms of visual localization and discrimination. *Amer. J. Ophthal.*, 1941, **24**, 1255-1264.

Teitelbaum, P. Disturbances in feeding and drinking behavior after hypothalamic lesions. In M. R. Jones (ed.), *Nebraska Symposium on Motivation,* 1961. Lincoln: University of Nebraska Press, pp. 39-64.

———, and Epstein, A. N. The lateral hypothalamic syndrome: recovery of feeding and drinking after lateral hypothalamic lesions. *Psychol. Rev.*, 1962, **69**, 74-90.

Terzuolo, C. A., and Adey, W. R. Sensorimotor cortical activities. In J. Field (ed.), *Handbook of Physiology: Neurophysiology.* Vol. 2. Washington, D.C.: Amer. Physiol. Soc., 1960, pp. 797-836.

Teuber, H.-L. Physiological psychology. *Ann. Rev. Psychol.*, 1955, **6**, 267-296.

———. Some alterations in behavior after cerebral lesions in man. In A. D. Bass (ed.), *Evolution of Nervous Control from Primitive Organisms to Man.* Washington, D.C.: Amer. Assoc. Advancement Science, 1959, pp. 157-194.

———. The riddle of frontal lobe function in man. In J. M. Warren and K. Akert (eds.), *The Frontal Granular Cortex and Behavior.* New

York: McGraw-Hill Book Company, Inc., 1964, pp. 410-444.

———, Battersby, W. S., and Bender, M. B. *Visual Field Defects after Penetrating Missile Wounds of the Brain.* Cambridge: Harvard University Press, 1960.

Warren, J. M., and Harlow, H. F. Learned discrimination performance by monkeys after prolonged postoperative recovery from large cortical lesions. *J. comp. physiol. Psychol.*, 1952, **45**, 119-126.

Weiskrantz, L. Behavioral changes associated with ablation of the amygdaloid complex in monkeys. *J. comp. physiol. Psychol.*, 1956, **49**, 381-391.

———. Effects of medial temporal lesions on taste preference in the monkey. *Nature*, 1960, **187**, 879-880.

———, and Cowey, A. Striate cortex lesions and visual acuity of the rhesus monkey. *J. comp. physiol. Psychol.*, 1963, **56**, 225-231.

———, Mihailovič, L., and Gross, C. G. Stimulation of the frontal cortex and delayed alternation in the monkey. *Science*, 1960, **131**, 1443-1444.

Wilson, M. Effects of circumscribed cortical lesions upon somesthetic and visual discrimination in the monkey. *J. comp. physiol. Psychol.*, 1957, **50**, 630-635.

———, Stamm, J. S., and Pribram, K. H. Deficits in roughness discrimination after posterior parietal lesions in monkeys. *J. comp. physiol. Psychol.*, 1960, **53**, 535-539.

Wilson, W. A., Jr. Alternation in normal and frontal monkeys as a function of response and outcome of the previous trial. *J. comp. physiol. Psychol.*, 1962, **55**, 701-704.

———, and Mishkin, M. Comparison of the effects of inferotemporal and lateral occipital lesions on visually guided behavior in monkeys. *J. comp. physiol. Psychol.*, 1959, **52**, 10-17.

———, and Oscar, M. Probability learning in monkeys with lateral frontal lesions. *J. comp. physiol. Psychol.*, In press.

———, ———, and Gleitman, H. The effect of frontal lesions in monkeys upon widely-spaced delayed-response trials. *J. comp. physiol. Psychol.*, 1963, **56**, 237-240.

Wolff, H. G., Chapman, L. F., Thetford, W. N., Berlin, L., and Guthrie, T. C. Highest integrative functions in man during stress. *Res. Publ. Assoc. nerv. ment. Dis.*, 1958, **36**, 491-534.

Woolsey, C. N. Patterns of sensory representation in the cerebral cortex. *Fed. Proc.*, 1947, **6**, 437-441.

———. Organization of somatic sensory and motor areas of the cerebral cortex. In H. Harlow and C. N. Woolsey (eds.), *Biological and Biochemical Bases of Behavior.* Madison: University of Wisconsin Press, 1958, pp. 63-81.

———. Organization of cortical auditory system. In W. A. Rosenblith (ed.), *Sensory Communication.* New York: M.I.T.-John Wiley & Sons, Inc., 1961, pp. 235-257.

CHAPTER 3

Behavior Genetics

JERRY HIRSCH AND L. ERLENMEYER-KIMLING
University of Illinois *Columbia University*

INTRODUCTION

Behavior genetics studies relations between heredity and behavior, the two extremes in the continuum of levels of biological organization. The study of heredity and variation is the subject matter of genetics. The study of behavior is the subject matter of psychology. Until recently, communication between these two historically contemporary fields has been almost nonexistent. The concepts and methods applied in one have been developed independently of those used in the other. This chapter considers the concepts and methods of genetics, their relations to behavior, and the far-reaching implications of the development of behavior genetics for the study of behavior.

THE STUDY OF BEHAVIOR

The study of behavior entails not only naturalistic descriptions, but also the taxonomic classifications of diagnosis and systematics, as well as analytic experiments in the laboratory. In the same way that "it is the basic task of the systematist to break up the almost unlimited and confusing diversity of individuals in nature into easily recognizable groups, to work out the significant characters of these units, and to find constant differences between similar ones" (Mayr, 1942, p. 9), so the student of behavior must select out of the continuous stream of activity that is behavior, those units which are suitable for study and which can be classified.

The importance of a "suitable" choice of descriptive features and taxonomic categories will be appreciated when it is realized that the units of behavior chosen and the criteria by which they are recognized will strongly influence the future course of research with respect to both the kinds of analyses performed and the kinds of interpretations made. It is convenient, therefore, to consider the study of behavior as consisting of three distinguishable phases: description, taxonomy, and analysis.

Description. To be part of science, the results of observation must be formulated in terms that will place them in the public domain. An adequate description of a behavior presents a statement of its differentiating properties and of its relations to other events, so that other observers can identify it.

Descriptions of behavior may refer to either means or ends—what Hinde (1959) has called "physical description" and "description in terms of consequences." Ideally, physical descriptions would be made in terms of the means by which a behavior is executed, and thus would refer to the intensity, duration, and pattern of the physical activities of the body. In practice, though, to avoid cumbersome detail, only the grosser aspects of behavior are usually described, e.g., the startle reaction, the amount of time spent in crouching by a bird, the rate of intromission in sexual behavior. Descriptions in terms of consequences, made by reference to the goals of behavior, do not always include an account of the particular means by which those goals are sought. This type of description groups into broad categories (such as reproductive behavior, homing, maze-learning) all those performances that achieve the same end.

The two types of descriptions, however, are not always alternative. In some cases, for example, description in terms of muscular contractions is undesirable because of the unwieldy complexity of the data that result. In other situations, description by consequence is equally undesirable. "Sometimes—as in the threat and courtship posture of birds, which involve both a relatively stereotyped motor pattern and an orientation with respect to the environment—both must be used" (Hinde, 1959, p. 571).

Taxonomy. An ideal behavioral taxonomy would provide a theoretical system under which classes of behavior could be arranged in some meaningful and consistent way. The classes in the taxonomy would be defined in terms of the behaviors they include. In such a system the subjects of classification are behaviors, and the subjects of taxonomy are classifications (King and Nichols, 1960; Simpson, 1961). As yet, no taxonomic system has been devised which attempts to do for behavior what the Linnaean system attempted for species.

Analysis. Analysis breaks behavior down into components that can then be related to antecedent and contemporary events, as well as to the physical properties of the organism. The responses of any behaving organism can be represented as points in a four-dimensional space whose axes are labeled: response, time, stimulus, and individuals. The experimental analysis of behavior may be thought of as the study of covariation along these four dimensions, because behavior shows: (1) temporal variation or differences in response over time; (2) stimulus-response covariation or changes in response as a function of changes in stimulation; and (3) individual variation or differences in response dependent upon the particular organism observed.

Terms like conditioning, learning, maturation, fatigue, adaptation, sensitization, etc., refer to those aspects of behavior in which, for a given individual observed under constant stimulus conditions, a response changes over time. Tropisms, taxes, kineses, preferences, and all of the psychophysical relations refer to those aspects of behavior in which, for a given individual observed over a given time interval, the response changes as a function of changes in stimulation. Mental tests and psychometric methods are techniques for the measurement and analysis of individual differences in response; for all behavior, under a given set of stimulus conditions over a given time interval and in a specified population, the characteristics of the response will depend upon the particular individual observed; the three major approaches employed in the experimental analysis of behavior are conditioning, psychophysics, and psychometrics.

LEVELS OF ORGANIZATION AND DIMENSIONS OF ANALYSIS

The most characteristic feature of all biological material is "organization," by which we mean that when two or more things are put together something new appears, the qualities of which are not additive and cannot be expressed in terms of the qualities of the constituents (see Fig. 3-1). There is a fantastic architecture running through the whole gamut of organization, in which subordinate systems are united at successive levels into ever higher and larger systems. Electrons and nuclei combine to form atoms, atoms to molecules, molecules to amino acids, amino acids to peptides, peptides to proteins, proteins and nucleic acid to nucleoproteins, etc. Chemical and colloidal structures are integrated into organelles (specialized cellular structures), different organelles are integrated into cells, cell populations into tissues, different tissues into organs and systems of organs, these into multicellular organisms, bisexual organisms into Mendelian populations, and the last finally into ecological communities.

These levels of organization are properties of nature, some of which are obvious and readily observable as in the case of the whole organism, while others, as in the case of the chemical constituents of the hereditary material, have only been discerned as the result of decades of research.

The strategy of the scientist's approach to the study of behavior involves the use of dimensions of analysis. Stimulus conditions are manipulated as independent variables and response measures are observed as dependent variables. The methods

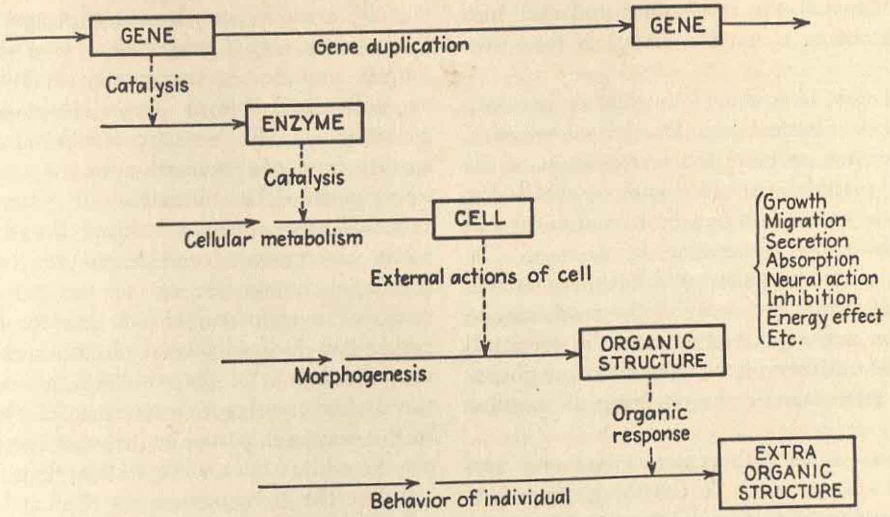


Fig. 3-1. Levels of biological organization and their interrelations (after Wright in Sinnott, Dunn, and Dobzhansky *Principles of Genetics,* 5th ed., McGraw-Hill Book Company, Inc.).

of research must not be confused, however, with the objects studied. Upon the presentation of radiant energy of different wave lengths, properly instructed subjects respond with different hue names (Graham and Ratoosh, 1962). The data which are collected in this way can be used to test (1) genetic theories about the inheritance of genes controlling the development of color vision, (2) physiological theories about the nature of the color receptor apparatus (Fuller and Thompson, 1960, pp. 123-126), or (3) theories about psychophysical relations. Certainly, dimensional analysis is a powerful tool which can yield reliable information about many properties of behavior. The interpretation of that information, however, requires both an understanding of biological organization and an appreciation of the levels of organization for which the information from a particular analysis may be relevant.

THE STUDY OF HEREDITY

Any organism can be described in terms of a large number of characteristics or traits, as many in fact as an observer chooses to analyze. A man's chemical composition, basal metabolic rate, height and weight, coloring, sensory acuities, intellectual capacity, and intellectual achievement are all part of his *phenotype*—the observable aspects of the individual. Underlying the phenotype is *heredity*—the developmental potentials transmitted to the individual from his ancestors. An individual's complete genetic endowment is called his *genotype*. Both phenotype and genotype are of a mosaic nature; the elements of the one bear a definite, but not simple, relationship to the elements of the other. In a complex way the components of the genotype are precursors of the components of the phenotype.

While the genotype remains constant through life, the expression of phenotypic traits may change with development, maturation, and shifting environmental influences. Even when two individuals possess the same hereditary potential for a given character, the character may develop in different ways in these individuals if their environmental backgrounds are sufficiently dissimilar. The range of diverse phenotypes that can be displayed by a genotype is called the *norm of reaction* of that genotype (see p. 106). The phenotype is thus the result of the interaction between heredity and environment, and variations in phenotypic characters are a function of genotypic differences between individuals, as well as of differences in environmental conditions.

Genetics is concerned with the laws of transmission of the units of heredity from parent to offspring, with the function and interaction of these units in development and evolution, and with the physico-chemical structure of the units themselves. In this section we shall consider three approaches to the study of heredity: transmission genetics, population genetics, and physiological genetics. Many of the concepts introduced here will be described in greater detail in later sections.

Transmission Genetics. Transmission genetics, often called classical or Mendelian genetics, studies the mechanics of the transmission of the hereditary material and the ways in which the transmission system ensures both continuity and variation from one generation to the next. The principles of transmission genetics derive from two lines of study: the study of the mechanics of cell division and reproduction, and the statistical study of the variations in the expression of phenotypic characters among the offspring of specified matings.

In bisexual species, the reproduction of a new generation occurs when a female *gamete* (sex cell) is fertilized by a male gamete to form a *zygote*. All of the multiplying cells of the newly created individual stem from the zygote by successive cell divisions.

Cells consist of many elements. At the moment, our interest centers on the *chromosomes*, the threadlike structures visible in the cell nucleus, for breeding experiments (genetics) and microscopic observation (cytology) have shown that these topographically heterogeneous bodies are the carriers of hereditary information (the chemical code which determines whether a developing organism will become a fly, an elephant, or a man, etc.). Cell division is accompanied by a process called *mitosis* in which the chromosomes are duplicated. Mitosis provides the two resulting daughter cells with identical sets of chromosomes. Within a species, barring accidents of reproduction, the cells of all individuals contain a characteristic number of chromosomes, the *chromosome complement* of the species; the total appearance —i.e., size, shape, and number—of the chromosome set is known as the *karyotype*. Most *somatic* cells (all cells except the gametes) contain *diploid* or duplicate sets of chromosomes. *Homologous* chromosomes (those of similar structure and presumably identical function) are present as pairs. For example, the human karyotype is 46 chromosomes (23 diploid homologues); in *Drosophila melanogaster*, the fruit fly, it is 8 (4 pairs), and in *Carassius auratus*, the goldfish, it is 94 (47 pairs).

If the gametes contained duplicate sets of chromosomes as the somatic cells do, the zygote resulting from their union would have four sets; the chromosome complement of a species would then double every generation. However, just as mitosis provides for continuity in the cells of an individual, *meiosis* serves to maintain the constancy of the chromosome number of the species from one generation to the next. At *gametogenesis* (the formation of gametes from somatic cells), meiosis reduces the diploid somatic chromosome complement to the haploid or single chromosome set of the gametes. The union of a male and female gamete brings together two haploid sets of chromosomes to provide the zygote of the resulting individual with the diploid number characteristic of the species. In this way each parent contributes one half of its own hereditary information to the offspring.

While the chromosomes are the carriers of hereditary information, the hundreds or thousands of *genes* located on each of them are the units in which this information is transcribed. Structurally, a gene, though not yet visible in fine detail, is known from breeding evidence to occupy a specific *locus* (region) on a particular chromosome. Functionally, genes are directly involved in specialized chemical activities, and they are only indirectly related to the phenotypic traits (e.g., eye color, blood group, the ability or inability to taste certain compounds, etc.) that are the end products of chains of biochemical reactions. Nevertheless, it is through phenotypic characters that the genic units of heredity have been identified. The detection of genes depends on the fact that the traits controlled by them are expressed in observably different forms, for example, blue and brown eye colors. To be able to produce such differences, a gene must itself exist in alternative forms, called *alleles*, that represent differences in the chemical functioning of the gene. Some genes are known to have many different forms, or multiple alleles (e.g., the ABO blood-type alleles) while for others only two forms have been recognized.

In 1865, long before the relationship between chromosomes and genes was realized, Mendel formulated the classical laws of inheritance on the basis of breeding experiments in which the distribution of different phenotypic forms was analyzed in the progeny of known matings. Breeding or progeny tests are still the fundamental tool of genetics, but we now know that the laws of the inheritance of genes, and ultimately of traits reflect the pattern of chromosome transmission from one generation to the next. Since the zygote receives a diploid set of chromosomes—a haploid set from one parent and an homologous haploid set from the other parent—it also receives its genes in duplicate. If both homologues of a chromosome pair carry the same alleles of a given gene, the individual is said to be *homozygous* for that gene. He is capable of forming only one kind of gamete with respect to that particular gene. As an illustration, we can consider a gene with two alleles, **T** and **t**, which in humans controls the ability to taste the chemical compound phenylthiocarbamide (PTC). To homozygotes with the two alleles, **TT**, the compound has a bitter taste in solution, while to homozygotes of genotype **tt**, it is tasteless.

Frequently, the gametes from the two parents contain different alleles of a gene. The offspring is then a *heterozygote* for that particular gene. In a mating between a **TT** and a **tt** homozygote, all of the offspring have the genotype **Tt**. In this particular case, the heterozygotes are tasters and therefore phenotypically indistinguishable from the **TT** homozygotes. The activity of the allele **T** masks that of **t**, preventing the latter from expressing itself in the phenotype. Masking alleles are said to be *dominant* over the alleles whose effects they cover. The latter are called *recessive*. With some genes, neither of two alleles is completely dominant over the other, so that the heterozygote shows a form of the trait more or less intermediate between the homozygous forms. Genotypes, of course, are never seen. They can only be inferred from the phenotypes of the individual and his relatives.

Since homologues segregate to different gametes, a heterozygous individual produces two kinds of gametes, some carrying the maternal chromosome with one allele (e.g., **T**) and others carrying the paternal homologue with the other allele (**t**). This is the basis of Mendel's first law of inheritance, the *law of segregation* of alleles. Table 3-1 shows the possible types of matings and their progeny ratios for the single-gene taster trait. The relative proportions of genotypes and phenotypes in the offspring are referred to as *segregation ratios;* they represent the probability, not the certainty, that an offspring will fit a given classification. Notice that, when one allele is dominant, three-fourths of the progeny from matings of two heterozygous parents are expected to resemble their parents, while the remaining one-fourth (the recessive homozygotes) are phenotypically unlike either parent.

At gametogenesis the genes contributed by one parent rarely, if ever, remain together as a single set. While the homologous chromosomes of maternal and paternal origin segregate to different gametes, the segregation occurs independently for every pair. Since all combinations of maternal and paternal chromosomes are equally likely in the gametes, the individual genes show *independent assortment* (Mendel's second law). For example, if a **Tt** heterozygote is also heterozygous for the alleles **Aa** of another gene located on a different pair of chromosomes, four types of gametes (**TA**, **Ta**, **tA**, **ta**) can be formed with respect to the two genes. The results of a mating between two double heterozygotes assuming dominance at both loci are illustrated in Table 3-2; as an exercise the reader could work out the other possible mating combinations and their expected issue. Most individuals, of course, are homozygous for some genes and heterozygous for others, and thus are

TABLE 3-1

The inheritance of a single gene (PTC: taster-nontaster)

Parents (mating types)	Expected Proportion of Offspring	
	genotype	phenotype
TT × **TT**	all **TT**	all tasters
TT × **tt**	all **Tt**	all tasters
tt × **tt**	all **tt**	all nontasters
TT × **Tt**	½**TT**:½**Tt**	all tasters
tt × **Tt**	½**Tt**:½**tt**	½ taster:½ nontaster
Tt × **Tt**	¼**TT**:½**Tt**:¼**tt**	¾ taster:¼ nontaster

TABLE 3-2

Progeny genotype alternatives in the mating of double heterozygotes for two independent autosomal genes

Mating of double heterozygotes **TtAa × TtAa**

gametes	**TA**	**Ta**	**tA**	**ta**
TA	**TTAA**	**TTAa**	**TtAA**	**TtAa**
Ta	**TTAa**	**TTaa**	**TtAa**	**Ttaa**
tA	**TtAA**	**TtAa**	**ttAA**	**ttAa**
ta	**TtAa**	**Ttaa**	**ttAa**	**ttaa**

Expected distribution of phenotypes if there is dominance at both loci.

Ratios of phenotypically homogeneous classes, given dominance at both loci.

$$9\left\{\begin{matrix}1\ \mathbf{TTAA}\\ 2\ \mathbf{TTAa}\\ 2\ \mathbf{TtAA}\\ 4\ \mathbf{TtAa}\end{matrix}\right. : 3\left\{\begin{matrix}1\ \mathbf{TTaa}\\ 2\ \mathbf{Ttaa}\end{matrix}\right. : 3\left\{\begin{matrix}1\ \mathbf{ttAA}\\ 2\ \mathbf{ttAa}\end{matrix}\right. : 1\ \{\mathbf{ttaa}$$

capable of producing an enormous array of alternative kinds of gametes. Genes on the same chromosomes are known as *linkage groups* because they are transmitted together. Sometimes during meiosis, however, linkages break and homologues exchange parts. This is called *crossing over.* When crossing over occurs, the chromosome passed along in the gamete represents a mixture of genes of maternal and paternal origin; thus crossing over results in gene recombination.

In many bisexual species, one pair of chromosomes differs from the others. This is the pair of *sex chromosomes,* the chief determiners of an individual's sex. In man, and in many other animal and plant species, females have two identical sex chromosomes, designated as *X,* while males have one *X* and another chromosome, *Y,* that differs in structure and genetic composition from the *X.*[1] Because females in these species transmit only one kind of sex chromosome, they are called *homogametic.* Males, capable of transmitting either an *X* or a *Y,* are called *heterogametic.* Female offspring result from the fertilization of a maternal gamete by an *X*-carrying paternal gamete; male offspring result when the paternal gamete carries a *Y* chromosome. Males receive all of their *X*-chromosome genes from their mothers and transmit them only to their female offspring. Many of the genes located on the *X* chromosome are not present on the smaller and functionally less active *Y*. Females are either homozygous or heterozygous for genes on the *X* chromosome just as both sexes are for genes on all the other chromosomes (the *autosomes*). In males, however, these *sex-linked* genes are not paired, and their full effect, even in the case of recessive alleles, is always expressed. For this reason, recessive forms of sex-linked traits are encountered much more frequently in males than in females. Common examples in humans are color blindness, hemophilia (a condition which prevents normal clotting of the blood) and primaquine sensitivity (drug-induced hemolytic anemia). Fig. 3-2 illustrates the pattern of sex-linkage.

Although many characters are apparently controlled by single genes, many others depend upon the combined effects of a number of genes acting together. These are called *polygenic* characters, and the complex of genes involved is known as a polygenic system. Genes in the system may be located on the same or on different chromosomes. Within the polygenic system, each gene obeys all of the usual rules of transmission, but, since its effect is small compared to the total and since

---

[1] In some species, moths and birds for example, this relationship is reversed.

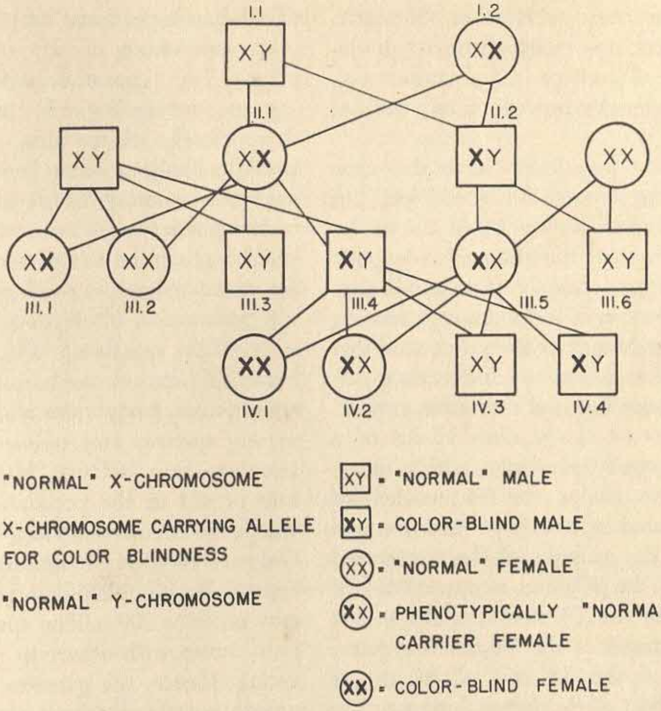

I.1
I.2
II.1
II.2
III.1
III.2
III.3
III.4
III.5
III.6
IV.1
IV.2
IV.3
IV.4
X = "NORMAL" X-CHROMOSOME
X = X-CHROMOSOME CARRYING ALLELE FOR COLOR BLINDNESS
Y = "NORMAL" Y-CHROMOSOME
XY = "NORMAL" MALE
XY = COLOR-BLIND MALE
XX = "NORMAL" FEMALE
X**X** = PHENOTYPICALLY "NORMAL" CARRIER FEMALE
XX = COLOR-BLIND FEMALE

Fig. 3-2. Pattern of recessive sex-linkage, taking color blindness as an example. Notice that individuals IV. 1-4 are offspring of a cousin marriage between III. 4 and III. 5.

many different combinations of alleles are possible, the study of polygenic characters requires different methods from the study of single-gene characters. Generally, characters determined by one or a very few genes can be classified into a few sharply defined categories of expression that appear to be qualitatively different from one another. These traits are studied by counting the numbers of individuals in the different phenotypic classes and comparing the observed ratios with segregation ratios expected for given types of matings. Polygenic characters, on the other hand, show continuous variation; the number of classes to which individuals can be assigned is limited only by the sensitivity of the method of measurement. Each observation is unique or potentially so. Intelligence as measured by the IQ test, for instance, is considered to be polygenically determined, and it displays a continuous distribution of phenotypic values. Without regular discontinuities there are no natural means of grouping observations into frequencies, and each observation is a measurement of a different grade of expression of the character. Therefore, it is necessary to use descriptive statistics such as means and variances to replace class frequencies in representing continuous distributions.

Population Genetics. At a very brief glance we have seen how the mechanisms of transmission of genes, the units of heredity carried in the chromosomes, account for continuity of hereditary material from generation to generation on the one hand and give rise to variation on the other hand. From the study of transmission genetics we are led to *population genetics,* the study of the distribution of genes in populations, the mechanisms responsible for stability or change in gene frequencies, and the role of such mechanisms in evolution.

The term *Mendelian population,* as used in genetics, refers to a community of interbreeding individuals. If we speak of rabbits and wolves living in a forest in Norway and of rabbits in Pennsylvania, we are talking of three separate populations: between the first and the second there can be no interbreeding because they are not members of the same species, between the first and third there is no interbreeding because of their geographic isolation from each other. Similarly, among humans there are many distinct populations that, for lack of contact or for various cultural reasons, are generally reproductively isolated from each other.

The members of a population have the same chromosomes and the same chromosomal loci, but they may carry different alleles. All of the alleles carried by the individual members of a population constitute the *gene pool* of that population. Of course, the proportion of individuals possessing one allele may be greater than the proportion that are carriers of another allele. Since everyone carries the gene in some form, the relative proportions or frequencies of the various alleles of a given gene in the population always add to unity; for a gene with two alleles, the frequencies are customarily designated as p and q—and $p + q = 1.00$. Returning to the example of the tasters and nontasters of PTC, the **T** allele is transmitted in all of the gametes of the **TT** homozygotes and in one-half of the gametes of the **Tt** heterozygotes; likewise one-half of the **Tt** and all of the **tt** gametes transmit the **t** allele. If we designate the frequency of **T** alleles as p and that of the **t** alleles as q, the proportions of **TT** and **tt** homozygotes in the population are represented by p^2 and q^2, respectively. The proportion of **Tt** heterozygotes is then $2pq$ (since **T** or **t** can be present in either the egg or the sperm, the heterozygote can be formed in two ways).

As long as matings occur at random among the genotypes in a population, there will be p**T** and q**t** alleles combining to produce the genotypes of the next generation. The ways in which the alleles can combine and the relative frequencies of the genotypes which they produce are shown in Table 3-3. Note that the genotypic frequencies are the same in the offspring generation as they were in the parent generation and the allelic frequencies must be the same. With random mating, and barring disturbing influences, the relative frequencies of both the genotypes and their alleles maintain an equilibrium over the generations. This principle, known as the *Hardy-Weinberg law*, is one of the fundamental concepts of population genetics. Since in nature disruptions of the equilibrium condition very often occur, the expectations of the Hardy-Weinberg principle represent a theoretical base line by which the magnitude of distorting influences may be estimated.

A random-mating system, in which individuals of all genotypes have equal opportunity to mate with individuals of any other genotype, rarely occurs. Two types of departure from random mating are *inbreeding*, the matings of individuals more closely related than average, which is especially likely to occur in small populations, and *assortative mating*, the tendency of either like or unlike phenotypes to mate together. Examples of phenotypic traits which are frequently the basis for assortative mating in humans are physical stature, skin color, intelligence, and perhaps certain personality variables; differences in courtship-behavior cues are one basis for assortative mating among many vertebrates and insects. Both of these mating systems may enhance the role of natural selection (see below). *Migration* of individuals into or out of the population is another way in which the Hardy-Weinberg balance may be upset. Only a very few of the vast number of gametes formed by an individual (e.g., the human male may produce 200 million spermatozoa per ejaculation) unite with others to produce surviving offspring. Hence, the gametes transmitted to a new generation of individuals represent a sample of the gametic potentialities of the preceding generation. In large populations with random mating, the sample is sufficiently representative to ensure the constancy of allelic frequencies; in very small interbreeding communities, however, chance (sometimes called *genetic drift*) may play a large role and greatly change the distribution of alleles in one generation.

The chief and most direct sources of distortion of the genetic equilibrium of a population are mutation and selection. *Mutation* is the inaccurate reproduction of a gene, transforming it from one allelic form to another. Much genetic variability arises from mutations which introduce new alleles that may then become incorporated into the gene pool. Mutations are constantly occurring—for instance, in every generation some **T** alleles will mutate to **t** and vice versa—but their importance to the equilibrium of the gene pool depends upon the differential rates with which the two alleles mutate to each other. Suppose that **T** changes to **t** at a much greater rate every generation than the rate of the reverse mutation, then over a period of time an excess of **t** alleles is accumulated, altering the relative frequencies of the two alleles and of the genotypes and phenotypes. What happens to the products of mutation, however, also depends on selection.

Selection refers to the causing of differential rates of reproduction of some genotypes relative to others. An allele that has adverse physiological effects upon its carriers is at a disadvantage compared with its alternative forms. In some cases the effects of such alleles prevent the individuals carrying them from surviving to maturity; in others the individuals survive but are relatively infertile—in either case, however, carriers of deleterious alleles fail to make a proportionate contribution to the gene pool of the succeeding generation. If dominant alleles are at a strong selective disadvantage, i.e., if their carriers very rarely reproduce, they are not maintained in the population at all; their occasional appearance in an individual represents a fresh mutation in the sex cells of one of the parents. Since recessive alleles can express their adverse effects solely in homozygotes, only the unaffected heterozygotes can continue to transmit the allele.

When the initial frequency of a deleterious recessive is high, it must decline to a level at which the rate of mutation balances the rate of elimination, because the intermating of heterozygotes will produce homozygous recessive offspring and more alleles can be lost in these homozygotes than may be replenished by new mutations. A well-known illustration of an adverse recessive in man is that

TABLE 3-3

The allelic and genotypic proportions to be expected in a random mating population are:
p^2**TT** : $2pq$**Tt** : q^2**tt** because gametes combine in the following ways:

		Egg	
		p**T**	p**t**
Sperm	p**T**	p^2**TT**	pq**Tt**
	q**t**	pq**Tt**	q^2**tt**

In random mating the genotypes pair according to their relative frequencies in the population

mating types and their proportions				*progeny types and their proportions*		
♀	♂	relative frequency		**TT**	**Tt**	**tt**
TT	× **TT**	$p^2 \times p^2$	$= p^4$	p^4		
TT	× **Tt**	$p^2 \times 2pq$	$= 4p^3q$	$2p^3q$	$2p^3q$	
Tt	× **TT**	$2pq \times p^2$				
TT	× **tt**	$p^2 \times q^2$	$= 2p^2q^2$		$2p^2q^2$	
tt	× **TT**	$q^2 \times p^2$				
Tt	× **Tt**	$2pq \times 2pq$	$= 4p^2q^2$	p^2q^2	$2p^2q^2$	p^2q^2
Tt	× **tt**	$2pq \times q^2$	$= 4pq^3$		$2pq^3$	$2pq^3$
tt	× **Tt**	$q^2 \times 2pq$				
tt	× **tt**	$q^2 \times q^2$	$= q^4$			q^4
sum of progeny types for each column.				$p^4 + 2p^3q + p^2q^2$	$2p^3q + 4p^2q^2 + 2pq^3$	$p^2q^2 + 2pq^3 + q^4$
when factored				$= p^2(p^2 + 2pq + q^2)$	$= 2pq(p^2 + 2pq + q^2)$	$= q^2(p^2 + 2pq + q^2)$
yields				$= p^2$	$= 2pq$	$= q^2$
because $(p + q) = 1$ and $(p + q)^2 = p^2 + 2pq + q^2 = 1^2 = 1$ therefore, we know that every generation has the same genotypic proportions *if mating is random.*				p^2**TT** :	$2pq$**Tt** :	q^2**tt**

causing phenylketonuria, a metabolic disorder generally accompanied by severe mental retardation; the allele is maintained in the gene pools of the European and American whites at a low frequency ($q \approx .005$).

All of the forces that disturb the genetic equilibrium of a population are forces that induce changes in the composition of the gene pool, which is precisely what constitutes *evolution*. Populations of individuals live in environments to which they must adapt for their survival. With changes in the environment, some previously well-adapted genotypes may be unable to function successfully. Whether or not the failure is due to inadequate physiological or behavioral adjustment to the new circumstances, natural selection may be said to operate against the carriers of particular genetic endowments when they fail to survive and produce their proportionate share of progeny. Other genotypes, however—sometimes just the ones that were at a disadvantage in an earlier environment—may thrive and increase in frequency. If none of the genotypes existing in the population are able to adapt to the new environment, the population is extinguished. We can see now the extreme importance of genetic variability in the continuing life of a population as it encounters changing demands from the environment.

Physiological Genetics. We have already noted that genes are specific chromosomal loci concerned with specific chemical activities, that alleles are the alternative ways in which the chemical activity may be carried out, and that these differences in form are the result of mutation, the inaccurate replication of a gene. Modern *physiological genetics* inquires still further into the nature of genes and their variants and into their role in development. The study of the relationship between phenotypes and genotypes belongs to this area of genetics.

Chromosomes consist chiefly of three chemical substances: proteins and two kinds of nucleic acids—deoxyribonucleic acid (DNA) and ribonucleic acid (RNA). DNA, the substance which in most organisms is found mainly in the chromosomes, is considered to be one of the fundamental components of hereditary material. The giant DNA molecule is composed of a simple sugar (deoxyribose), phosphate, and four kinds of nucleotide bases (two purines and two pyrimidines), linked together in extended chains; in one DNA molecule there may be as many as 30,000 linked units (Bonner, 1961), and a human zygote is estimated to contain in the neighborhood of 5 million nucleotide pairs (Beadle, 1961). Although the proportions of deoxyribose and phosphate in the nucleus are similar in a variety of species studied, the proportions of the nucleotide bases are not the same in different species (Chargaff, 1950). The proportion of purines is always equal to that of pyrimidines, because chemical bonds are formed only between adenine (a purine) and thymine (a pyrimidine) and between guanine (purine) and cytosine (pyrimidine).

Drawing on physical and chemical evidence about DNA and on Wilkins' X-ray diffraction studies, Watson and Crick (1953) suggested the structural model of the DNA molecule. According to this model, the DNA molecule is represented by two long strands twined about each other in the form of a double helix (see Fig. 3-3). The strands, consisting of alternating units of deoxyribose and phosphate, are linked together by nucleotide pairs and are complementary, i.e., whenever a purine occurs on one strand its pairing pyrimidine is present at the same location on the other strand. According to present hypotheses, the replication of the DNA molecule involves the separation of the two strands and the formation of new daughter strands by attachment of complementary bases to the parental strands (adenine to thymine, etc.). Experimental evidence from studies of replication in bacteriophage (Meselson and Stahl, 1958) and *in vitro* studies of enzymes (Lehman et al., 1958) support the hypothesized explanation of replication. Exact details regarding the manner of replication are not yet understood however. It is not known whether the parental strands uncoil, separate, and then synthesize new complementary strands, or whether, as seen in the diagrammatic representation, Fig. 3-3, the synthesis of daughter strands begins as uncoiling takes place. Nor is it known how the bases in the nucleus become available for synthesis.

The arrangement of the nucleotide bases along the length of the strands is considered to be the way in which genetic information is coded. The variety of arrangements is almost limitless—consider that, with only three pairing units in a chain, the four bases would give 64 ($= 4 \times 4 \times 4$) different possible sequential combinations (AGC, ACT, AAA, etc.). A current problem in genetics is to determine how the combinations of nucleotide

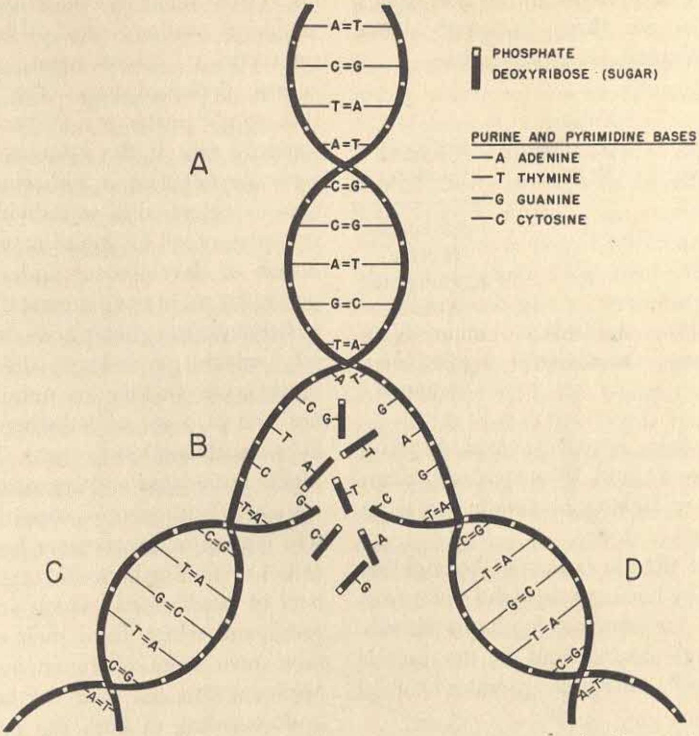


Fig. 3-3. Diagrammatic representation of DNA replication. (A) Helical strands united by paired nucleotide bases, (B) uncoiled strands in the process of replication, (C) and (D) resynthesized double helices, each containing one parental and one new daughter strand.

units account for the different bits of genetic information. Most, if not all, genes control the formation of specific proteins with very specialized functions. Since all proteins are made up of various combinations of about twenty amino acids, it is the coding of these twenty substances that must be accounted for at a genic locus. If the nucleotide bases are considered as code letters, then short sequences of base pairs constitute code words for the amino acids, and long chains of these "words" in the DNA strands determine the sequence of amino acids in a protein. The base sequence (in "messenger RNA"—see below) relating to the amino acid, phenylalanine, has been discovered (Nirenberg and Matthaei, 1961), and those that specify other amino acids have since been reported (Smith, 1962; Speyer et al., 1962).

Protein synthesis takes place chiefly in the *ribosomes*, tiny bodies found in the cytoplasm, outside of the nucleus where the chromosomes are located. Thus, it is necessary to look for some substance which is capable of carrying the information of chromosomal DNA to the cytoplasmic elements that operate in protein formation. The search for such an intermediary substance has turned to RNA, the nucleic acid present in both the nucleus and cytoplasm. RNA differs from DNA in its sugar—ribose replaces the deoxyribose of DNA—and in one of its nucleotide bases—in RNA, uracil is substituted for thymine. It is now believed that DNA is responsible for the synthesis of a special type of RNA which reflects the code sequence contained in the DNA. Upon migrating to the cytoplasmic ribosomes (whose composition is 40 to 60 per cent RNA and the remainder, protein), this "messenger RNA" is then capable of directing protein synthesis in accordance with the specifications of the genetic information laid down in the nuclear DNA.

The relating of specific variations in proteins to variations in gene action, is the closest approximation to the identification of primary gene products. One class of protein is composed of the antigens that stimulate the formation of antibodies. The human blood group antigens provide a well-known demonstration of the relationship between genes and this type of protein. At the ABO locus, for example, there are three principal alleles, $\mathbf{I^A}$, $\mathbf{I^B}$, and **i**, with the following properties:

Genotype	Phenotype	Antigen contained in cells	Antibody action
$\mathbf{I^AI^A}$ or $\mathbf{I^Ai}$	**A**	**A**	agglutinates **B** cells
$\mathbf{I^BI^B}$ or $\mathbf{I^Bi}$	**B**	**B**	agglutinates **A** cells
$\mathbf{I^AI^B}$	**AB**	**A** and **B**	no antibody
ii	**O**	no antigen	agglutinates **A** and **B**

(This multiple allele system is said to show *codominance,* since $\mathbf{I^A}$ and $\mathbf{I^B}$ are equally dominant over **i** but are lacking in dominance in respect to each other.)

Another class of protein is human hemoglobin. The allele carried by homozygotes who suffer from sickle cell anemia, for instance, results in the substitution of a single amino acid in the protein chain which would otherwise produce normal hemoglobin.

The major class of proteins in which gene action has been detected is the enzymes, the catalysts that control the rates of chemical activities carried out by cells. Enzymes catalyze specific chemical reactions which then combine into a series of steps to carry out the complex metabolic processes of the body. Deleterious alleles arising from mutations may produce inadequately functioning enzymes, so that the "normal" reaction at a given step in the sequence fails to occur. The metabolic sequence is then blocked at that step. In some cases, the same phenotypic expression may result from enzymatic blocks at different stages in a given sequence; in this way, different genotypes may yield similar phenotypes. On the other hand, the primary chemical effects of a gene may enter into a variety of processes in development so that the gene has multiple, or *pleiotropic,* consequences.

The apparently direct relationship between genotype and phenotype in the case of the blood-group antigens is most likely an exception rather than a rule. Generally, the phenotypes that we study are not even close approximations to primary gene products but are the products of genes interacting with each other in the development of body organs and tissues; different allelic combinations have different outcomes. The presence of one allele may modify or accentuate the effect of an allele at another locus (this is called epistatic interaction). Environment, too, influences the course of development. The genetic constitution that might make for a sturdy tomcat yields a scraggly runt if the intra-uterine environment is seriously deficient in nutrients. The total phenotype as expressed in an individual results from the interplay of all his genes acting in concert in the course of development under the influence of a particular set of environmental circumstances.

Behavior is a property of organisms—*the* property which psychology studies. The organism, however, is neither an unfathomable black box nor one of a set of indistinguishable black balls in a mathematician's urn. The organism is a highly articulated and organized set of interrelated systems about whose properties we now possess information of great value for the understanding of behavior. Furthermore, organisms live as members of populations, which are themselves structured and which have their own properties and their own rules of functioning. It has become apparent that the study of behavior requires an understanding of both the properties of individuals and the properties of populations.

PROPERTIES OF INDIVIDUALS

Organization of Components. Since the individual consists of sets of interrelated components, familiarity with these components and their interrelations makes possible the understanding of behavior not only in terms of the dynamics of their immediate interactions but also in terms of the adaptive value of the behavior.

One of the most impressive aspects of behavior is the way in which it is patterned. Characteristic features of behavior appear whenever an organism is observed under appropriate conditions, e.g., hungry babies cry, bears hibernate in winter, cuckoos parasitize the parental instincts of other species, etc. Upon analysis, many behavior patterns have been shown to consist of components, often called reflexes, bearing definite temporal and physiological relations to one another. For example, the sight and solicitations of young squabs elicit parental feeding behavior from both male and female ring doves, but only after an appropriate sequence of preceding events.

In a series of elegant experimental analyses, representing one of the outstanding contributions to the study of behavior, Lehrman (1959, 1962) has described the sequence of behaviors in the reproductive cycle of the ring dove, *Streptopelia risoria.* He has shown how various steps in the sequence are dependent upon secretion of appropriate hormones and how hormonal activity in turn is dependent upon the appearance of the appropriate behaviors in the sequence (see Fig. 3-4). The beauty and power of Lehrman's work lies in his approach to the analysis of an integrated system of adaptive events.

We see that behavior patterns are organized systems of responses, which include the activity of organs, themselves the expression of the functioning of cells which in turn can be analyzed into relations among subcellular constituents.

The individual starts life as the zygote resulting from *syngamy,* the fusion of the gametes at fertilization. Immediately after implantation of the zygote, cleavage begins and is followed by all of the subsequent stages of development. In development, the differentiating cells segregate to form the various organs and tissues which are arranged in the pattern of the species and bear physiological relations to one another.

Mitosis, Differentiation, and Development. The single-celled zygote develops into a 2,000,000,000,000 celled human neonate (Swanson, 1960, p. 25) as the result of approximately fifty-six cell divisions (De Beer, 1958, p. 22). The hereditary information transmitted from the previous generation in each *genome* (haploid chromosome set) of the parental gametes is pooled at syngamy by the fusion of the gametes to form the zygote. At cell division, this information is transmitted through the chromosomes to the nuclei of the resulting cells by the process of mitosis. During mitosis the chromosomes of the nucleus separate and congregate in the equatorial plane of the

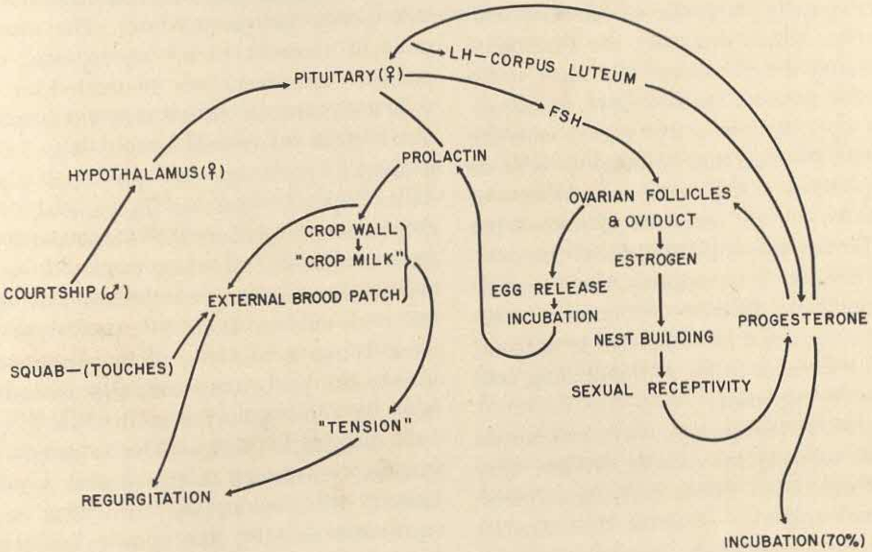


Fig. 3-4. In the reproductive cycle of *Streptopelia risoria,* the stereotyped bowing movements and characteristic cooing vocalization of male courtship trigger the following chain of stimulating events in the female: hypothalamic stimulation of pituitary gland production of follicle-stimulating hormone (FSH); FSH stimulation of estrogen production by ovarian follicles and oviduct. Estrogen induces nest building behavior, setting the stage for sexual receptivity and production of progesterone. Feedback to the pituitary stimulates production of the luteinizing hormone (LH), stimulating development of the corpus luteum, which further stimulates progesterone production, which now acts back upon the ovarian follicles to induce egg release.

Egg release by the female induces incubation by both parents. This stimulates pituitary production of prolactin, which in turn stimulates the crop wall to produce "crop milk." After the young have hatched, the combined effects of "tension" of the milk-distended crop wall, sight of the squab, plus its tactile stimulation of the parental brood patches elicit from both parents the regurgitation feeding response, which gives to the young the accumulated "crop milk."

cell. By this time each chromosome has reproduced by duplicating itself exactly. The two replicates of each chromosome go to opposite poles of the cells where they reconstitute a nucleus with a normal karyotype. Meanwhile, at the equator, a new cell wall forms and as its development extends it to the outer walls, it segments the cytoplasm to complete the division of the old cell into two new ones.

Since mitosis appears to divide the duplicated chromosomes of a cell equally between two resulting daughter cells, the differentiation that is so characteristic of the development of multicellular organisms cannot be explained in terms of the distribution of the hereditary information of the nucleus in any way similar to the way in which we explain the genetic basis of individual differences. It is now believed that unequal division of the cytoplasm is responsible for differentiation. As cells proliferate, different ones come to occupy different positions in space. Because of cytoplasmic heterogeneity (e.g., the so-called animal-vegetal polarity, which describes the directional gradient between the physiologically most active region with the greatest carbohydrate concentration and the opposite less active region with the greatest protein concentration), daughter cells do not receive identical cytoplasmic endowments at cell division. No orderly analogue of mitosis has been found for the equal distribution of the cytoplasm at cell division. Consequently, the chemicals of the nucleus have different cytochemical substrates to draw upon and are subjected to different cytochemical influences in the differentiating cells of the developing organism.

The cells of many tissues, e.g., nerve and muscle cells, lose the capacity for mitotic division after they have differentiated. Other cells, for instance, liver and renal epithelia, conserve this capacity but can exercise it only under special conditions (Ducoff and Ehret, 1959, p. 82). In the gonads during gametogenesis another type of cell division takes place; familiarity with some of its more intimate details is absolutely essential for an understanding of individual differences and evolution.

Meiosis, Individual Differences, and Evolution. As pointed out previously in the discussion of transmission genetics, at syngamy sample halves of the genetic information, carried by each of the parental sexes, combine to form a new individual with a full complement of the genetic information of the species. At gametogenesis, the information, contained in the diploid set of chromosomes, is distributed by the process of meiosis to the haploid gametes according to definite rules.

Meiosis differs from mitosis because a meiotic cell division consists of two divisions of the nucleus accompanied by only one division of the chromosomes. Reduction is accomplished by a modification of mitosis which divides a single diploid chromosome set into a pair of replicating haploid sets, thus, in spermatogenesis, producing a quartet of haploid gametes. During the meiotic reduction division, the members of each chromosome pair meet in the nucleus and orient themselves so that, when they part in anaphase, every gamete receives one from each pair. The orientation of the two members of any pair is a random event, and the assortment to the gametes of the segregating homologues occurs independently for each pair. This process insures diversity, because it maximizes the likelihood that gametes will receive unique genomes. The essential features of recombination,[2] segregation, and independent assortment are illustrated in Fig. 3-5. The illustration shows that in a species with three pairs of chromosomes, e.g., *Drosophila willistoni*, gametogenesis produces eight alternative types of gametes. In general, N pairs of chromosomes produce 2^N alternative gametic genomes. Since man has twenty-three pairs of chromosomes, at syngamy the conjugating gametes are each chosen from $2^{23} = 8{,}388{,}608$ alternatives. When a pair of parents produces two offspring, the probability that the second one will have the same genotype as the first is $(\frac{1}{2^{23}})^2 = 1/70{,}368{,}744{,}177{,}664$. (The chance that the gamete, contributed to the second zygote by the father, will be identical with that contributed to the first is $\frac{1}{2^{23}}$. The same calculation applies to the mother's contribution. The chances that these two independent events will occur at the same time is the product of their separate probabilities, i.e., $(\frac{1}{2^{23}})^2$.) Therefore, the probability that the two unrelated individuals will share the same total genotype is effectively zero.

The above discussion assumes the integrity of

---

[2] Recombination has two meanings, (1) rearrangement of the genes on a chromosome following the exchange of allelic material between chromosomes at crossing over, and (2) the union of maternal and paternal genomes at syngamy to provide the unique combination of genetic material that comprises the genotype of each new organism.

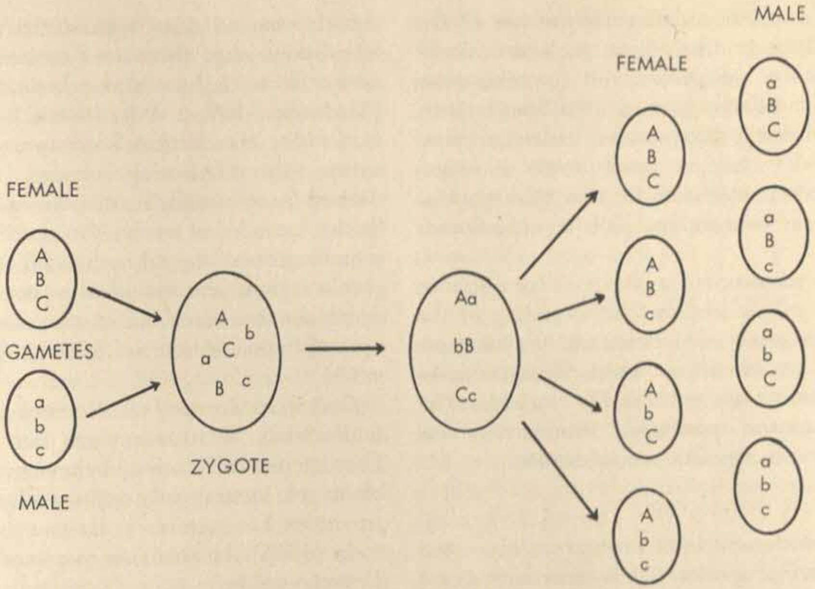


Fig. 3-5. Recombination, segregation, and independent assortment. Maternal components (chromosomes or genes) in capital letters, paternal in lower case.

the individual chromosomes from one generation to the next; variability has only been attributed to the astronomically large number of possible combinations of integral chromosome units. Earlier, it was mentioned that chromosomes sometimes break and exchange parts. When this occurs, occasionally, the order of the genetic material on the chromosome is rearranged. When, as normally happens, recombination occurs with the homologue of the chromosome from which the part was detached, the newly reconstituted chromosome is said to contain an *inversion*. Less frequently, a detached part of one chromosome becomes attached to a nonhomologous chromosome, and the newly constituted chromosome is said to contain a *translocation*. If the detached part combines with a homologous chromosome without an exchange occurring, the homologue to which it has been added is said to contain a *duplication*. A chromosome with a part missing is said to have a *deficiency*. Inversions, translocations, duplications, and deficiencies are *aberrations* which involve segments of chromosomes but not entire chromosomes. Sometimes, one or more chromosomes will be added to, or deficient from, the normal chromosome complement. This is known as *aneuploidy*. Individuals with entire genomes in multiples greater than two are known as *polyploids*.

Despite the number of mechanisms producing variability, it is possible by inbreeding to produce homozygosity at many loci and thus achieve a limited degree of genetic uniformity in a population. However, since every population contains many alleles which, when homozygous, vary in effect from mildly deleterious to lethal, there is at least one condition limiting the amount of uniformity that even inbreeding can achieve.

There is still another mechanism which provides nature with insurance against genetic uniformity: *gene interaction*. The phenotypic expression of many genes is conditioned by the genetic background in which they occur. That is, phenotypic expression depends upon both the alleles and the combinations of alleles of other genes that are present. Since it appears that total uniformity is unattainable, genetic backgrounds will vary and gene interaction will guarantee polymorphisms.

Thus it becomes clear why individual differences are found in populations of bisexual species: variation occurs because the mechanics of heredity and reproduction generate unique genotypes. Moreover, reproductively isolated populations develop different gene pools. Even though such populations belong to the same species and share the same genes, the relative frequencies of the different alleles in their gene pools are almost certain to differ. Mutations and recombinations will occur at different places, at different times, and with differing frequencies. Selection pressures will also vary. A consequence of reproductive isolation, therefore, is that each population travels its own unique path of evolutionary divergence.

There are momentous implications for a science of behavior in our modern understanding of the genotypic structure of populations. We now see that the very materials on which we must make our observations are intrinsically variable. The basic mechanisms producing this variety are gene mutation and genetic recombination.

PROPERTIES OF POPULATIONS

The population mode of existence has evolved with the bisexual species. While individuals come into existence, pass through developmental stages, and then die, populations endure as long as the supply of individuals is replenished by reproduction. Dobzhansky has called to our attention that a population possesses "a corporate genotype;" its genetic structure, while clearly a function of the genetic constitution of the component individuals, nevertheless obeys its own rules of functioning "distinct from those which govern the genetics of individuals" (Dobzhansky, 1951, p. 15).

Biological Individuality. Because of the heterogenic nature of Mendelian populations with the manifold multivalued dimensions of allelic variation that comprise their gene pools, we have seen that the genotypes of the component individuals are unique events. The organization of each genotype is determined by the random sample of allelic values and genic combinations that nature's meiotic lottery deals to each organism at anaphase segregation and gametic recombination.

The differences between the individual members of a population are neither the differences of degree nor the differences of kind that the philosophers distinguish. Individual differences are combinational. All individuals differ from one another not because they have unique endowments but because they possess unique combinations of endowments. The number of hereditary factors from which combinations can be assembled, though large, is finite. Nevertheless, "the combinations themselves are far more numerous than the individuals who can enjoy them, so that for each man actually on stage there are hundreds of possible men still waiting for a cue behind the scenes" (Medawar, 1957, p. 154). While the mechanism of heredity may ultimately prove to be atomic in nature, "the relationship between [organisms] is defined by a virtually continuous spectrum of affinities, bounded at one end by identical twins and extending the other end far beyond the genetically visible regions into the affinities between animals which are not members of the same species or even of the same order or class" (Medawar, 1957, p. 154).

Trait Variations and the Norm of Reaction. Scientific study involves analysis into components. The experimental study of behavior analyzes a behavior of interest into individually measurable properties. For example, in Bastock and Manning's study (1955) the courtship sequence of *Drosophila melanogaster* was analyzed into four stages. (1) Orientation: the male stands close to or follows the female. (2) Vibration: he rapidly vibrates that wing closest to the female's head. (3) Licking: he extends his proboscis and licks the female's ovipositor. (4) Attempted copulation: he tries to mount her. In man, intelligence has been analyzed into components like verbal comprehension, word fluency, numerical ability, spatial memory, etc. (Anastasi, 1958, pp. 317-342).

Whenever a trait is measured in the members of a population, a distribution of individual differences is obtained: for each trait, the individuals in a population are found to be distributed over a range of trait score values. When several traits are studied, their relationship may vary from complete independence to high correlation. The rank of a given individual in the population is often found to vary from one trait distribution to another, e.g., rank on tests of verbal abilities does not necessarily predict rank on tests of musical abilities. Anastasi calls this *trait variability* (1958, p. 317). As used in the present discussion, on the other hand, *trait variation* refers to variation, or individual (and group) differences, in the expression of one trait of a population under a given set of conditions.

Norm of reaction, as we have seen, refers to the range of phenotypes that can arise from the interplay between a given genotype and the various environments in which it can live and develop. Clearly, we can never know the complete norm of reaction for any genotype. In fact, precise observation on even part of the norm of reaction can only be made with species which reproduce by self-fertilization or budding because in all other species, except for the rare identical siblings, every genotype has a unique existence as a single organism that can only develop once (Stebbins, 1950, p. 37).

Conceptually, it is important to realize, though, that every genotype does have a norm of reaction. In the heterogenic populations whose behavior we study, partial information is accumulated about the norm of reaction for different elements of the gene pool. We learn what we can about genes and chromosomes and about specific constellations of genes and chromosomes, always bearing in mind how incomplete that knowledge must necessarily be.

The Abnormality of the Normal. Since the number of discriminable traits is probably unlimited, let us consider the meaning of the term normal in the light of the information we have already reviewed about genotypic and phenotypic organization. It is common to learn in the report of an investigation that "observations were made on normal subjects." For any given trait, this can only mean that the measurements obtained on the subjects do not fall at the extremes of the distribution of measurements for that trait in some population.

It is generally assumed that distributions of biological properties can be represented by a normal distribution curve. With respect to any particular trait, therefore, 0.95 (i.e., $\pm 1.96\sigma$) of the population may be expected to lie within the limits of what is usually defined as the "normal" range of variation. For two independent traits, $(0.95)^2 = 0.902$ of the population will be normal; for three traits, the value is 0.857; for 23 traits, it is 0.307; and for 100 traits, it is 0.006. In general, for n independent traits, the value is $(0.95)^n$. This means that, if there were only twenty-three independent traits, e.g., one for each chromosome pair, almost 70 per cent of a population would *not* be normal with respect to at least one of the traits. Since there is "a vast array of attributes which are potentially measurable (whether by present methods or not), and probably often uncorrelated mathematically . . . *practically every human being is a deviate in some respects*" (Williams, 1956, p. 3).

Since it cannot be known a priori which individuals in a population are normal and which are not with respect to a trait to be studied in an experiment, it is meaningless to report that "normal" subjects were selected for observation because "normal" can only mean that the subjects looked healthy to the investigator.

Heritability and Instinct. The interindividual or phenotypic variance in behavior can be partitioned into components

$$\sigma_P^2 = \sigma_G^2 + \sigma_E^2 + \sigma_{GE}^2$$

assignable to heredity (G), environment (E), and their interaction (GE). *Heritability* refers to that part of the total variance which is attributable to genetic differences among organisms. (In quantitative genetics "heritability" often has a somewhat more restricted meaning than it is being given here.)

It used to be common practice to ask what percentage of the variance in a behavior was attributable to heredity and how much to environment (and to their interaction). Consideration of what is inherited leads us to a critical reexamination of the nature-nurture issue raised by this question. Since the total biological inheritance of the organism is contained in the single-celled zygote, behavior as such cannot be inherited. Genes are inherited, and these possess certain norms of reaction. For a given genotype, a difference in behavior under two or more sets of environmental conditions is a function of differences between those conditions. For a given set of environmental conditions, a difference in behavior between two or more genotypes is a function of differences between those genotypes. Heritability, therefore, cannot be a property of behavior; it is a property of populations with respect to particular traits measured under certain conditions and not of traits per se (Lerner, 1958, p. 63; Fuller and Thompson, 1960, p. 64). The very same behavior that *must* have zero heritability in a clone, e.g., a set of identical siblings, *might* have substantial heritability in a heterogenic group of organisms.

Behavior traits are properties of organisms that behavioral scientists choose to measure, just as traits like length and weight are properties of objects that the physical scientists choose to measure. Heritability is a measure of relationship between a trait and the population in which it is studied, just as weight is a measure of relationship between the mass of a body and the gravitational field in which it is studied. There will be no confusion about heritability if it is remembered that the methods of genetics only permit the study of *differences*—Mendel's first law of segregation defines a gene in terms of an allelic difference and Mendel's second law of independent assortment defines two genes in terms of an interlocular difference. The heritability concept merely summarizes the extent to which phenotypic differences (the trait variance) are related to genotypic differences (the genetic variance) at a given generation under a given set of environmental conditions. Where there are no interindividual gene differences, as in a clone, heritability values must be zero. Where there are interindividual gene differences, as in any cross-fertilizing population, trait heritabilities can only be determined by empirical measurement. For example, intelligence, as measured by a given test, may have quite different heritabilities in different populations; it may also have different heritabilities in the same population under different conditions, e.g., the equalization of educational opportunities should have the effect of increasing the heritability of intelligence because it would minimize environmental sources of variation.

Our discussion so far has dealt with measurements at a given time in a given group of organisms. Heritability has been represented as the ratio of the genetic variance to the phenotypic variance. In the study of the developing organism over time, it is found that certain behaviors appear with striking regularity, e.g., all or most birds fly at an age that is characteristic of their species and, in the appropriate season, build nests of a kind characteristic of their species. Furthermore, the essential features of these behaviors do not seem to be acquired as a result of tuition and rewarded practice because they develop even when the immature organism is raised in isolation from conditions in which training or imitation might plausibly be expected to play a role (Spalding, 1872). Such behaviors are called *instincts* and are frequently said to be the expression of the genetic endowment of a species; although strictly speaking, no information about their genetic correlates can be known without genetic analysis.

It should be noted that heritability and instinct have different meanings and are studied by different methods. The operations involved in the measurement of the heritability of a behavior in a population require the measurement of individual differences and of correlations between relatives in the expression of that behavior. The operations involved in studying the ontogeny of an instinct usually include the observation of the expression of a behavior in an organism reared in an isolation experiment.

Social Structure, Social Interaction and Systems of Mating. "Among the different kinds of populations that exist in nature, . . . the organism-like integration is . . . evident in the breeding associations which are formed in all sexual and cross-fertilizing organisms. The integrating agent in such Mendelian populations is the process of reproduction itself, which establishes mating, parenthood, and progeny bonds between the component individuals." (Dobzhansky, 1951, p. 15). Sociality starts with bisexuality, and both the social structure of a population and the behavior of its individual members influence its genetic structure. The degree and type of social interaction occurring among members of a population, however, vary greatly from species to species. At one extreme are the "social insects" (ants, termites, wasps, and bees), some mammals and birds (such as prairie dogs, beavers, and penguins) that form more or less highly organized communities, and man himself. At the opposite extreme are species—for example many forms of insect and marine life—in which interindividual contact is limited chiefly to breeding, to temporary protective groupings formed under certain conditions, and to occasional competitive encounters in the search for food, territory, or mating partners. Ranging between the two extremes, are many species that display intermediate grades of social interaction. Comparisons of social behavior at various evolutionary levels have so far revealed no clear-cut phylogenetic trend in terms of degree or kind of sociality (Thompson, 1958).

The size of a given population bears an important relationship to social behavior and to the evolutionary force of natural selection. The actual number of individuals in the population, however, is a less important consideration than the density of the population, i.e., the number of individuals per unit area of the occupied territory, and the relative availability of food and shelter suitable to the needs of the species. While crowded populations present more opportunity for social contact, a relatively greater proportion of the interaction is of a competitive nature. Aggression and mortal combat, desertion of the young, and cannibalism in some species, have frequently been observed as the social fruits of competitive crowding. Nor is aberrant social behavior necessarily directed against other individuals. For example, in the periodic mass migrations of the Norwegian lemmings (presumably triggered when population density reaches excessive levels) large portions of the population hurl themselves from cliffs or into rivers or the sea in the course of blind running which bears some resemblance to human group-panic situations (Deevey, 1960). Vulnerability to diseases and to parasitic infections, and in some species, at least, vulnerability to predation, is enhanced by high density of the population. In other species it is the isolated individual that more readily becomes the victim of predators. In conjunction with natural selection which may operate against the "weaker" members of the population in various ways, negative social behavior resulting from overcrowding in the territory tends to reduce the size of the group to a more optimal level for a given environment. Upon approaching that level, however, the same pressures that earlier led to excessive numbers are likely to come into play again, and the cycle is repeated. The importance of these cyclic variations in population size lies in the fact that, during the declining phase, only a sample (frequently not a representative one) of the genotypes survive, reproduce, and contribute to the gene pool of the succeeding generation.

As Thompson (1958) has pointed out, cohesion is the "psychological counterpart of density." A strong degree of cohesion which binds the members of a population into a tightly clustered group may act in the same way as crowding, or high density, whether or not adequate territory is available. On the other hand, a population may show a tendency to scatter, so that the amount of social interaction between individuals or groups of individuals is diminished. Besides lacking the protection offered by group defense, widely scattered individuals lack the social stimulation necessary in many species for the release of courtship (Lehrman, 1961) and other essential behaviors. Another consequence of scattering may be the formation of isolated breeding groups that develop into subpopulations with their own gene pools; prolonged reproductive isolation is, of course, one of the mechanisms of speciation. An interesting example of the relationship between population spread, behavior, and gene changes is found in the deer mouse, *Peromyscus maniculatus*. In the course of dispersing over a large area, the original population has evolved two physically distinguishable subspecies, *gracilis*, living primarily in forests, and *bairdii*, which lives in meadows and open fields. Although they are capable of interbreeding, as shown in laboratory studies, the two subspecies do not cross-mate in their natural territory because the habitat preferences that have evolved keep them from coming into contact with each other (Harris, cited in Fuller and Thompson, 1960, p. 135).

Highly cohesive groups sometimes display low readiness to accept outsiders of their own species who attempt to enter the group. In turn, rigorous exclusion of strangers from the mating system of the group results in a closed mating system and increases the likelihood of inbreeding in a small population. Evidence showing that genetically different strains of mice (Scott and Fredericson, 1951) and of dogs (King, 1954) exhibit different degrees of tolerance toward outgroup individuals indicates that in some instances, at least, this type of permeability has a genetic basis. (We cannot, of course, extend these findings by analogy to explain human group "clannishness" which involves many compounded sociological and economic factors.) Intrusion of outside individuals into an established population may effect total disorganization of the existing structure. A study of populations of rats living in neighborhoods of a large city (Calhoun, cited in Scott, 1958) demonstrates such an outcome; when a number of alien rats were released in the neighborhood, the mortality rate among the resident animals increased significantly and was even greater among the intruders. The result was a net decrease in the local rat population.

Other aspects of social behavior that are important in defining population structure are the amount of cooperative group-directed behavior exhibited by individual members, and the organization of social roles within the group. Cooperative action responsible for the protection of the entire group is notable, for example, in the peripheral guards travelling at the edges of reindeer and caribou herds and the herds and flocks of other species. Crow "sentinels" posted to alert the feeding flock with their danger calls offer another example of survival dependency based on cooperative behavior. The prime illustration of group action, aside from man, is found in the social insects with their highly organized systems of behavioral roles. Individuals in such colonies belong to one of several castes, each one biologically differentiated from the others and having its own social functions to perform in maintaining the welfare of the colony. Among termites, for example, there are six biologically specialized castes representing various divisions of labor; in the honeybee hive, the functions that are essential to the population are apportioned among the "queen" and the "drones"—the fertile female and the males, respectively, whose functions are chiefly reproductive—and the "workers," sterile females that obtain the colony's food and tend the hive and the young.

These parameters of population structure apply as well to human groups, but because of the cultural heritage superimposed on man's biological heritage, the relationships between social behavior and the properties of populations are more complex than in other animals. Even the most primitive human societies are distinguished by high levels of group interaction, with organized systems of cooperative behavior relative to defense and the division of labor. Unlike the biologically determined activities of the members of insect societies, the various social and occupational roles that arise in human societies are not rigidly set by biological endowments. Biological and cultural factors show an interdependence in respect to many roles. Not all persons are physically capable of becoming glassblowers, wrestlers, jockeys, or astronauts—to mention a few obvious categories; on the other hand, individual inclinations or the restrictions of the society prevent some persons regardless of their qualifications for a particular occupation from following it. Some roles are even less dependent upon biological constitution as shown by the fact that many occupations reserved for one sex or a certain age group or a given ethnic group in some societies belong to the opposite sex or different age or ethnic groups in other societies.

Human caste systems in which social and occupational status are assigned at birth are generally the by-product of historical conquests resulting in the arbitrary assignment of members of the conquered groups and their descendants to subservient roles. That the cultural division of a human society into castes is in no way comparable to the biological distinction between castes in the organized societies of insects, is clearly demonstrated by the ability of individuals to take on the roles of other castes once the barriers are down. In modern India, after centuries of restrictions on the development of the individual, some members of the former lower castes are able to fill the traditional occupations of the upper caste, while some individual members of the latter are recognizably unsuited to the tastes and social functions which previously would have been theirs by right of birth (Dobzhansky, 1962).

Human societies develop various marriage patterns that can affect the genetic structure of the population. A condition that could result in genetic selection for specific traits would exist, if there were large-scale occurrence of assortative mating—the marriage of phenotypically similar individuals with differential rates of reproduction for the various classes of assortment. To the extent that trait expression is genetically conditioned in a given environment, assortative mating increases the probability of bringing together individuals having similar genetic correlates of such a trait and of their producing similarly endowed offspring. Although assortative mating occurs in human societies with some frequency, the trait on which the choice is based is not always apparent. It was noted earlier in the chapter that intelligence is considered to be one basis for assortative mating. Actually, it would probably be more accurate to say that people tend to choose marriage partners of comparable educational levels, and that insofar as intelligence and educational level are crudely correlated, mate selection bears a measure of relationship to intelligence. Restriction of marriage to members of one's own occupational caste might seem at first glance to induce assortative mating for behaviors related to the performance of given tasks. A second look, however, reveals that this is not so; a caste originally composed of behaviorally diverse individuals retains its genetic variability. In fact, caste systems, by preventing marriage between like phenotypes across caste, tend to restrict rather than facilitate assortative mating (Dobzhansky, 1962).

Groups that do not intermarry with others are designated as *population isolates.* Isolates may be either geographically remote communities or cohesive groups whose religious, social, or historical traditions keep them apart within the framework of a larger population. In either case, isolates are precluded from appreciable genetic exchange with other groups. The frequency of marriages between near relatives is generally higher in small isolates where the choice of mate is limited. By increasing the probability that marriage partners will carry alleles derived from a common ancestor, consanguineous marriages also increase the likelihood that rare recessive alleles of a given gene may be brought together in the offspring. This is the individual effect of consanguinity. The population effect of numerous cross-matings of individuals descended from a common ancestor may facilitate natural selection against an allele which is deleterious (in terms of viability or fertility) in the homozygous state.

Almost all cultures restrict in some way the kind and degree of consanguinity sanctioned between marriage partners. What constitutes the "incest taboo" is different in different societies. Marriages between mother and son are, so far as known, universally abhorred (Klineberg, 1954), but a few cultures tolerate father-daughter matings. Brother-sister marriages are historically reported to have been not only sanctioned but required in some of the royal dynasties of the Egyptian Pharaohs, the Peruvian Incas, and the Hawaiians. Certain present-day communities in Brazil are said to tolerate, if not condone, such marriages (Willems, cited in Spuhler, 1954).

By and large, the consanguineous matings of greatest frequency and relevance to population structure are those between cousins. Some societies approve marriages only between second cousins and more remote degrees of relationship, which are of little genetic significance. Some permit any type of cousin marriage, others permit marriage only between cross-cousins, i.e., the children of a brother and sister, but not between parallel cousins (children of two sisters or of two brothers). With even finer discrimination, some societies ban cross-cousin marriages except those between a brother's son and his sister's daughter; in others, only the reverse kinship (brother's daughter and sister's son) is allowed in marriage. While the several types of first-cousin marriages do not differ in the transmission of autosomal genes, they do differ in terms of the degree of genetic correlation for sex-linked genes (Freire-Maia and Freire-Maia, 1961).

In modern populations, geographical and social isolates are disappearing and, with them, the frequency of consanguineous marriages is declining. One effect that the breakdown of isolates may be expected to have is the gradual dispersion among larger populations of alleles which were previously limited chiefly to restricted mating groups. At the same time, the frequency of individuals homozygous for rare alleles should decline.

METHODS AND DATA IN BEHAVIOR GENETICS

The understanding of a science entails an appreciation of its methodology—the various techniques it uses in the analysis of its problems—as well as the nature of the material it studies. Behavior-genetic analysis is, of course, twofold, employing the methods of both psychology and genetics. The discussion in this section will stress mainly the latter. Some of the investigations in which the various research methods have been employed are described in order to illustrate the range of problems that have already been subjected to behavior-genetic analysis.

Selection. Earlier we discussed natural selection as one of the evolutionary forces acting on a population and noted that selection refers simply to the fact that some individuals produce more viable offspring than others. *Artificial selection* is a breeding technique used by man to change the phenotypic composition of a population. It is a useful method if there are alleles of *additive* (noninteracting) gene combinations to be selected. Whether the intention is to arrive at a more productive stock of dairy cattle or a fiercer breed of watchdogs or a DDT-susceptible strain of insects, artificial selection involves choosing as parents for each generation only those individuals with the desired expression of the trait or traits, and discarding all those that do not exhibit a previously determined level of phenotypic merit. Artificial selection is assortative mating enforced on the basis of phenotypic similarity. Although progress under selection is measured by changes in the phenotypic value of the population, these changes are achieved and maintained through increases in the frequency of appropriate alleles in the gene pool.

When a character is controlled by a single gene, selection for a recessive phenotype can be completed within one generation, if it is possible to mate together only the recessive homozygotes. Selection for a dominant, on the other hand, proceeds more slowly because inclusion of heterozygotes in the breeding group at each generation retains the recessive allele in the gene pool. The rate of progress depends upon the relative frequencies of the two alleles at the onset of selection—the lower the frequency of the recessive allele, the slower the rate of its elimination.

Most traits of interest in behavior genetics are controlled by many genes acting in combination, with the different combinations of alleles showing different gradations of phenotypic expression. Beginning with a population of diverse behavioral types, artificial selection changes the phenotypic characteristics of the population by redistributing the allelic combinations in the gene pool of successive generations. To illustrate, let us consider a population of guinea pigs which is scored for speed of running a maze and is found to be phenotypically heterogeneous—some fast, some slow, and some intermediate grades of runners. As is most commonly done in selection experiments, we select for each extreme of the behavioral distribution. The "fast" line is initiated by mating the rapid runners, and generation after generation, mating only the fastest pigs among the progeny; selection of the "slow" line is carried out in similar fashion at the opposite end of the scale, and obviously the two lines are never crossed. We might decide also to keep an unselected, random-mating population as a control that will enable us to estimate the extent of phenotypic changes arising through chance. Now suppose that the running rate of an animal is a heritable trait (otherwise we could not change the population mean through selection) determined by several genes. We can designate the alleles that contribute to increased speed as "plus," and those involved in slow running as "minus." Under selection the average scores of the two selected lines should begin to diverge, as more and more "plus" alleles accumulate in the "fast" line and new combinations of "minus" alleles build up in the "slow" line.

Theoretically, selection ceases to have an effect when all the alleles of genes acting additively in one direction have become fixed in a line, i.e., when there is homozygosity at all of the relevant

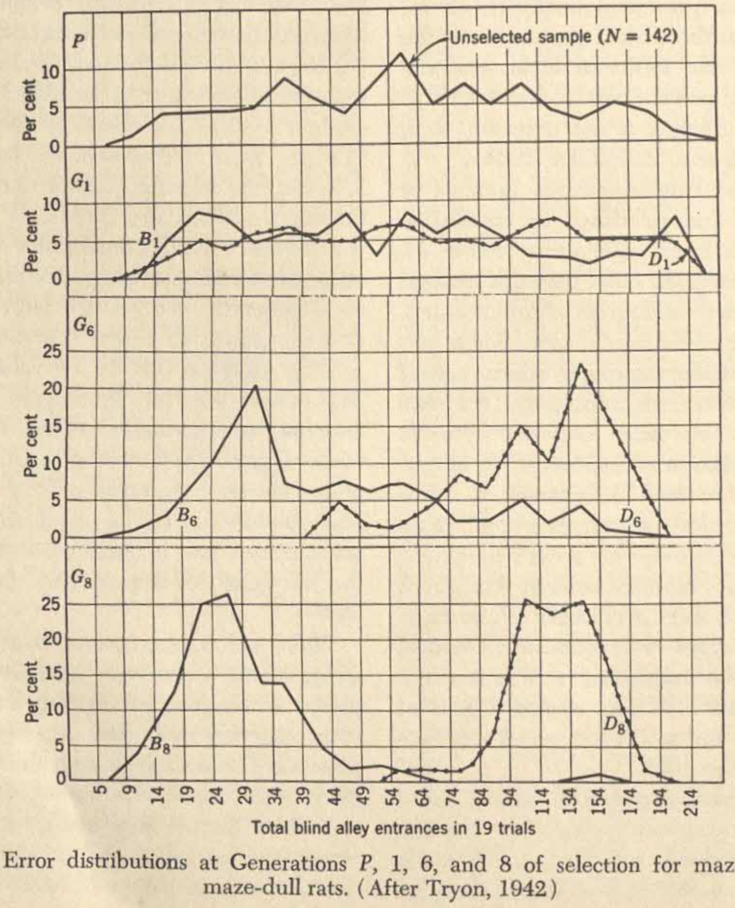


Fig. 3-6. Error distributions at Generations P, 1, 6, and 8 of selection for maze-bright and maze-dull rats. (After Tryon, 1942)

loci, and no further additive genetic variability remains. In practice, selection progress is contingent upon a number of factors. (1) Since permanent phenotypic changes depend upon modifications of the gene pool, we must be able to correctly identify and select for breeding the phenotypes that carry the desired alleles. To the extent that the correspondence between phenotype and genotype is not perfect—and we have seen that one-to-one correspondence is rarely to be expected for behavior traits—some identifications will be false, and undesirable alleles will be retained in the population. Population changes in the direction of selection will then be delayed. (2) The intensity of selection pressure that can be applied is important. If at the start we have very few extreme scorers, we have to include in the breeding group some less extreme phenotypes (presumably with fewer of the appropriate alleles) to get a sufficient number of offspring in the next generation. In doing so, we may be diluting the intensity of selection. (3) Natural selection, too, may operate against extreme types, making them less viable (or less fertile), and thus countering the effectiveness of the artificial selection. (4) Many other factors may begin to operate long before genetic variability is dissipated and all of the alleles have become fixed. For example, phenotypic selection is partially limited by a scoring scale with very few categories, which restricts the possibility of discriminating between phenotypic variants. Physiological and anatomical limitations of the organism may be important considerations; in selecting guinea pigs for speed of running, how fast could any pig be expected to run?

In behavior genetics, selection studies are used to estimate the heritability of a trait[3] in a population and to tailor populations for further, more detailed genetic analysis. The selective breeding method has been used for studying a relatively wide range of behaviors. Investigators working with populations of rats or mice have succeeded in developing lines selected for: maze-learning ability and other aspects of rodent "intelligence" (see Fig. 3-6); "activity" versus "inactivity," scored by the amount of motor activity in a revolving wheel; "emotionality" versus "non-emotionality" measured by the amount of excretion in an open field; and susceptibility versus resistance to audiogenic seizures, i.e., convulsive behavior in response to auditory stimulation. These and other selection studies are described in greater detail by Fuller and Thompson (1960). *Drosophila* populations have been selected for phototactic behavior, [approach or non-approach to a light source (Hirsch and Boudreau, 1958)], speed of mating (Manning, 1961), and geotaxis, [the negative or positive response to gravity as measured in the vertical mass-screening maze shown in Fig. 3-7 (Dobzhansky and Spassky, 1962; Erlenmeyer-Kimling et al., 1962)]. Fig. 3-8 presents the results of sixty-five generations of forward selection for geotaxis starting from an unselected foundation stock. It also shows twelve generations of reverse selection (selecting the most positive flies from the negative line, and vice versa) starting at generation fifty-two of forward selection. The extent of the response to selection together with the prolonged and gradual nature

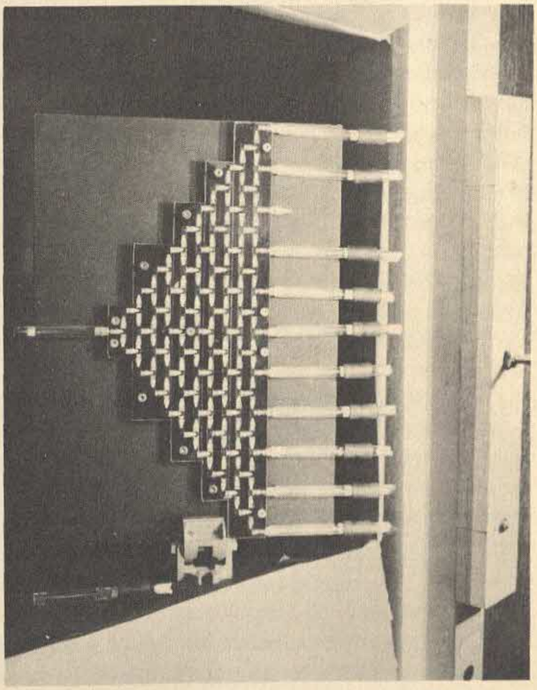


Fig. 3-7. Photograph of ten-unit, vertically placed, mass-screening, geotaxis maze facing fluorescent tube. Large groups of flies can be introduced in vial at left and collected, automatically classified in a frequency distribution in vials at right, to which they are attracted by food and light. Small trap-like funnels discourage backward movement in the maze. (From Hirsch, 1959)

---

[3] There are, however, some genetic situations, e.g., nonadditive gene relations, that cannot be adequately studied by this method.

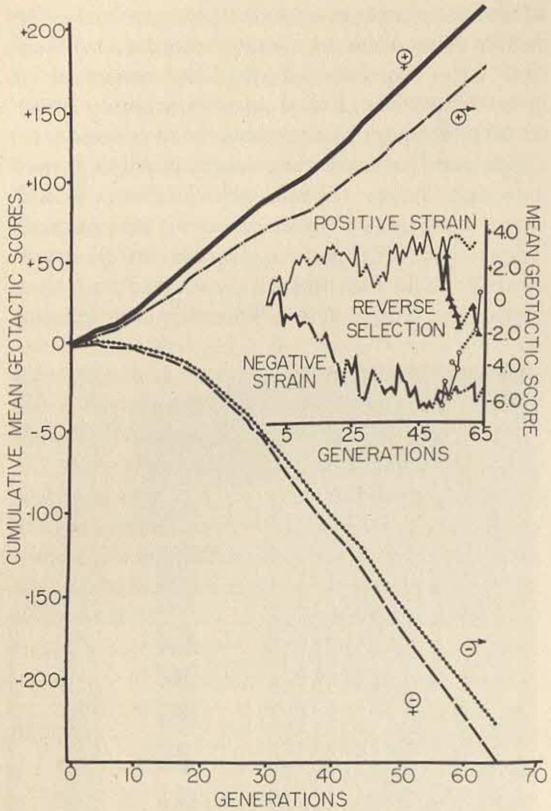


Fig. 3-8. The difference between males and females in mean response to selection for geotaxis (cumulative curves). The course of the response to selection and reverse selection; both sexes combined (average curve insert). (From Erlenmeyer-Kimling, Hirsch, and Weiss, 1962.)

of the strain divergence shows that a polygenic system is involved. In several studies it has been found that the gains accomplished by selection are lost when selection pressure is relaxed. These findings together with the rapid response to reverse selection suggest, "though (do) not completely prove, that the genetic variance was in part not simply additive." (Dobzhansky and Spassky, 1962, p. 1711).

Inbreeding and Consanguinity Comparisons. Inbreeding, as we have noted previously, is the mating of individuals more closely related than average. In the laboratory where the mating system can be controlled, inbreeding provides a means of increasing the genetic homogeneity of a population. For cross-fertilizing organisms, intensively inbred strains offer the best approximation to the "pure" strains obtainable in species that reproduce by self-fertilization.

The intensity of an inbreeding program, of course, depends upon the genetic relationship between the breeding pairs. While a parent and offspring share almost exactly 50 per cent of their alleles, the degree, or coefficient, of genetic relationship between any other two individuals (except identical siblings) expresses the proportion of alleles that they are likely on the average to have inherited from common ancestors. Although the genetic relationship between siblings will vary from zero to unity, it will average 50 per cent, because, on the average, they share about one-fourth of their maternal and one-fourth of their paternal alleles. Other coefficients of average relationship are:

Between uncle (or aunt) and niece (or nephew)	25.0 per cent
Between grandparent and grandchild	25.0 per cent
Between first cousins	12.5 per cent

With one form of intensive inbreeding program, entailing continuous brother-sister matings, homozygosity at most loci is theoretically expected within about twenty-four generations (Fuller and Thompson, 1960, p. 82); with matings of less closely related individuals, the progress is correspondingly slower. In practice, advantages enjoyed by heterozygotes, in terms of superior viability or fertility, may sharply limit the rate at which homozygosity increases in the strain. Even if homozygosity is attained, phenotypic variability does not necessarily disappear; in fact there is evidence suggesting that homozygotes are frequently more variable than heterozygotes, possibly because the latter are better "buffered" physiologically against minor fluctuations in their developmental environment (Lerner, 1954). Inbreeding, then, increases the probability (not the certainty) of genetic similarity at most loci among the individual members of a population.

As experimental tools, inbreeding and artificial selection are similar in several ways. Both are planned assortative mating systems—selection being assortment based on phenotypic merit (together with differential rates of reproduction for the classes of assortment) and inbreeding being assortment based on genetic relationship. After the first few generations, moreover, selection experiments almost necessarily involve some degree of inbreeding within a selected line (especially in very small breeding groups) unless precautions are taken to avoid matings between closely related animals. Without selection, on the other hand, the effect of genetic assortment is unpredictable in respect to the fixation of a particular allele at any given locus. Selection and inbreeding are useful techniques primarily because they provide material for further genetic analysis. In artificial selection, matings are based on phenotypic similarity within a line, and further breeding experiments can be performed to study the genetic basis of the phenotypic differences between lines. Different inbred strains known to be genetically unlike can be compared and analyzed for phenotypic differences existing between them. Comparisons of inbred strains subjected to a range of environmental treatments yield data on heredity-environment interactions. Inbred strains thus play an important role in experimental behavior genetics. In other areas of psychology, too, e.g., psychopharmacological and psychophysiological studies, inbred strains are frequently used because they offer relative control over one complex set of variables.

Man, of course, is not a laboratory animal to be subjected to breeding experiments. In lieu of laboratory-developed inbred strains, however, valuable resources for the study of human genetics are provided by geographical and social isolates with their comparatively higher incidence of inbreeding, and by whatever consanguineous marriages occur in larger populations. We have seen that social customs in different cultures influence the degree and type of consanguinity permitted between marriage partners. Parent-offspring and brother-sister matings are so seldom encountered in human populations that they are of limited general value for research. Likewise, aunt-nephew and uncle-niece marriages, although reported with more frequency than the unions of closer relationship, are rare in most societies. Marriages between cousins are more common, and in some societies the proportions of such marriages reach high values—for example, almost 30 per cent of all marriages among the Fiji Islanders, and about 13 per cent among the Parsee caste in India, have been reported as occurring between first cousins (Stern, 1960, p. 380).

Usually, the effect of a dominant allele is phenotypically evident in any individual who carries it, and it can be followed easily enough from generation to generation without resorting to studies of consanguinity. It is for the recessive alleles that consanguineous marriages yield important data. With random mating, the likelihood that any individual will be homozygous for a given recessive is equivalent to q^2, where q represents the frequency of the allele in the population. If the value of q is high, the probability is also high that two unrelated carriers of the allele will meet, marry, and produce some homozygotes among their offspring. The allele for blue eye color would be an example of a recessive which is common in populations of Northern Europe, but rare in African and Asian populations; another allele which is relatively common in many populations is the one producing PTC taste blindness. While consanguinity has no very remarkable effects as far as common alleles are concerned, the story is quite different for rare recessive alleles. On the average, first cousins may be expected to have inherited one eighth of their genes from a common ancestor (this means that they have a one-eighth probability of carrying the same allele at a given locus). That is, the cousin of a carrier of a very rare allele has a much greater chance of also being a carrier than does an unrelated individual taken at random from the population. Correspondingly, the offspring of a marriage between two such cousins are more likely to be homozygous for the allele than are the children from a mating between nonrelatives.

In genetical research, the observation of high consanguinity rates among the parents of individuals that show a particular rare condition contributes to the evidence for the involvement of a recessive allele. Of course, the importance of consanguinity for a given condition has to be evaluated in relation to the frequency of consanguineous marriages in the population. In a population where only 1 per cent of the marriages are between cousins, if 25 per cent of the affected individuals are offspring of cousin marriages, the hypothesis of recessivity is supported (although it is not confirmed). On the other hand, in a population where 30 per cent of the marriages are between cousins and there is a 25 per cent frequency of consanguinity in a given condition, consanguinity data in themselves yield no especially useful information.

Behavioral abnormalities accompany a number of recessive conditions in which increased consanguinity rates are noted. For instance, three rare metabolic disorders—phenylketonuria, Tay-Sachs disease (juvenile amaurotic idiocy), and galactosemia—result, almost invariably in the case of the first two and very frequently in the case of the third, in severe mental deficiency. Increased parental consanguinity rates are found for all three disorders. A large proportion of individuals afflicted with Tay-Sachs disease, which also produces multiple sensory defects, are the offspring of Jewish parents who trace their ancestry to a particular region of Eastern Europe (Aronson and Volk, 1962). Several neurological disorders with major behavioral involvements, certain forms of hereditary deafness, congenital blindness, and total color blindness also show higher incidences of consanguinity than expected in the general population (Stern, 1960).

Besides providing valuable information about recessive inheritance, consanguinity data may also yield, by more sophisticated mathematical analyses, estimates of the frequency and mutation rate of a given allele in the population, as well as the number of genes involved in the determination of a given trait (Steinberg, 1962).

Strain and Race Comparisons. Many of the data which have been gathered in behavior genetics come from comparisons of inbred strains or of different strains or races occurring in nature. While some studies have been limited to the observation of behavioral differences between strains, other investigations have analyzed the genetic correlates of behavioral differences by cross breeding the strains and comparing the segregation ratios (see the next section) with those expected under certain genetic hypotheses.

From what we now know about genetics and the properties of populations, we should in fact be very surprised if reproductively isolated populations did *not* differ in some way on most phenotypic measures. While comparisons have traditionally dealt with average values, population differences need not always be expressed in terms of the central tendency of the scale distribution. Other moments of the distribution, e.g., measures of dispersion, skewness, and kurtosis, may in some cases prove more sensitive in the detection of dissimilarities between populations.

It is to be expected, therefore, that behavioral differences will almost always be found when inbred strains are compared. Some of the behavioral variations that have been observed in inbred strains of mice or rats include: fighting tendencies, emotionality or timidity (Lindzey et al., 1960), susceptibility to audiogenic seizures, maze running speed, and preference for alcohol (Rodgers and McClearn, 1959).[4]

Domestic breeds of dogs, although not as highly inbred as some laboratory strains of animals, differ in behavior as well as in the physical characteristics for which they have been selected (Fuller and Thompson, 1960). In domestic strains of rabbits, differences are observed in "maternal behavior," i.e., the time of nest building and treatment of the young (Sawin and Crary, 1953). In nature, reproductively isolated strains or races have developed divergent genetic backgrounds which influence behavior. For example, the "dance" by which the lightcolored Italian honeybees communicate the location of food sources to their hive has a pattern unlike that used by the darker-colored Swiss and Austrian strains (von Frisch, 1951).

Another kind of strain difference which has been carefully studied in honeybees is their resistance to foulbrood (a bacterial disease which infects the larvae and can spread through the entire colony). Part of the mechanism responsible for the superior resistance of one strain consists of the "hygienic" behavior of the worker bees which remove diseased larvae from the hive; workers of the susceptible strain, on the other hand, remove very few of the infected larvae. Crosses between the "hygienic" resistant strain and the "unhygienic" susceptible strain have, so far, been consistent with the hypothesis that there are two independent activities involved in the hygienic behavior: (1) uncapping of the cells in which the larvae are sealed, and (2) removal of the larvae from the hive. It appears that hygienic bees are homozygous for recessive alleles at two loci, one which determines uncapping and another which is responsible for the removal behavior (Rothenbuhler, 1958).

Races are populations of a species that differ in the relative frequency of various alleles in their gene pools. In a sense, it can be said that every isolate constitutes a "race," but the composition of the gene pool of a given isolate will be closest to that of groups of more recent common origin than to that of any others. Attempts to categorize human races have been based, at various times, on coloring and facial features, on skeletal structure, on blood types, etc. None of these classifications leads to clear-cut distinctions among human groups according to region of origin. Furthermore, the alternative systems do not produce consistent results. Whatever classifications are used to describe phenotypic or allelic frequencies in different groups, it is important to remember that populations, not individuals, are being described. Particular individuals in one race may more closely resemble those of another race in some characters than they resemble members of their own group.

---

[4] Other studies are cited and reviewed by Fuller and Thompson, 1960.

If human populations differ in their gene pools, we should expect to find some behavioral differences between them, just as we find morphological and physiological divergences between them and as we so easily find differences in the behavior of races and strains of other animals. The study of human racial differences employs the methods of population genetics; measurement of the character in large samples of different populations and estimation of the phenotypic distributions. Another approach to the study of human races makes use of hybrids, that is, individuals whose parents derive from two races.

Racial differences are well documented for at least two sensory conditions—color blindness and taste blindness. Studies of populations in many parts of the world show a wide range of allelic frequencies for the autosomal recessive that produces the deficit in tasting ability and also for the sex-linked alleles responsible for different kinds of color blindness. Among American whites approximately 30 per cent of the tested individuals were PTC-nontasters and about 8 per cent of the males were color-blind; 6-11 per cent of a tested Chinese population were nontasters and 7 per cent of the males were color-blind, while African Negroes had only 3-9 per cent nontasters and about 4 per cent color-blind males (after Boyd, 1950 and Stern, 1960). As we might expect, many relatively rare biochemical and neurological disorders vary in incidence among racial groups (Kalow, 1962). The frequency of the several forms of mental illness, too, has been reported to differ among populations; for example, in contrast to most other populations, the frequency of manic-depressive psychosis is reported to exceed that of schizophrenia in the mental institutions of India (Dhunjibhoy, cited in Klineberg, 1954). Caution is needed, however, in interpreting such findings, since the diagnostic standards in classifying the diseases and contrasting cultural attitudes toward particular behavioral expressions may distort the apparent frequencies of psychiatric disorders.

While the genetic correlates of sensory differences are fairly amenable to study, behavior-genetic analysis of intellective and personality characteristics is considerably more difficult because of their greater sensitivity to cultural influences. Test instruments for measuring "intelligence" or for scoring personality variables are highly dependent on cultural content, and their standards cannot be extended to populations with divergent environmental backgrounds. Attempts to develop "culture-free" tests are doomed at the outset, since no psychological test instrument can be free of cultural content. "Culture-common" tests (those that presumably depend on elements shared by the cultures of the various populations to be tested) might seem to offer a more promising approach to the problem, but attempts to formulate such tests have been relatively unsuccessful, possibly because items that are common to several cultures may not have the same salience in different cultures.

Segregation Analysis. Changing the distribution of a behavioral phenotype in a population through artificial selection or the measurement of behavioral differences between reproductively isolated groups provides evidence for the existence of genetic differences which may be amenable to further analysis. The nature of the transmission of the genetic correlates of a trait—whether one or many genes are involved, whether some alleles show dominance, etc.—can be discovered only through further breeding experiments entailing segregation analysis. The methods and principles of segregation analysis are those of transmission genetics; their application permits the testing of various genetic hypotheses.

A. *The single-gene hypothesis.* Many physical characters and some behavior traits can be grouped into a few clearly distinguishable phenotypic classes. Very often, the phenotypic differences are found to result from allelic substitutions at a single locus e.g., taste and color sensitivity in man and the waltzer behavior in mice. The single-gene hypothesis is tested by comparing the observed ratios of individuals in the different phenotypic classes with those expected under various possible dominance situations.

To illustrate, let us consider two inbred strains of mice, one of which consistently displays a behavior phenotype **A**, while the other shows a different form of the behavior **a**. On crossbreeding the two strains, the hybrid generation (called the

F_1 generation) behaves like the parental strain **A**. However, in the F_2 generation, i.e., the progeny obtained by intermating the members of the F_1, only three fourths of the animals have phenotype **A**, and the remaining one fourth resemble strain **a**. Now we see a familiar pattern emerging: if strain **A** is homozygous (**AA**) for a dominant allele, and strain **a** for the recessive form, the F_1 individuals are all heterozygotes (**Aa**) and should be like their strain **A** parents in behavior. Segregation of alleles in the cross of F_1 heterozygotes (**Aa** × **Aa**), would then yield three F_2 genotypes, one fourth **AA**, one half **Aa**, and one fourth **aa**, the first two being of the same phenotype. Further breeding tests help in considering this hypothesis. Some of the F_1 mice are mated with strain **A** and others with strain **a**. If the hypothesis is correct, the backcross to strain **A**(**Aa** × **AA**) should yield only **A** behavior, but the opposite backcross, to strain **a**, (**Aa** × **aa**) should give a 50:50 ratio of the two phenotypes. Intermating the **a** phenotypes of the F_2 or the backcross to strain **a** should produce all **a** individuals.

The phenotypic segregation ratios for a single-gene depend on the dominance relationships among the alleles. When neither allele is dominant, the F_1 individuals are intermediate in behavior between the parental strains, as are one half of their offspring in the F_2, and one half of the members of each kind of backcross. The ex-

TABLE 3-4

Expected phenotypic and genotypic ratios in experimental crosses under two genetic hypotheses

Hypothesis 1—Allele **A** dominant over **a**

	P_1 Strain **A**	×	P_2 Strain **a**
Phenotype	**A**		**a**
Genotype	**AA**	↓	**aa**
		F_1	

Phenotype	**A**
Genotype	**Aa**

	F_1	×	F_1	F_1	×	P_1	F_1	×	P_2
Phenotype	**A**		**A**	**A**		**A**	**A**		**a**
Genotype	**Aa**	↓	**Aa**	**Aa**	↓	**AA**	**Aa**	↓	**aa**
		F_1			B_1			B_2	
Phenotype		¾**A**:¼**a**			**A**			½**A**:½**a**	
Genotype		¼**AA**:½**Aa**:¼**aa**			½**AA**:½**Aa**			½**Aa**:½**aa**	

Hypothesis 2—No dominance (**I** is intermediate between **A** and **a**)

	P_1 Strain **A**	×	P_2 Strain **a**
Phenotype	**A**		**a**
Genotype	**AA**	↓	**aa**
		F_1	

Phenotype	**I**
Genotype	**Aa**

	F_1	×	F_1	F_1	×	P_1	F_1	×	P_2
Phenotype	**I**		**I**	**I**		**A**	**I**		**a**
Genotype	**Aa**	↓	**Aa**	**Aa**	↓	**AA**	**Aa**	↓	**aa**
		F_2			B_1			B_2	
Phenotype		¼ **A**:½**I** :¼**a**			½ **A**:½**I**			½**I** :½**a**	
Genotype		¼**AA**:½**Aa**:¼**aa**			½**AA**:½**Aa**			½**Aa**:½**aa**	

pectations from the various crosses, under the hypothesis of dominance and under the hypothesis of no dominance, are shown in Table 3-4.

B. *Two independent traits and correlation.* Some phenotypic characters seem to go together more often than not. The relationship between two characters may result from one of three possible genetic situations. It may be due to the multiple effects (pleiotropic activity) of a single gene, in which case segregation analysis will fail to break down the relationship. Herter and Sgonina's study (see Fuller and Thompson, 1960) of floor-temperature preference in three mouse strains shows a correlation between the behavior and the temperature of stomach fur and fur density; according to the genetic analysis, these pleiotropic effects appear to be controlled by a single gene with at least three alleles. Another type of association exists between traits determined by closely linked genes that occupy nearby loci on the same chromosome. Since linked genes usually remain together in gamete formation, it is often difficult to distinguish between pleiotropy and linkage. With sufficiently large numbers of subjects, however, it is possible to detect linkage when crossing-over takes place between homologous chromosomes. For example, if one strain is homozygous for alleles **AB** on the same chromosome and another for **ab**, the recombinations **Ab** and **aB** are likely to appear in some of the descendants of crosses between them. The frequency of crossing over and recombination between two genes depends upon the distance between them on the chromosome: the shorter the distance, the tighter the linkage and the less the likelihood of crossing over.

In a particular strain, traits determined by genes on different chromosomes may appear to be associated because certain combinations of alleles have come together in the homozygous state. Crosses between such strains permit the segregation and independent assortment of these genes to be observed in the F_2 and backcross generations. For instance, the hygienic behavior of worker bees in clearing the nest of diseased larvae is a genetically controlled character. Two activities—(1) uncapping the cells in which the larvae are sealed, and (2) removing the larvae—are necessary for effective hygienic behavior. Genetic analysis (Rothenbuhler, 1958) shows that, although the two activities generally go hand in hand in the hygienic strain and are absent in the unhygienic strain, they are actually independently determined. The F_1 cross between the strains shows neither of the components of hygienic behavior, but in the F_2 and backcrosses, four combinations of behavior are seen: (*a*) a group that neither uncaps nor removes, (*b*) those that uncap but don't remove, (*c*) those that don't uncap but do remove the larvae when the cells are uncapped for them (by the experimenter), and (*d*) those that uncap and remove like the parental hygienic strain. Here is a case where segregation analysis has revealed not only the mode of genetic transmission (both of the hygienic activities depend on recessive alleles), but it has also been able to show the independence of two activities which originally appeared to be parts of a unitary trait.

C. *Polygenic characters.* Most behaviors do not fit into a few sharply defined classes, but rather show a graded distribution of phenotypes e.g., maze learning, emotional reactions, and activity in rats; geotaxis, phototaxis, and mating speed in flies; and IQ in man. These traits are usually the result of the accumulated small effects of many genes. Polygenic characters may be studied by the same breeding methods used in the analysis of single-gene traits, but the treatment of the data differs. When working with polygenic systems we collect data in terms of mean and variance values of the phenotypic distribution, rather than considering the proportions of individuals observed and expected in a few discrete classes.

The simplest illustration of the polygenic model can be provided by considering a trait determined by the combined effects of genes at two unlinked loci. In a cross between two strains, each homozygous for a different allele at the two loci, the F_1 should be uniformly heterozygous, showing no genetic variance, and with a mean score value intermediate between those of the two parents, assuming no dominance or epistasis. Segregation in the F_2 produces a range of scores with, theoretically, a mean identical to that of the F_1. The backcrosses of the F_1 to the parental strains are expected to be more variable than the F_1 but less variable than the F_2, and to have mean values lying between those of the F_1 and of the parent strain to which the backcross is made. With dominance or epistasis, the expectations regarding the mean value in the various generations are correspondingly altered.

Further analyses of data on crosses between strains that differ in polygenic traits are carried out by means of more sophisticated statistical procedures which entail partitioning of the phenotypic variances into genetic and environmental components. These calculations allow estimation of the trait heritability in the tested populations and also permit estimation of the number of genes involved.

The majority of behaviors mentioned in the discussions of selection and inbreeding appear to be polygenically determined. In a few studies, attempts have been made to estimate the number of genes operating in the polygenic system. For example, using two inbred strains of mice, Thompson and Fuller (1957) estimated that exploratory activity as measured in one type of situation is controlled by about seven pairs of genes. Certain morphological characters are shown to be influenced by many genes. In *Drosophila,* for instance, as many as six gene pairs in one chromosome alone (Breese and Mather, 1957) influence the number of abdominal bristles formed, and all of the other chromosomes are known to contain genes which are effective in this respect too. There is good reason to expect that at least part of the variance in almost all behaviors will prove to be related to the polygenic diversity that is characteristic of all bisexual species.

D. *Segregation analysis and human behavior genetics.* Although experimental crossbreeding is precluded in studies of human traits, data for segregation analysis may be gathered on the offspring of various types of matings occurring in a population. The proportions of PTC tasters and nontasters, for example, can be determined among the children of the three mating types (taster-taster, taster-nontaster and nontaster-nontaster). Many relatively rare conditions—deviations from "normality" as seen in mental deficiency and mental illness—are not so readily located by random sampling. Studies of such conditions face possible biases which create difficulties for the testing of genetic hypotheses. For instance, families with several affected individuals have a greater chance of being located than do those in which only one member is affected. To overcome this and other types of bias, special mathematical methods have been devised (see Morton, 1962).

Diallel Crosses. A mating scheme that is of value for the study of quantitative traits provides means for analyzing the existence and degree of additive, dominance, and epistatic effects, maternal effects (cytoplasmic effects or the influence of prenatal factors), and interaction between environmental fluctuations and genotypes. This is the diallel mating system which involves all possible mating combinations between several genotypes. In one version of the diallel cross each member of a set of males is crossed in turn with each member of a set of females. Consider the case with two males, S_1 and S_2, and two females, D_1 and D_2. The resulting offspring can be designated as: S_1D_1, S_1D_2, S_2D_1, and S_2D_2. Offspring within each of these combinations, say a litter of mice or a clutch of chickens, are full sibs. Offspring from combinations S_1D_1 and S_1D_2 (or from S_2D_1 and S_2D_2) are paternal half sibs, those from combinations S_1D_1 and S_2D_1 (or from S_1D_2 and S_2D_2) are maternal half sibs; S_1D_1 and S_2D_2 are unrelated.

Another version of the diallel cross method involves all possible matings between two or more inbred lines, including matings within lines and reciprocal crosses between lines. With three inbred lines, the numbers of possible mating combinations is nine, and in general with n lines there are n^2 combinations. The lines used in such crosses are assumed to be randomly sampled from some general population (Hayman, 1954).

In either case, by partitioning the variance of measurements made in offspring in the various mating groups, it is possible to isolate nonadditive genetic components of variation. For polygenic inheritance, diallel crosses provide a powerful analytic tool yielding data otherwise difficult to obtain. Another advantage of this breeding method is that estimates can be obtained from the F_1 generation without the need for subsequent crosses, although additional confirmation can be provided from F_2 or backcross data. Broadhurst and Jinks (1961) present a reanalysis in the diallel framework of some well-known behavior genetic studies.

Chromosome Analyses. The methods so far considered yield information about heritabilities, i.e., the extent to which the phenotypic variance (individual differences in behavior) is correlated with the genetic variance (genotypic diversity) in a population. In some species further analysis is possible, and the genetic variance can be partitioned into components assignable to the individual chromosomes. In some organisms, breeding procedures permit chromosomes and their parts to be juggled almost at will. Thus, chromosome pairs can be used as independent variables and genotypes can be synthesized.

In well-mapped species like *Drosophila melanogaster* the chromosomal locus is known for many specific genes that control the phenotypic expression of easily discernible traits. Such dominant genes can be used as "markers" to label chromosomes, whose presence or absence in an individual can be inferred from the expression of the trait. The use of genetic markers, chromosomal inversions, and balanced lethal systems permits homologous chromosomes of different populations under study to be compared with one another by means of a common standard of reference. Inversions effectively inhibit crossing over and gene recombination in structural heterozygotes because they impede the pairing of homologous loci at meiotic synapsis. Inversions preserve the integrity of the chromosomes from the population under study, and the morphological markers make it possible to follow the segregation of specific inversion chromosomes in crosses. Some genes are lethal when in a homozygous state. When two nonallelic, homozygous lethal, dominant genes are used as markers—one on each of the homologues of a pair of inversion chromosomes—a balanced lethal system is created. An example of such a system would be a chromosome pair having the genes **Ab** on one homologue and the genes **aB** on the other, where both **A** and **B** are dominant and homozygous lethal. A mating between two heterozygous individuals of this type (**Ab/aB** × **Ab/aB**) can produce three possible genotypes: **AABB**, **aaBB**, and **AaBb**. Only the last of these is viable. Hence, a balanced lethal system provides a mechanism for fixing a chromosome pair in a population.

In one study (Hirsch, 1959), a stock carrying inversion chromosomes maintained with balanced lethal systems was crossed with an ordinary laboratory stock to produce three populations differing in known ways with respect to both the kind and amount of free genetic variance. One population was made *isogenic* (genetically uniform) for the *X* chromosome, with the autosomes left to random assortment; another was made isogenic heterozygous for Chromosome *II* with Chromosomes *X* and *III* left to random assortment; and the third population was made isogenic for both the *X*

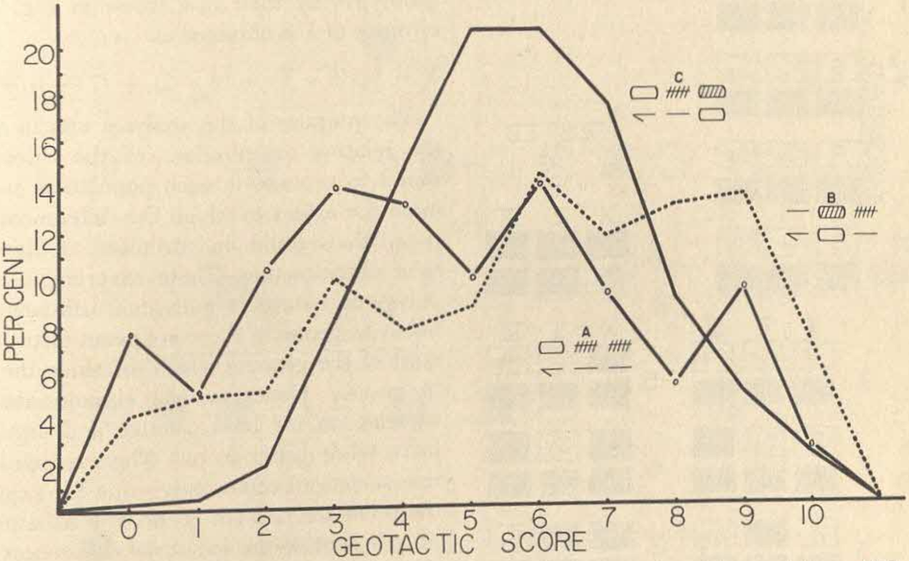


Fig. 3-9. Distributions of geotactic scores for males in three experimental populations. When a chromosome is represented by a block, either hatched or plain, every member of the population in which it occurs shares the identical chromosome. When a chromosome is represented by a line, hatched or not, the genetic composition of that chromosome varies randomly throughout the population. Hatching signifies that the two chromosomes in a pair are not homozygous, i.e., presumably they are largely heterozygous. Lines with half arrowheads represent the Y chromosome. (From Hirsch, 1959)

chromosome and a heterozygous third chromosome pair, with Chromosome *II* left to random assortment. Comparison of the response distributions of the three populations (labelled *A*, *B*, and *C* respectively in Fig. 3-9) in the geotaxis maze showed differences with respect to both central tendency and dispersion. Restricting free genetic variation to a single chromosome pair, in population *C*, reduced the behavioral variance to about 50 per cent of that in the populations which had free genetic variation in two major chromosome pairs. This experiment demonstrated the feasibility of studying behavior in populations and also of analyzing the genetic correlates of distributions of individual differences.

Later experiments (Hirsch and Erlenmeyer-Kimling, 1962) analyzed the three major chromosome pairs from a freemating *Drosophila* population and the two populations derived from it through selection for positive and negative geotaxis. Each of the three populations was crossed with a special laboratory stock carrying marker genes and inversion chromosomes in a balanced lethal system. Backcrossing the F_1 to the population under test yielded eight morphologically distinguishable classes of backcross progeny differing with respect to the combinations of structurally heterozygous (marker-test) and structurally homozygous (test-test) chromosome pairs they carried (see Fig. 3-10). The effect of each chromosome on geotactic performance was estimated by comparing the maze distributions of the structurally homozygous backcross classes with the distributions of the classes that were structurally heterozygous for the same chromosome. The difference in mean geotactic score between these two groups furnished an estimate of the increase in the degree of positive or negative geotaxis produced by the given tested chromosome over that of its homologue in the marker population. To illustrate, the effect of the *X* chromosome from the tested population was estimated by the average difference between the means of the appropriate backcross genotypes. Representing the mean scores of these genotypes by their row labels in Fig. 3-10, the estimate of *X* is obtained as:

$$X = \tfrac{1}{4}[(5-1)+(6-2)+(7-3)+(8-4)]$$

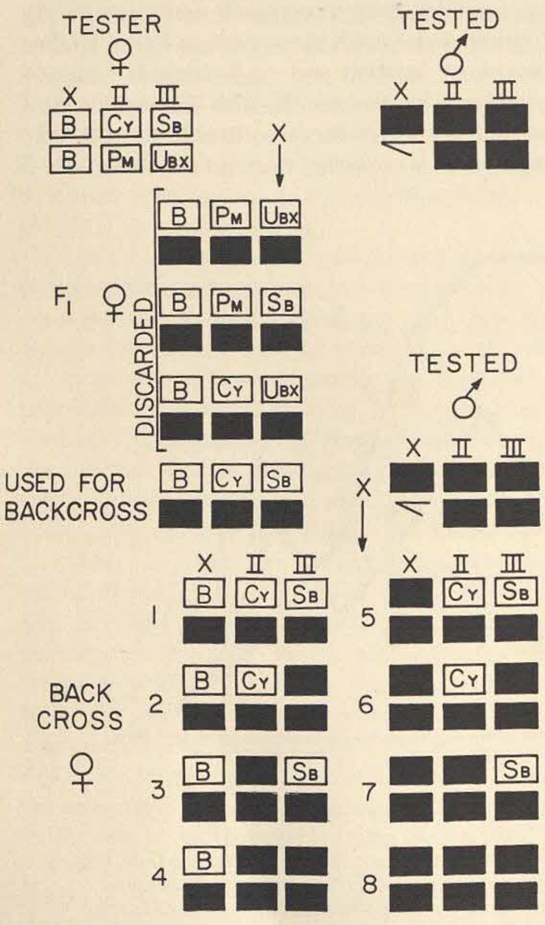


Fig. 3-10. Breeding plan for chromosome analysis. Letters in the white blocks represent marker genes. (From Hirsch and Erlenmeyer-Kimling, 1962)

The purpose of the analyses was to determine the relative contributions of the three chromosomes to geotaxis in each population and to estimate the extent to which the differences between populations could be attributed to differences in their chromosomes. These experiments show the polygenic nature of individual differences in the behavior geotaxis; there are genes distributed over most of the genome which influence the response to gravity. Genes on one chromosome respond to selection for both positive and negative geotaxis; while genes on two others respond to selection in one direction only—one for negative and the other for positive geotaxis. Furthermore, over 40 per cent of the measured differences between the selected lines was due to the genetic changes produced by selection.

Single-gene Comparisons. Mutant genes that are known for their morphological effects may also have pleiotropic effects on behavior. Breeding techniques that place mutant genes in a uniform genetic background permit the study of their contributions to differences in behavior. Essentially, the breeding methods are similar to those described in the preceding section, although numerous successive backcrosses are required to ensure an isogenic background on which the effects of allelic substitutions at the locus of interest may be assessed.

In *Drosophila,* strains bearing mutant genes are found to differ from each other and from nonmutant stocks in mating activity (vigor of courtship, frequency of mating, etc.) and, consequently in their acceptability as mating partners (Bastock, 1956; Reed and Reed, 1950); similar findings are reported for mutant strains in various moth species (Caspari, 1958). One type of *Drosophila* mutant showed decreased mating activity in the light, while in the dark they equalled or even exceeded the control stock (Rendel, 1951); in this situation, "it is the 'reaction norm' of the organism that is influenced by the gene substitution" (Caspari, 1958, pp. 112-113). In another study mutant strains selected different environments when given access to all possible combinations of light-dark, cold-warm, and humid-dry conditions (Waddington et al., 1954). The effects of single genes have been observed also in the phototaxis response of *Drosophila melanogaster* (Médioni, 1961; Scott, 1943). Many of these latter investigations have used different eyecolor mutants, and in some cases the response has been studied at different wave lengths.

Single-gene effects on behavior have been observed in mammals also; an outstanding illustration is the series of mutants in mice (e.g., the "waltzer" and "shaker" genes) which produce behavioral correlates by their direct action on the nervous system (see Fuller and Thompson, 1960, pp. 137-139).

Pedigree Analysis. One of the most direct methods of studying single-gene traits in man is through the analysis of family pedigrees. The pedigree in Fig. 3-11 illustrates the transmission of an autosomal dominant. For a recessively inherited condition, only those individuals who are homozygous for the recessive allele will be affected. While it is possible to work backward from the affected homozygotes to specify certain ancestors as heterozygous carriers, it is never possible to infer that other members of the family group were not carriers. A pedigree of recessive sex-linkage was given earlier in Fig. 3-2.

Pedigree analysis is of value when details are

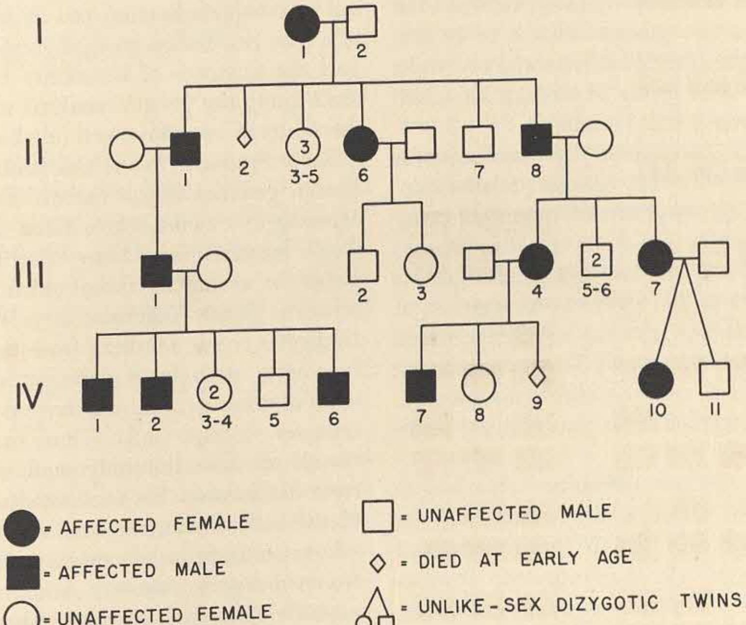


Fig. 3-11. Hypothetical pedigree of Huntington's chorea (adapted from data, courtesy of Dr. A. Falek) inherited as an autosomal dominant. The mean age of onset is thirty-five years for this neurological disorder which is often accompanied by psychiatric symptoms. All the above individuals are presumed to be beyond the age of onset.

available for very large kindreds, or when a number of family pedigrees showing the same condition can be brought together. Collections of isolated reports of pedigrees obtained from different sources, however, are of doubtful reliability. The errors in such collections may be of several kinds, for example: (*a*) testing and diagnostic procedures may not have been sufficiently standardized to ensure that precisely the same criteria were used by all investigators in evaluating the condition, and (*b*) nonsystematic investigations may ignore isolated cases and concentrate only on families in which a number of individuals show the given phenotype.

Family Correlation. Correlation coefficients can be used to estimate the degree of phenotypic similarity between individuals on any quantifiable dimension; for example, we could calculate the correlations in height, weight, and IQ-test performance between pairs of individuals chosen at random from the population. Note that the correlation now under discussion is between two classes of individuals measured on a single trait, where each individual in one class bears some specified relationship to an individual in the other class—such a correlation provides a measure of the extent to which the trait variation (discussed earlier) is a function of the relationship between the individuals in the two groups. This is to be distinguished from the correlation between two traits measured in the same group of individuals—such a correlation measures trait variability.

The phenotypic correlations between relatives can be compared with their genetic relationship. While theoretically, an observed parent-offspring or sib-sib correlation of .5 or greater is consistent with the interpretation that genetic factors play a major role in the phenotypic variance, it does not preclude the alternative interpretation that environmental factors may be very important (e.g., the intelligence of foster children sometimes shows a high correlation with that of their foster parents).

There are two serious difficulties attached to the interpretation of correlation data and its genetic significance. The first is that members of the same family generally share common environmental influences as well as common genetic factors, so that a high sib-sib correlation, for instance, can just as well be assigned to similarity of environment as to similarity of heredity. Attempts to overcome this problem have been made by studying children reared from a very early age in foster homes, and comparing fosterparent-child correlations, which are presumably purely environmental, with biological parent-child correlations (e.g., Skodak and Skeels, 1949). Unfortunately, such data are not available in great quantities and incur some biases of their own. Although the weight of evidence in these investigations suggests that a substantial portion of the correlation in intelligence test scores is contributed by heredity, conflicting data are reported from some studies. The second difficulty in evaluating the meaning of correlations is concerned chiefly with sib-sib measures and is related to the possibility of assortative mating in their parents. Insofar as assortative mating occurs to any appreciable extent, the genetic correlation between siblings would be expected to exceed .5, but what the value should be cannot be known without adequate information on the assortative mating tendencies. Family data may be used in the multiple-variance method developed by Cattell (1960) which endeavors to handle these problems statistically and through appropriate selection of various types of family relationship.

At most, correlation measures yield some preliminary information about human traits that cannot be satisfactorily analyzed by segregation methods. The correlation method, in general, may suggest the existence of hereditary factors related to some trait; the genetic analysis may then be furthered by more refined and reliable methods.

Twin Studies. One of the standard methods of human genetics makes use of the study of twins. *Monozygotic* twins derive from the splitting of a single fertilized egg. Since they have all of their genes in common, these twins are genetically identical (their coefficient of relationship is 1.0). *Dizygotic* twins, resulting from the fertilization of two eggs, each by a different sperm, have the same average genetic relationship as that between ordinary siblings (0.5). Thus, the underlying rationale of the twin study method is that phenotypic differences between monozygotic co-twins should reflect environmental (including prenatal) influences alone, while phenotypic differences between dizygotic co-twins should reflect the compounded effects of hereditary and environmental factors. Comparison of phenotypic similarities within pairs of the two kinds of twins should yield information about the heritability of a given trait in the population studied.

Qualitative characters—those that can be described by a few discrete phenotypic classes, such as eye color, blood type, etc.—are expressed in terms of the concordance and discordance percentages observed among pairs of twins. *Concordance* means that both members of a pair show the same phenotypic expression of the given trait. *Discordance* means that the co-twins do not both belong to the same phenotypic class. For quantitative characters, measured on a graded scale of values, e.g., personality test scores, the intrapair difference in score or the correlations in performance are calculated rather than concordance-discordance rates. In either situation, twin data are used to test for the presence of heritable variations. Two of the better known formulae for estimating heritabilities are illustrated in the following examples:

(1) *Qualitative characters.* If among 50 pairs of monozygotic twins, 48 pairs (or 96 per cent) were found to be concordant for microcephaly (a neurological disorder characterized by small head size and mental deficiency), and if 16 out of 50 dizygotic pairs were concordant (32 per cent), the heritability estimate for microcephaly in the population would be:

$$\frac{\%\ \text{Concordant}_{MZ} - \%\ \text{Concordant}_{DZ}}{100 - \%\ \text{Concordant}_{DZ}} = \frac{96 - 32}{100 - 32} = .94$$

(2) *Quantitative characters.* In a longitudinal study of senescent twins (Jarvik et al., 1962), the average intrapair correlation on a vocabulary test was .85 for monozygotics (41 female pairs) and .37 for dizygotics (26 female pairs). In this case the heritability estimate would be:

$$\frac{r_{MZ} - r_{DZ}}{1 - r_{DZ}} = \frac{.85 - .37}{1 - .37} = .76$$

Theoretically, a heritability estimate of 1.0 would indicate that all of the phenotypic variability of the trait (in the group studied and under the prevailing conditions) is attributable to genetic factors, while at the other extreme an estimate of 0.0 would attribute the entire observed variance to environment. It is reasonable to expect neither extreme, however, and in actual studies intermediate values are obtained. The interpretation of these heritability estimates entails a number of problems (Osborne and DeGeorge, 1959); in no case can the interpretation go beyond the demonstration that some hereditary factor plays a role, and in many cases, the value of heritability estimates alone is questionable.

By extending twin studies to include investigation of other family members of varying degrees of relationship, e.g., full sibs, half sibs and step sibs, the twin-family method (Kallmann, 1954) has the advantage of permitting more complete interpretations of the twin data in terms of intrafamily variations.

Another procedure involves the comparison of monozygotic twins reared apart with those reared together (Shields, 1962). Monozygotic co-twins brought up apart represent identical genotypes subjected to different environmental influences; their phenotypes are contingent on the genotype's norm of reaction. Unfortunately, studies of separated twins have led to misinterpretations, resulting in statements, all too frequently encountered in the literature of the behavioral sciences, to the effect that "environment is more important than heredity" or "heredity is more important than environment." Generally the interpretation of data from such studies is also hampered by basic sampling problems. Separated twins are difficult to locate for testing, and the few available cases do not make a satisfactory sample. While some estimate can be made for degree of environmental differences, such as years of schooling, family income, etc., qualitative differences in the environments of separated twins are rarely well treated.

Whatever special procedural approaches are taken, the question of zygosity in like-sexed pairs is crucial to the study of twins. Modern methods of zygosity determination depend on the analysis of a succession of characters in the search for differences which would establish that the co-twins are not monozygotic. Sometimes physical characteristics, such as facial features and coloring of hair and eyes, are sufficiently dissimilar to rule out monozygosity. In other cases more extensive tests from blood-group data and fingerprint analysis are needed. Monozygotic twins should be concordant for all of the blood groups; dizygotic twins may be concordant for many or all of the blood groups. The probability that two-egg twins will continue to demonstrate concordance decreases as the number of tested blood groups (ABO, M-N, Rhesus factors, etc.) is increased (unless the parents show unusual similarity in blood groups as might be the case in multiple consanguinity families). Fingerprint analysis is based on the fact that fingerprints conform to one of three basic types, each of which is largely determined by heredity: whorls, loops, and arches (Kallmann, 1959, p. 332), and that the total dermatoglyphic pattern is unique to the individual. Monozygotics, therefore, should differ very little in their fingerprint patterns, as classified according to one or more of several standard systems; differences greater than specified score values, which depend on the classification system, are indicative of dizygosity. When blood group and fingerprint findings are inconclusive, reciprocal skin grafting, a more drastic method, is decisive for zygosity determination, since the grafts will "take" permanently only in monozygotic co-twins. The methods and problems of zygosity determination are considered in detail by Osborne and DeGeorge (1959).

The twin-study method, in one or another of its forms, has been used for the investigation of a variety of human behaviors, notably intellectual abilities, personality traits, and patterns of aging and behavior pathology—including neurotic tendencies, alcoholism, homosexuality, suicide, mental illness (schizophrenia and manic-depressive psychosis), and mental retardation. Many of the significant twin studies are reviewed by Fuller and Thompson (1960) and Kallmann (1959).

Human Cytogenetics. A recent development in the study of human genetics, and one affecting the investigation of some of the human behavior pathologies, is the application of new techniques from *cytogenetics*, the direct study of chromosomes. For many years, the human chromosome complement was believed to number 48, until the report by Tjio and Levan in 1956, and immediate confirmation by other investigators, of a 46-chromosome count in normal human diploid cells. The improved methods of tissue culture which made this discovery possible have also been responsible for permitting increased accuracy in the identification of the 46 chromosomes and for the association of chromosomal anomalies with certain pathological conditions.

Chromosomes for microscopic examination are obtained in cells cultured from bone marrow, from skin, or from the leucocytes (white blood cells) of peripheral blood. The leucocyte method is generally preferred because of its greater simplicity. By appropriate culture techniques cell division is arrested at the metaphase stage of mitosis. Swelling of the cells, by addition of a hypotonic salt solution, spreads the chromosomes so that they are more easily distinguished as separate bodies. Chromosomes prepared for examination in this way are seen in Fig. 3-12; each body is a single chromosome in the process of dividing (at mitotic metaphase). An arrangement of chromosomes cut out of an enlarged photomicrograph, paired and numbered in order of decreasing size, is called an *idiogram* (Fig. 3-12, left side).

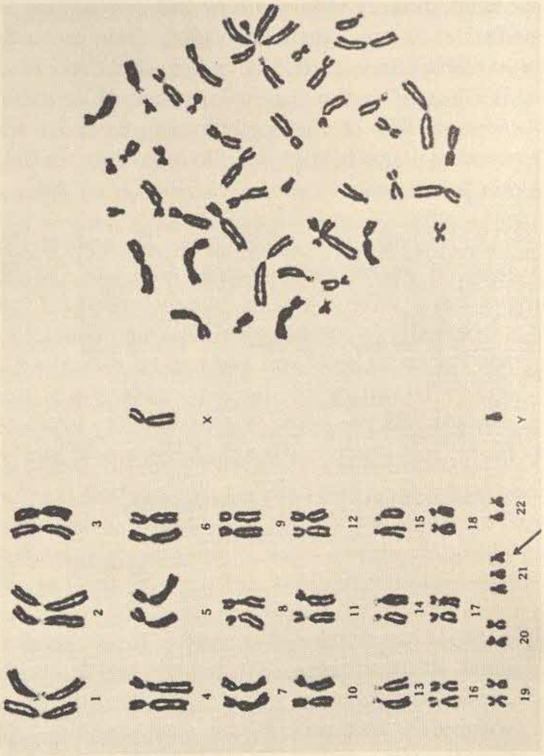


Fig. 3-12. Karyotype of mitotic chromosomes of a male with Down's syndrome. Note trisomy of chromosome No. 21 (see arrow). Without the extra No. 21, the karyotype would represent a normal male; in a normal female karyotype there would be 2 *X* chromosomes and no *Y*. Arrangement at left is an idiogram. Spread at right shows appearance of chromosome under microscope. (Courtesy of Drs. R. W. Breg and O. J. Miller)

For human behavior genetics, the new developments in chromosome studies hold great interest because of the demonstrated correlation between chromosome aberrations and certain conditions associated with mental deficiency. Chief among these is *Down's syndrome* (often called "mongolism") which occurs in about one per 650 live births (Bearn and German, 1961) and accounts for 5-10 per cent of individuals in institutions for the mentally subnormal (Benda, 1946, p. 4). In addition to their markedly low mental ability, these individuals are characterized by a number of recognizable physical features, for example: thickness and fissuring of the tongue, unusual patterning of the creases in the palms and in the soles of the feet, unusual shaping and positioning of the ears, and most notably flattening of the face and slanting of the eyes. The latter features led to the unfortunate designation of the syndrome as "mongolism" or "mongolian idiocy," and to the misconception that these individuals were "throwbacks" to the Mongolian race; modern preference is for the designation "Down's syndrome," named after Langdon-Down who first described it in detail.

For many years Down's syndrome represented a genetic enigma. Although it did not appear to follow a single-gene mode of inheritance nor a polygenic pattern, many facts suggested a heritable basis for the syndrome. In the first place, affected individuals physically resemble each other more than they resemble members of their own families, so that a biological (natural) unit seems to be indicated (Kallman, 1953). Furthermore, monozygotic twins are almost invariably concordant, while dizygotics are usually discordant (Allen and Baroff, 1955; Smith, 1960). Discordance of dizygotic twins would argue strongly against the possibility that the syndrome results from environmental disturbances affecting the fetus, as was frequently postulated. Although the occurrence of affected individuals is generally a unique event in a given family, some families do show increased incidence of the syndrome, and of the few reported offspring born to affected women, slightly less than one-half also carried the syndrome (Jarvik et al., 1964).

The possibility of a chromosome aberration as an etiological factor in Down's syndrome was suggested by Waardenburg in 1932, but it was not until 1959 that cytogenetic techniques permitted the confirmation of this hypothesis by Lejeune et al. (1959) and other investigators. Affected individuals were found to have 47 rather than the usual 46 chromosomes in their cells; examination revealed that they carried the chromosome 21 in triplicate instead of in duplicate. The chromosome trisomy is considered to be the result of nondisjunction occurring during meiosis. *Nondisjunction* refers to the failure of a pair of homologues to separate and be distributed to different gametes during meiosis; thus, in spermatogenesis, two of the resulting quartet will lack the chromosome entirely while the other two are likely to receive it twice. At fertilization, union of the gamete containing both homologues with a normal gamete produces a zygote carrying the chromosome in triplicate (see Figs. 3-12 and 3-13).

Since the original reports of an abnormal chromosome number in Down's syndrome, many more cases have been studied. Besides the cases of nondisjunctional trisomy, another type of aberration has appeared. This involves a reciprocal translocation (exchange of parts during meiosis) between chromosomes 21 and one of the other chromosomes. The translocation mechanism results in

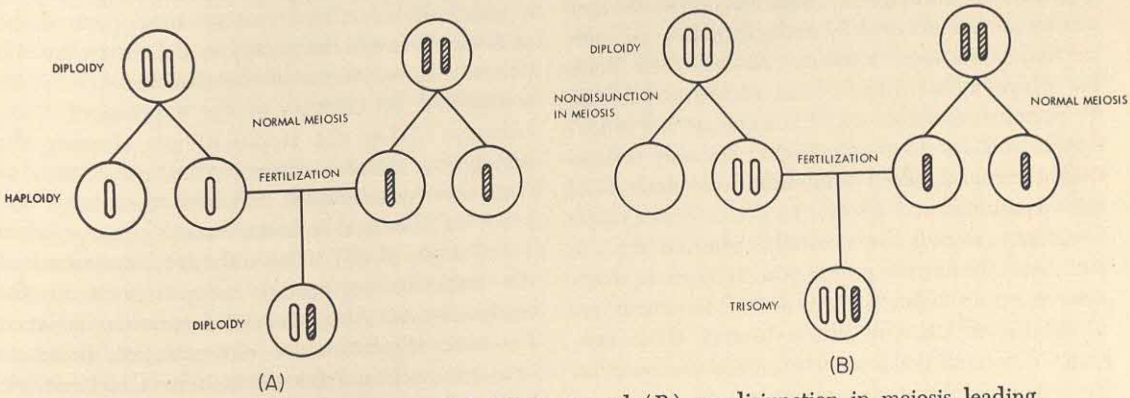


Fig. 3-13. (A) Typical meiosis and fertilization, and (B) nondisjunction in meiosis leading to trisomy of a chromosome.

the formation of several possible types of gametes, one of which will contain both translocation products. One product represents a joining of the major portion of the fragmented No. 21 to the major part of another chromosome. The second product, composed of the smaller parts of No. 21 and the other chromosome, apparently may be lost without serious damage. Union of the gamete containing the major translocation product with a normal gamete results in an individual with 45 chromosomes, but actually carrying almost all of the normal amount of chromosomal material. Since the gametes of these phenotypically normal individuals may contain both the translocation product and a normal No. 21, their offspring may have 46 chromosomes (two of No. 21 plus the translocation product that has most of the material from a third No. 21). Such persons have the normal species number of 46 chromosomes but nevertheless, an abnormal karyotype. They have Down's syndrome because they, like the nondisjunctional cases are effectively trisomic for chromosome 21. In most cases, one parent is found to be a translocation carrier with 45 chromosomes, and the aberration may be assumed to have occurred in a previous generation. Cytological examinations of families with affected individuals reveal that the translocation may be carried by a number of family members; for example, a family has been reported in which 14 members are phenotypically normal carriers with 45 chromosomes and six individuals with 46 chromosomes show Down's syndrome (Shaw, 1962). Other autosomal anomalies have been found to produce gross syndromes accompanied by mental deficiency, but they are much rarer than Down's syndrome.

Abnormalities of the sex chromosomes are also of interest. Individuals with *Klinefelter's syndrome* are phenotypically male, but they are sterile and often show testicular atrophy, etc. Their karyotype includes two *X* chromosomes and one *Y*. Sexually infantile, sterile females with *Turner's syndrome* have 45 chromosomes with only one sex chromosome, *X*. Both syndromes are products of nondisjunction and appear to occur with greater frequency among the mentally retarded than in the mentally normal population. It is particularly interesting to note that studies of the incidence of sex-linked color blindness among these individuals prompted the search for and discovery of the sex chromosome aberrations.

GENETIC PARSING, NATURAL UNITS, AND BIOCHEMICAL PATHWAYS IN DEVELOPMENT, BEHAVIOR, AND EVOLUTION

For the most part, behavior-genetic analysis has so far been limited to the identification of some genetic correlates of behavioral phenotypes. The information so obtained may now be used to analyze the relations between genes and behavior, i.e., the detailed study of the steps in the pathways from genes to behavior.

The conceptual key to the study of behavior-genetic intermediaries is supplied by the modern conception of the nature of gene action—the genes regulate metabolism through the activity of the gene-controlled enzymes. The methodological key to the study of this problem lies in the breeding techniques of classical genetics. Just as the parsing of a sentence provides an understanding of the sentence in terms of its elementary units and their organization, so, by means of the Mendelian techniques of hybridization and testcrossing, can behavior genetics accomplish an elegant dissection of the total organism under study and a subsequent reassembly of its component parts into an array of alternative organizations (Ginsburg, 1958).

Various metabolic reactions may be found to be correlated with behaviors. For example, it was once reported that cholinesterase concentration was linearly correlated with maze performance in rats (Krech et al., 1954). One way to ascertain whether a chemical-behavioral correlation is essential, and therefore indicative of an important mechanism, is by determining whether the two correlated traits segregate independently in a breeding experiment (Hirsch, 1962*a*, p. 29). If a metabolic reaction is an essential part of the pathway between the genotype and the behavioral phenotype, variations in the reaction will be accompanied by changes in the expression of the behavior. When the strains of rats showing the correlation between maze performance and cholinesterase were crossed and an F_2 generation was bred, the chemical-behavioral correlation vanished (McGaugh, 1959). The different expressions of the behavior segregated independently of the cholinesterase. Also, successful two-way selection for brain cholinesterase concentration failed to produce correlated changes in behavior (Roderick, 1958). It is now clear that gene-controlled metabolic reactions are natural units because they are the steps in the biosynthetic pathways used by organisms in the development of their structural and functional organization. The biochemical abnormality, phenylketonuria, with its behavioral implications, provides the prototype for our modern understanding of a natural unit (Beadle, 1946). Phenylketonuria is considered a natural unit because it is an abnormal allelic alternative in the gene-controlled link of a metabolic chain which has been produced by evolutionary selection pressure. Albinism, tyrosinosis, and alkaptonuria represent analogous situations. The last two involve adjacent links in a metabolic chain.

The nature of the relationship between genotype and phenotype is well illustrated by the four human conditions mentioned above. The concept of inborn errors of metabolism was introduced shortly after 1900 by Garrod with his recognition of a defect of enzymatic functioning in a condition called alkaptonuria. The condition is distinguished by a substance which causes the urine of affected individuals to turn black upon exposure to air and "has been estimated to occur in one out of 1,000,000 persons . . ." (Stern, 1960, p. 161). Normally, humans have a gene **A** which produces homogentisate oxidase, the enzyme that converts homogentisic acid into carbon dioxide and water. The rare mutant **a** in homozygous form, **aa**, does not produce the enzyme, or at least not in sufficient quantities, thus preventing the conversion of homogentisic acid.

Recessive homozygotes lacking the enzyme that transforms tyrosine into 3:4-dihydroxyphenylalanine (DOPA), a precursor of the pigment melanine, develop as albinos without pigment in their skin or hair. Homozygosity for a recessive allele at another locus causes a blocking in the pathway from phenylpyruvic acid to homogentisic acid. In this condition, called tyrosinosis, excessive amounts of tyrosine are produced and excreted in the urine.

Phenylalanine is an essential amino acid, a normal protein constituent. Higher mammals must obtain phenylalanine from their diets because they are incapable of manufacturing it. Since more phenylalanine is usually present in the diet than is required for protein synthesis, most of it is oxidized to tyrosine, itself a protein constituent as well as a precursor for thyroxine, epinephrine, and thyroxine pigments. The condition known as phenylketonuria is the result of homozygosity for a recessive mutant which prevents the enzyme phenylalanine hydroxylase from carrying out the normal oxidation of phenylalanine to tyrosine in the liver. Blocked from conversion into tyrosine, phenylalanine accumulates in quantities too great to be reabsorbed by the renal transport system and "spills over" into the blood plasma and cerebrospinal fluid in concentrations about 30 times the normal (Borek et al., 1950). Abnormally large amounts of phenylalanine derivatives, especially phenylpyruvic acid are excreted in the urine.

Although one hypothesis regarding gene action might be that the DNA in phenylketonurics determines synthesis of a protein with a different amino acid sequence than that of the normal enzyme protein (the altered and "erroneous" form causing the protein to lose its enzyme properties) the explanation of the primary defect in phenylketonuria may turn out to be more complicated (Harris, 1962). In normal individuals, phenylalanine hydroxylase is not formed until biochemical differentiation of the liver occurs after birth. Harris suggests that the primary defect in phenylketonuria may be a factor which determines when and where the complete enzyme system is formed, and that "the enzyme deficiency may well be a more remote consequence of gene action than is usually envisaged" (1962, p. 90).

Physically, the condition is usually represented by dilution of hair and skin pigmentation, a characteristic posture, etc. Behaviorally, most phenylketonurics exhibit some degree of intellectual impairment. Phenylketonurics account for slightly less than 1 per cent of institutionalized mental defectives in the United States and Western Europe. Exactly how mental development is affected by the inborn error in phenylketonuria is not yet known. Generally, it is assumed that the excessive accumulation of phenylalanine and its derivatives interferes with the normal metabolic functioning of the brain during early development. Detection of the condition within the first few weeks of life is now possible, and treatment in the form of an almost phenylalanine-free diet is effective in preventing mental deficiency.

The various biosynthetic chains and their links are being identified through the analysis of these and similar genetic deviations from what natural selection has produced as the norm. Since many of the same reactions, and possibly large segments of the chains as well, may be found in taxonomically related species, comparative studies can contribute to our understanding of the evolutionary history of these pathways. Thus selection, inbreeding, hybridization, and testcrossing provide the operations by which natural units can be defined and analyzed. They are "natural" because they have proven to be the raw materials of evolution.

RELATIONS BETWEEN EVOLUTION AND BEHAVIOR

Less than one hundred years ago, most of the moths of the various species found in the midlands of Britain were light-colored. The proportion of the once rare dark-colored forms has increased to the extent that they now constitute 95-98 per cent of the population in some regions (Ford, 1960). The sixteen unrelated species in which this change has occurred share a common behavioral characteristic and a common peril: they rest on tree trunks and they are the prey of birds. With the spread of industrialism and its sooty by-products, the dark-colored forms are afforded protective advantages lacking to the lighter organisms against the smoke-blackened background of their resting places (Kettlewell, 1956). The progress of the "industrial melanics," as they are often called, represents one of the most striking examples of convergent evolutionary change ever directly observed. We can analyze what has happened: A genetically variable population exists in which most individuals are light-colored, some carrying genes that produce melanin. Light coloration provides optimum protection against the light-colored barks; dark-colored individuals, when they occur, are more readily visible to predators and have a low survival rate, i.e., they are ill-adapted to the environment. Now the environment changes and the adaptive advantages of the two colorations reverse. The darkening background offers protection to darker organisms. Mutations or gene combinations that produce melanics are favored; such individuals survive and reproduce. The gene pool is changed. An evolutionary change has occurred, "a change in the hereditary characteristics of successive generations of a population" (Wallace and Srb, 1961, p. 9). In this case, the evolutionary change is adaptive, but it need not always be so.

The adaptation in the moth populations could just as well have been behavioral as morphological. Selection could possibly have operated on those variants which chose less exposed resting places. There are many illustrations of behavioral shifts, in the nesting behavior of various species of birds for example (see Tinbergen, 1960), that function as adaptations to environmental situations. In fact, ". . . the evolutionary changes that result from adaptive shifts are often initiated by a change in behavior, to be followed secondarily by a change in structure . . ." (Mayr, 1960, p. 371). Behavior is a factor in evolution as well as an effect of evolutionary change. The two questions, then, that must be asked about the relationship between evolution and behavior are (1) how have behaviors changed in the course of evolution? and (2) how does a behavior influence evolutionary processes?

Behavioral changes occurring in the course of evolution are studied by comparison of related species. We have seen earlier how the dispersal of the mouse, *Peromyscus maniculatus,* over a wide area has led to both behavioral and structural changes in the subspecies *bairdii* and *gracilis.* An excellent example of behavioral adaptation in *Drosophila* has been analyzed by Pittendrigh (1958). *Drosophila persimilis* occupies the cooler, wetter woodlands, while *D. pseudoobscura,* a closely related species, is distributed in warmer, drier woods. Experimental evidence reveals that the *pseudoobscura* population has "responded to selection stemming from desiccation hazards in a host of complementary ways, such as waterproofing of the cuticle, timing its activity [i.e., emergence from the puparium, flying, feeding, and sexual activity occur during the moist periods near dawn and sunset] and behaving hygropositively [i.e., selecting and approaching moisture] and photonegatively" (Pittendrigh, 1958, p. 400). That the negative phototaxis is part of the adaptive complex in maintaining the water balance is shown by the fact that increases in temperature or decreases in moisture intensify the photonegative reaction in *pseudoobscura* and produce photonegative tendencies in *persimilis* (which is generally photopositive). As Pittendrigh has pointed out, these "behavior patterns are adaptive *consequences* (in evolutionary perspective) of the difference in [ecological] distribution" (1958, pp. 410-411).

Some of the ways in which behavior shapes evolutionary changes are through social interaction with its effects on population structure, as we have already considered. Between populations the development of differences in habitat selection and in mating behavior is significant for other evolutionary divergences because of the fostering of reproductive isolation. Still another type of behavioral modification important to evolutionary progress is the kind of change which more directly favors structural changes. According to one modern view of human evolution the first stage was bipedal locomotion, then "came the use of tools, the hunting habit, increase in intelligence and, finally, the animal we know as man" (Washburn and Avis, 1958, p. 435). The interdependence of structural and behavioral changes is illustrated by the following hypothetical description of a sequence in human evolution: bipedal locomotion (structural and behavioral) was followed by the use of tools (behavioral); increased brain size (structural), resulting from new selection pressures created by the use of tools, made speech possible (structural and behavioral); slower growth rate (structural), resulting from increased brain size, produced longer periods of dependency, which were in turn associated with changes in the social life (behavioral) of the species.

CELL FUNCTIONING AND MEMORY

The genetic material can be considered to consist of a linearly arranged sequence of genes, which can be called the *genetic map.* Operationally, the linearity of this sequence has nothing to do with a physical entity. Evidence for it comes from recombination experiments of various kinds and is deduced entirely from the probabilities of obtaining various progeny ratios from specified genetic crosses. It is now known that DNA carries the genetic information and that DNA is essentially unidimensional, in the sense that the information encoded in it is presumably contained in the sequence of bases along the DNA molecule.

A synthesis of current theories about the genetic material asserts that there is a one-to-one correspondence between position on the genetic map and position along the polynucleotide chain of the DNA. Furthermore, it is assumed that the order and the relative distances of mutations along the genetic map is direct reflection of the order and relative distances of alterations in the polynucleotide chain. As we have seen earlier, DNA is assumed to control the primary structure of proteins, and experimental evidence indicates that some mutations do affect the primary structure of proteins. It is proposed that "messenger RNA," patterned on the DNA, carries the genetic information to the ribosomes, enabling them to fabricate a particular protein molecule.

Keeping in mind this review of a few fundamental concepts in modern molecular biology, we can turn to the consideration of some recent developments that have fascinating implications for the study of behavior.

The cells of the nervous system produce RNA and protein on a large scale. According to Hydén (1962), "This activity seems to be as characteristic of neurons as is the generation of bio-electric potentials and is intimately linked with the specialized nervous function." The production of nucleoproteins in the nerve cell is continuous. "Messenger-RNA" synthesized in the nucleus migrates into the cytoplasm where it combines with the ribosomes to produce protein. In turn, protein and other material are continuously transported down the axon by peristaltic movements (Hydén, 1960, p. 294).

The neuron in Hydén's view is structurally similar to an "enormous gland cell." Stimulation of neurons increases their production of RNA and proteins. In a series of experiments, rabbits and rats were placed on a circular disc and rotated at a rate which provided stimulation strong enough to subject the vestibular nerves to considerably increased demands, but slow enough not to damage peripheral organs (Hydén and Pigon, 1960; Hydén and Egyházi, 1962). Subsequent neurochemical analysis revealed a significant increase in the RNA production in the vestibular Deiters' nerve cells.

In later experiments, the RNA production in Deiters' cell was found to be even greater for rats required to learn to walk a tightrope to obtain food than that induced by rotation alone. Moreover, there were qualitative changes in the proportions of the RNA constituent bases: a relative increase in the production of adenine and a relative decrease in the production of uracil.

Hydén and his co-workers suggest that a pattern of nervous impulses is translated into an RNA molecular code which may provide the molecular basis for memory—that is to say, the engram may be stored in RNA as the cell's response to a particular frequency modulation resulting from sensory stimulation.

It is now believed that in highly differentiated tissue much of the DNA in a particular cell population is inactive. Factors in the cytoplasm repress large regions of the chromosomes, and as a result the DNA of those regions is not actively engaged in the synthesis of "messenger-RNA." When nervous impulses are generated by the stimulation of sensory cells or motor neurons, there results a particular frequency modulation which is expressed as an electrical pattern. The electrical pattern of each impulse that is transmitted affects the ionic equilibrium in the cytoplasm. In line with the recent evidence for DNA repression (Allfrey and Mirsky, 1962), Hydén makes the speculative, but interesting, suggestion that one consequence of patterned impulse conduction is the inhibition or inactivation of specific chromosomal repressors in the nerve cell: "When a given repressor . . . is inhibited, a specific region of the chromosome will produce its characteristic "messenger-RNA" which goes on to produce certain special proteins to meet particular functional demands." The new protein is thought to be different from that produced by the nerve cell prior to the release of the DNA by the particular modulated frequency of impulses.

The essence of the theory is that the response of the nerve cell to a new pattern of incoming impulses differs from its response to a familiar pattern. The protein molecules in the cell presumably do not have the proper configuration to dissociate or to be activated in response to a new pattern. A particular new pattern of frequency modulation must first induce DNA production of RNA which will make a specific protein molecule capable of dissociating when that pattern reappears. Only after this has occurred can a new frequency evoke a reaction which will trigger off transmission accross the synapse.

Memory is related to these events in the following way. Once the DNA production of RNA has been specified by a particular pattern of frequency modulation, the production of reactive protein continues within the neuron throughout most of the organism's life. Hydén believes that the many permutations permitted by the rearrangement of the four nucleotide bases in the complex structure of RNA provide "a molecular substance that, in principle, could encode the 10^{15} to 10^{20} bits of information thought to be needed for a lifetime" (1962, p. 64). Since, to the best of our knowledge the different nerve cells in a given organism have the same karyotypes, it is assumed that they can produce similar RNA capable of reacting in similar fashion to a given pattern of frequency modulation. All the nerve cells involved in a given complex process are thus assumed to be integrated by a common mechanism. "Such a substrate for learning and multiple storage of memory would not require a strict localization within the CNS and could explain the well-known findings by Lashley that memory is not destroyed by the removal of large sections of the brain cortex" (1962, p. 66).

The speculations in the foregoing account have one very important property which distinguishes them from most of the more familiar theories of learning and memory. Memory here is linked to a "primary mechanism of general biological validity." It is assumed that there is a similar biochemical mechanism underlying individual memory and the inherited properties of the species—a very reasonable assumption in view of the intimate relationship between DNA and RNA. Furthermore, this model permits the individuality encoded in the chromosomal DNA of each genotype at meiosis and fertilization to be propagated into the learning and memory mechanism by means of the known sequence of DNA producing RNA producing protein. Such a model is most aptly consistent with the distributions of individual differences invariably found in studies of learning and memory. It is the first to be suggested that appears capable of reconciling the universal feature of improvement following practice with the idiosyncratic features of individual performance.

HERITABILITY AND BEHAVIOR

Studies of domestication in the rat have demonstrated some different kinds of organization that can co-exist as alternative forms within a species. In laboratory experiments, for example, some rats learn their way through mazes readily on the basis of visual cues while others do better on the basis of predominantly kinaesthetic cues. One group of studies (Richter, 1954) disclosed some of the relations between behavior and the balanced combinations of components that are alternative possibilities in a species. In domesticated strains of rats, activity, as measured in a revolving drum, was found to be controlled by the gonads. Control rats registered a daily activity score as high as 18,000 revolutions, while gonadectomized rats scored only a few hundred revolutions. Cortisone therapy sufficed to maintain a high activity level in the gonadectomized rats. When the same experiment was repeated on wild rats, the presence or absence of gonads made no detectable difference in measured activity. Further study of the differences between laboratory and wild rats revealed that wild rats have much larger adrenals, and laboratory rats have much larger gonads. So it appears that activity may be under the control of adrenal output in the one case, and gonadal output in the other.

Certain conclusions follow from such observations as well as from consideration of the relations between heritability and behavior. The behavior of an organism does not provide an unequivocal index to the genotype of that organism nor, for any level of organization, does it provide a sure index to the component systems, their functional interrelations, or their role in the execution of the observed behavior. The implications of this for behavioral science, though clear, have only recently started to be appreciated, and many conceptions about "laws" of behavior are currently being revised. It is now realized that the formerly widespread assumption that there are principles of behavior (Hull, 1943), which are independent of the behaving organism, is, in most if not all cases, untenable.

Our understanding of this problem has been formulated in the following way:

> The applicability of any "laws" derived from the study of (1) the behavior of one member of a population to the behavior of other members of the population, or (2) the average behavior of many individuals to the behavior of a single individual, or (3) the behavior of the members of one population to the behavior of the members of another population, will require experimental analysis for each behavior we wish to study for all populations in which we wish to study it (Hirsch, 1962*b*, p. 14).

This formulation follows directly from the concept of "scientific law," which has been defined as "a general proposition about all things of a certain sort" (Braithwaite, 1960, p. 2). To the extent that individuals differ with respect to those features of their organization which are involved in a given behavior, their performance on that behavior *may* follow different scientific laws.

SUMMARY

In a survey of the major concepts and methods of behavior-genetic analysis, the role of description, classification, and analysis has been discussed for behavior study and that of transmission, population, and physiological genetics for the study of heredity.

Since the individuals, whose behavior we study, live in populations, detailed consideration has been given to the properties both of individuals and of populations. Individuals can be analyzed into component systems whose heterogeneity is the developmental outcome of mitosis and differentiation. Populations are composed of individuals whose initial uniqueness is generated by the probabilistic nature of meiosis and fertilization. Mutation, recombination, and differential selection foster the evolutionary divergence of all populations.

Trait variations, or individual differences in the expression of one trait, is distinguished from trait variability, or differences in the rank ordering of individuals on a series of traits. The concept of a normal individual is seen to have no generality. Heritability is shown to be a property of populations and never of behaviors, and to be a concept distinguishable from instinct.

The interaction between behavior and the gene pool is seen in the influence of bisexuality on sociality and that of social structure on the mating system, which determines the genetic structure of a population.

Behavior-genetic analysis employs the methods of both psychology and genetics. Pertinent genetic methods are reviewed and illustrated with some behavior-genetic results. The implications of this approach are examined for our understanding of important problems in ontogeny and phylogeny, such as the elucidation of developmental pathways, evolutionary sequences, and taxonomic relations.

Reports of recent biochemical measurements, indicating the possible similarity of the mechanisms underlying both species memory—heredity—and individual memory, have been examined with respect to their implications for our understanding of both the general and the idiosyncratic features of learning. It is now evident that, in order to be meaningful, behavioral laws require greater specificity of biological reference than has hitherto been realized.

REFERENCES

Allen, G., and Baroff, G. S. Mongoloid twins and their siblings. *Acta. Genet.*, 1955, 5, 294-326.

ALLFREY, V. G., AND MIRSKY, A. E. Evidence for the complete DNA-dependence of RNA synthesis in isolated thymus nuclei. *Proc. Natl. Acad. Sci.*, 1962, **48**, 1590-1596.

ANASTASI, A. *Differential Psychology.* (3rd ed.) New York: The Macmillan Co., 1958.

ARONSON, S. M., AND VOLK, B. W. Genetic and demographic considerations concerning Tay-Sach's disease. In S. M. Aronson and B. W. Volk (eds.), *Cerebral Sphingolipidoses.* New York: Academic Press, Inc., 1962.

BASTOCK, M. A gene mutation which changes a behaviour pattern. *Evolut.*, 1956, **10**, 421-439.

———, AND MANNING, A. The courtship of *Drosophila melanogaster. Behav.*, 1955, **8**, 85-111.

BEADLE, G. W. Genes and the chemistry of the organism. *Amer. Sci.*, 1946, **34**, 31-53.

———. Genetics. In M. Gilstrap (ed.), *Promise of the Life Sciences.* Washington, D.C.: The Graduate School, U.S. Dept. of Agriculture, 1961.

BENDA, C. E. *Mongolism and Cretinism.* New York: Grune & Stratton, Inc., 1946.

BEARN, A. G., AND GERMAN, J. L. Chromosomes and disease. *Sci. Amer.*, 1961, **205**, 66-76.

BONNER, D. M. *Heredity.* Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1961.

BOREK, E., BRECHER, A., JERVIS, G. A., AND WAELSCH, H. Oligophrenia phenylpyruvia. II. Constancy of the metabolic error. *Proc. Soc. Exper. Biol. & Med.*, 1950, **75**, 86-89.

BOYD, W. C. *Genetics and the Races of Man.* Boston: Little, Brown & Company, 1950.

BRAITHWAITE, R. B. *Scientific Explanation.* New York: Harper & Brothers, 1960.

BREESE, E. L., AND MATHER, K. The organization of polygenic activity within a chromosome in *Drosophila. Hered.*, 1957, **11**, 373-395.

BROADHURST, P. L., AND JINKS, J. L. Biometrical genetics and behavior: reanalysis of published data. *Psychol. Bull.*, 1961, **58**, 337-362.

CASPARI, E. Genetic basis of behavior. In A. Roe and G. G. Simpson (eds.), *Behavior and Evolution.* New Haven: Yale University Press, 1958.

CATTELL, R. B. The multiple abstract variance analysis equations and solutions: For nature-nurture research on continuous variables. *Psychol. Rev.*, 1960, **67**, 353-372.

CHARGAFF, E. Chemical specificity of nucleic acids and mechanisms of their enzymatic degradation. *Experientia*, 1950, **6**, 201-209.

DEBEER, G. *Embryos and Ancestors.* (3d ed.) Oxford: Clarendon Press, 1958.

DEEVEY, E. S. The hare and the haruspex. *Amer. Sci.*, 1960, **48**, 415-430.

DOBZHANSKY, T. *Genetics and the Origin of Species.* (3d ed.) New York: Columbia University Press, 1951.

———. Genetics and equality. *Sci.*, 1962, **137**, 112-114.

———, AND SPASSKY, B. Selection for geotaxis in monomorphic and polymorphic populations of *Drosophila pseudoobscura. Proc. Natl. Acad. Sci.*, 1962, **48**, 1704-1712.

DUCOFF, H. S., AND EHRET, C. F. (eds.) *Mitogenesis.* Chicago: University of Chicago Press, 1959.

ERLENMEYER-KIMLING, L., HIRSCH, J., AND WEISS, J. M. Studies in experimental behavior genetics: III. Selection and hybridization analyses of individual differences in the sign of geotaxis. *J. comp. physiol. Psychol.*, 1962, **55**, 722-731.

FORD, E. B. Evolution in progress. In S. Tax (ed.), *Evolution after Darwin.* Vol. I. *The Evolution of Life.* Chicago: University of Chicago Press, 1960.

FREIRE-MAIA, N., AND FREIRE-MAIA, A. The structure of consanguineous marriages and its genetic implications. *Ann. Hum. Genet.*, (London), 1961, **25**, 29-39.

FULLER, J. L., AND THOMPSON, W. R. *Behavior Genetics.* New York: John Wiley & Sons, Inc., 1960.

GINSBURG, B. E. Genetics as a tool in the study of behavior. *Perspect. Biol. Med.*, 1958, **1**, 397-424.

GRAHAM, C. H., AND RATOOSH, P. Notes on some interrelations of sensory psychology, perception, and behavior. In S. Koch (ed.). *Psychology: a Study of a Science.* Vol. 4. New York: McGraw-Hill Book Company, Inc., 1962.

HARRIS, H. Metabolic defects and mental retardation. In *Mental Retardation. Proc. Assoc. nerv. ment. Dis.*, 1962, **39**, 87-96.

HAYMAN, B. I. The theory and analysis of diallel crosses. *Genet.*, 1954, **39**, 789-809.

HINDE, R. A. Some recent trends in ethology. In S. Koch (ed.), *Psychology: a Study of a Science.* Vol. 2. New York: McGraw-Hill Book Company, Inc., 1959.

HIRSCH, J. Studies in experimental behavior genetics: II. Individual differences in geotaxis as a function of chromosome variations in synthesized *Drosophila* populations. *J. comp. physiol. Psychol.*, 1959, **52**, 304-308.

———. The contribution of behavior genetics to the study of behavior. In F. J. Kallmann (ed.), *Expanding Goals of Genetics in Psychiatry.* New York: Grune & Stratton, Inc., 1962*a*.

———. Individual differences in behavior and their genetic basis. In E. Bliss (ed.), *Roots of Behavior.* New York: Paul B. Hoeber, Inc., 1962*b*.

———, and Boudreau, J. C. Studies in experimental behavior genetics: I. The heritability of phototaxis in a population of *Drosophila melanogaster*. *J. comp. physiol. Psychol.*, 1958, **51**, 647-651.

———, and Erlenmeyer-Kimling, L. Studies in experimental behavior genetics. IV. Chromosome analyses for geotaxis. *J. comp. physiol. Psychol.*, 1962, **55**, 732-739.

Hull, C. L. *Principles of Behavior*. New York: Appleton-Century-Crofts, Inc., 1943.

Hydén, H. The neuron. In J. Brachet and A. E. Mirsky (eds.), *The Cell: Biochemistry, Physiology, Morphology*. Vol. 4. New York: Academic Press, Inc., 1960.

———. A molecular basis of neuron-glia interaction. In F. O. Schmitt (ed.), *Macromolecular Specificity and Biological Memory*. Cambridge: The MIT Press, 1962.

———, and Egyházi, E. Nuclear RNA changes of nerve cells during a learning experiment in rats. *Proc. Natl. Acad. Sci.*, 1962, **48**, 1366-1373.

———, and Pigon, A. A cytophysiological study of the functional relationship between oligodendroglial cells and nerve cells of Deiters' nucleus. *J. Neurochem.*, 1960, **6**, 57-72.

Jarvik, L. F., Falek, A., and Pierson, W. P. Down's syndrome (mongolism): The heritable aspects. *Psychol. Bull.*, 1964, 61, 388-398.

———, Kallmann, F. J., Lorge, I., and Falek, A. Longitudinal study of intellectual changes in senescent twins. In C. Tibbitts and W. Donahue (eds.), *Social and Psychological Aspects of Aging*. New York: Columbia University Press, 1962.

Kallmann, F. J. *Heredity in Health and Mental Disorder*. New York: W. W. Norton & Company, Inc., 1953.

———. Twin data in the analysis of mechanisms of inheritance. *Amer. J. Hum. Genet.*, 1954, **6**, 157-162.

———. Psychogenetic studies of twins. In S. Koch (ed.), *Psychology: a Study of a Science*. Vol. 3. New York: McGraw-Hill Book Company, Inc., 1959.

Kalow, W. *Pharmacogenetics, Heredity and the Response to Drugs*. Philadelphia: W. B. Saunders Company, 1962.

Kettlewell, H. B. D. Investigations on the evolution of melanism in *Lepidoptera*. *Proc. Roy. Soc. Lond. B.*, 1956, **145**, 297-303.

King, J. A. Closed social groups among domestic dogs. *Proc. Amer. Phil. Soc.*, 1954, **98**, 327-336.

———, and Nichols, J. W. Problems of classification. In R. H. Waters, D. A. Rethlingshafer, and W. E. Caldwell (eds.), *Principles of Comparative Psychology*. New York: McGraw-Hill Book Company, Inc., 1960, 18-42.

Klineberg, O. *Social Psychology*. New York: Henry Holt & Company, Inc., 1954.

Krech, D., Rosenzweig, M. R., Bennett, E. L., and Krueckel, B. A. Enzyme concentrations in the brain and adjustive behavior patterns. *Sci.*, 1954, **120**, 994-996.

Lehman, I. R., Zimmerman, S. B., Adler, J., Bessman, M. J., Simms, E. S., and Kornberg, A. Enzymatic synthesis of deoxyribonucleic acid. V. Chemical composition of enzymatically synthesized deoxyribonucleic acid. *Proc. Natl. Acad. Sci.*, 1958, **44**, 1191-1196.

Lehrman, D. S. On the origin of the reproductive behavior cycle in doves. *Trans. N.Y. Acad. Sci.*, 1959, **21**, 682-688.

Lehrman, D. S. Gonadal hormones and parental behavior in birds and infrahuman mammals. In W. C. Young (ed.), *Sex and Internal Secretions*. Vol. II (3d ed.). Baltimore: The Williams & Wilkins Company, 1961, 1268-1382.

———. Interaction of hormonal and experiential influences on development of behavior. In E. Bliss (ed.), *Roots of Behavior*. New York: Paul B. Hoeber, Inc., 1962.

Lejeune, T., Gautier, M., and Turpin, R. Les chromosomes humains en culture de tissue. *C.R. Acad. Sci.* (Paris), 1959, **248**, 602-603.

Lerner, I. M. *Genetic Homeostasis*. New York: John Wiley & Sons, Inc., 1954.

———. *The Genetic Basis of Selection*. New York: John Wiley & Sons, Inc., 1958.

Lindzey, G., Lykken, D. T., and Winston, H. D. Infantile trauma, genetic factors, and adult temperament. *J. Abnorm. & Soc. Psychol.*, 1960, **61**, 7-14.

Manning, A. The effects of artificial selection for mating speed in *Drosophila melanogaster*. *Anim. Behav.*, 1961, **9**, 82-92.

Mayr, E. *Systematics and the Origin of Species from the Viewpoint of a Zoologist*. New York: Columbia University Press, 1942.

———. The emergence of evolutionary novelties. In S. Tax (ed.), *Evolution after Darwin*. Vol. I. *The Evolution of Life*. Chicago: University of Chicago Press, 1960.

McGaugh, J. L. Some neurochemical factors in learning. Unpublished doctoral dissertation. Berkeley: University of California, 1959.

Medawar, P. B. *The Uniqueness of the Individual*. London: Methuen, 1957.

Médioni, J. Contribution à l'étude psycho-physiologique et génétique du phototropisme d'un insecte: *Drosophila melanogaster* Meigen. Thèses Présentées à la Faculté des Sciences de l'Université de Strasbourg, 1961.

MESELSON, M., AND STAHL, F. W. The replication of DNA in *Escherichia coli*. *Proc. Natl. Acad. Sci.*, 1958, **44**, 671-682.

MORTON, N. E. Segregation and linkage. In W. J. Burdette (ed.), *Methodology in Human Genetics*. San Francisco: Holden-Day, 1962.

NIRENBERG, M. W., AND MATTHAEI, J. H. The dependence of cell-free protein synthesis in *E. coli* upon naturally occurring or synthetic polyribonucleotides. *Proc. Natl. Acad. Sci.*, 1961, **47**, 1588-1602.

OSBORNE, R. H., AND DEGEORGE, F. V. *Genetic Basis of Morphological Variation*. Cambridge: Harvard University Press, 1959.

PITTENDRIGH, C. S. Adaptation, natural selection, and behavior. In A. Roe and G. G. Simpson (eds.), *Behavior and Evolution*. New Haven: Yale University Press, 1958.

REED, S. C., AND REED, E. W. Natural selection in laboratory populations of *Drosophila*. II. Competition between a white-eye gene and its wild type allele. *Evolut.*, 1950, **4**, 34-42.

RENDEL, J. M. Mating of ebony, vestigial, and wild type *Drosophila melanogaster* in light and dark. *Evolut.*, 1951, **5**, 226-230.

RICHTER, C. P. The effects of domestication and selection on the behavior of the Norway rat. *J. Natl. Canc. Inst.*, 1954, **15**, 727-738.

RODERICK, T. H. The genetics of variation in cholinesterase activity in the cerebral cortex of the rat. Unpublished doctoral dissertation. Berkeley: University of California, 1958.

RODGERS, D. A., AND MCCLEARN, G. E. Genetic factors in alcohol preference in laboratory mice. *Amer. Psychol.*, 1959, **14**, 396.

ROTHENBUHLER, W. C. Genetics of a behavior difference in honey bees. *Proc. 10th Int. Cong. Genet.* (Montreal), 1958, **2**, 242.

SAWIN, P. B., AND CRARY, D. D. Genetic and physiological background of reproduction in the rabbit. II. Some racial differences in the pattern of maternal behavior. *Behav.*, 1953, **6**, 128-145.

SCOTT, J. P. Effects of single genes on the behavior of *Drosophila*. *Amer. Nat.*, 1943, **77**, 184-190.

———. *Animal Behavior*. Chicago: University of Chicago Press, 1958.

———, AND FREDERICSON, E. The causes of fighting in mice and rats. *Physiol. Zool.*, 1951, **24**, 273-309.

SHAW, M. W. Segregation ratios and linkage studies in a family with six translocation mongols. *Lancet*, 1962, **1**, 1407.

SHIELDS, J. *Monozygotic Twins Brought Up Apart and Brought Up Together*. London: Oxford University Press, 1962.

SIMPSON, G. G. *Principles of Animal Taxonomy*. New York: Columbia University Press, 1961.

SINNOTT, E. W., DUNN, L. C., AND DOBZHANSKY, T. *Principles of Genetics*. (5th ed.) New York: McGraw-Hill Book Company, Inc., 1958.

SKODAK, M., AND SKEELS, H. M. A final follow-up of 100 adopted children. *J. genet. Psychol.*, 1949, **75**, 3-19.

SMITH, A. A further note on mongolism in twins. *Brit. J. Prev. Soc. Med.*, 1960, **14**, 47.

SMITH, E. L. Nucleotide base coding and amino acid replacements in proteins, II. *Proc. Natl. Acad. Sci.*, 1962, **48**, 859-864.

SPALDING, D. Instinct, with original observations on young animals. 1872. Reprinted in *Brit. J. Anim. Behav.*, 1954, **2**, 2-11.

SPEYER, J. F., LENGYEL, P., BASILIO, C., AND OCHOA, S. Synthetic polynucleotides and the amino acid code, II. *Proc. Natl. Acad. Sci.*, 1962, **48**, 63-68.

SPUHLER, J. N. Some problems in the study of quantitative inheritance in man. *Amer. J. Hum. Genet.*, 1954, **6**, 130-139.

STEBBINS, G. L., JR. *Variation and Evolution in Plants*. New York: Columbia University Press, 1950.

STEINBERG, A. G. Population genetics: Special cases. In W. J. Burdette (ed.), *Methodology in Human Genetics*. San Francisco: Holden-Day, 1962.

STERN, C. *Principles of Human Genetics*. San Francisco: W. H. Freeman and Co., 1960.

SWANSON, C. P. *The Cell*. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1960.

THOMPSON, W. R. Social behavior. In A. Roe and G. G. Simpson (eds.: *Behavior and Evolution*. New Haven: Yale University Press, 1958.

———, AND FULLER, J. L. The inheritance of activity in the mouse. *Amer. Psychol.*, 1957, **12**, 433.

TINBERGEN, N. Behavior, systematics, and natural selection. In S. Tax (ed.), *Evolution after Darwin*. Vol. I. *The Evolution of Life*. Chicago: University of Chicago Press, 1960.

TJIO, J. H., AND LEVAN, A. The chromosome number of man. *Hereditas*, 1956, 42, 1-6.

TRYON, R. C., Individual differences. In F. A. Moss (ed.), *Comparative Psychology*. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1942.

VON FRISCH, K. Orientierungsvermögen und Sprache der Bienen. *Naturwissenschaften*, 1951, **38**, 105-112.

WADDINGTON, C. H., WOOLF, B., AND PERRY, M. M. Environment selection by *Drosophila* mutants. *Evolut.*, 1954, **8**, 89-96.

WALLACE, B., AND SRB, A. M. *Adaptation*. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1961.

WASHBURN, S. L., AND AVIS, V. Evolution of human behavior. In A. Roe AND G. G. Simpson (eds.), *Behavior and Evolution.* New Haven: Yale University Press, 1958.

WATSON, J. D., AND CRICK, F. H. C. The structure of DNA. *Cold Spring Harbor Symposia Quant. Biol.* 1953, **18**, 123-131.

WILLIAMS, R. J. *Biochemical Individuality: The Basis for the Genetotrophic Concept.* New York: John Wiley & Sons, Inc., 1956.

CHAPTER 4

Learning in Animals

M. E. BITTERMAN
Bryn Mawr College

In the seventy years or so since the problem of animal intelligence first was brought into the laboratory, the relation between theory and experiment has been a close one, with little effort devoted to parametric study and a great deal to the search for evidence favoring one or another of several divergent conceptions of the learning process. If the work of these years is to be understood, the theoretical questions which gave it direction and purpose must be understood. We shall review here the various theories proposed and assess the progress which has been made toward a resolution of differences.

Before the theories can be considered, however, something must be said about the class of phenomena with which they deal. We do not observe learning; we observe only changes in behavior. *Learning* is our name for a process (or set of processes) assumed to be responsible for certain of the observed changes. The nature of the process must be inferred from detailed studies of those changes and the variables to which they are related. What then are the changes that we attribute to learning, and what are the conditions under which they occur?

In one kind of experiment—which it is convenient to designate as *Thorndikian*—the consequences of response are manipulated (as when pressing a lever in a problem box or turning to the left in a *T*-maze is rewarded with food or punished with shock). In a second kind of experiment—the *Pavlovian* experiment—stimuli are paired (as when the onset of a tone is followed by food or shock), and the pairing is independent of response (tone is followed by food or shock whatever the animal's response to the tone). A third kind of experiment—the *Avoidance* experiment—also begins with pairing of stimuli, but the second stimulus follows the first only if the first fails to elicit some specified response (as when, in a shuttle box, shock follows tone only if the animal fails to change compartments in the interval between the onset of the tone and the scheduled onset of shock). There are many variations of these three basic procedures (Bitterman, 1962), all of which produce changes in behavior that are generally attributed to learning. For example, the animal may make with increasing frequency some response which terminates shock (in a Thorndikian situation) or forestalls it (in an Avoidance situation), or the animal may become increasingly agitated by the occurrence of a signal for unavoidable shock (in a Pavlovian situation).

What does it mean to "attribute" these changes in behavior to "learning?" It means, first of all, that we place them in a common category, distinguishing them from other changes in behavior (such as those produced by starvation or by surgery). We place them in a common category on the assumption that they stem from processes which are closely related to each other and distinctly different from the processes responsible for the changes which we categorize differently. Whether or not this assumption is correct will become clear only as we begin to isolate the processes responsible both for the included and for the excluded changes. Our name for the category—*learning*—reflects another assumption, which is that the processes responsible for the changes thus categorized are closely related to those responsible for what we know intuitively as learning in man. Whether or not the second assumption is correct will become clear only as we begin to understand the processes which operate at each level. It is possible, of course, to study the animal phenomena quite independently of their relation to the human phenomena. The two lines of investigation have, on the whole, been carried on quite independently of each other, despite the fact that it was an interest in the origins of human intelligence which led in the beginning to the experimental study of learning in animals.

EARLY DEVELOPMENTS

The doctrine of evolution brought with it in the latter part of the nineteenth century an intensification of interest in the psychology of animals. The subject had long captured the popular imagination and had been invested with considerable theological significance, but Darwin's theory gave it great scientific importance. The wealth of anatomical evidence for the theory was not in itself convincing, Darwin admitted; evidence of psychological continuity—especially of intellectual continuity—also was required, which he sought in the anecdotes of hunters, naturalists, pet owners, and zoo keepers. Were animals capable of reasoning? A story about a captive baboon suggested that they were: scratched one day by a kitten with which she was playing, the baboon examined the kitten's feet and promptly bit off the claws. A further indication of reasoning in animals was provided by the experience of a hunter named Colquhoun, who once "winged two wild-ducks, which fell on the opposite side of a stream; his retriever tried to bring over both at once but could not succeed; she then, though never before known to ruffle a feather, deliberately killed one, brought the other over, and returned for the dead bird" (Darwin, 1871, p. 46). It was evidence such as this on which Darwin rested his conclusion that there was nothing new in the mind of man—that there was no human characteristic or capability which was not manifested also, at least in incipient form, by some lower animal.

Morgan. The transition from anecdotalism to the experimental study of animal intelligence was markedly influenced by C. Lloyd Morgan, an English psychologist. In his *Introduction to Comparative Psychology* (1894), Morgan emphasized the value of systematic and sustained investigation, urging those who witnessed what appeared to be an intelligent action by some animal not to be content merely with recording it, but to study it in an effort to discover its true nature. Morgan had a terrier named Tony that could open a garden gate. The dog would put his head under the latch, lift it, wait for the gate to swing open, and then run out into the road. A casual observer, seeing only the finished performance, might conclude that the dog understood the mechanical principle involved—a level of understanding which the anecdotalists were quite willing to attribute to animals (Romanes, 1881)—but Morgan, who had studied the development of the pattern, reached a quite different conclusion.

One day, Tony was seen running excitedly along the fence inside the garden, stopping at various points to put his head between the vertical railings and look out into the road. After a while, he happened to put his head out beneath the latch, which lifted as he withdrew; he began to look elsewhere, but soon noticed that the gate was swinging open, and ran out. Thereafter, Tony always was required to open the gate for himself. His performance improved slowly, with useless movements (such as poking his head out at the wrong places) gradually being eliminated, and nearly three weeks went by before he could go directly to the latch and lift it without any ineffectual fumbling. Morgan made a similiar observation during his experiments with chicks. The animals were kept in a corner of his study which was walled off with old newspapers, and one of the chicks learned to escape from the enclosure in the course of pecking at the number on one of the pages. In both cases, Morgan believed, an action performed at first without view to any particular result was found to have certain pleasurable consequences, and it was repeated because of those consequences. There was no need to assume that the animals could reason.

Morgan was an evolutionist and certainly not predisposed, he insisted, to exaggerate the intellectual differences between man and animal, but he was opposed to the undisciplined anthropomorphism of the anecdotalists, advocating instead a disciplined anthropomorphism. Like the anecdotalists, Morgan set out to deal with the behavior of animals in terms of functional categories derived from human experience; the community of structure made it reasonable, he thought, to expect a community of psychological functioning. By the same token, however, the marked differences in structure between man and even the highest (infrahuman) animals made it reasonable to expect some important functional differences. How then was the behavior of any given animal to be interpreted? Morgan counselled parsimony. Since the animal might be in a stage at which certain advanced capabilities had not yet been evolved from their more primitive precursors, it seemed safest not to assume advanced capabilities unless the assumption was required by the data. "In no case," he ruled, "may we interpret an action as the outcome of the exercise of a higher psychical faculty, if it can be interpreted as the outcome of the exercise of one which stands lower in the psychological scale" (Morgan, 1894, p. 53). The scale was to be derived from the analysis of human experience.

Although Morgan found no evidence that animals were capable of logical inference or conceptual thought, he did conclude that their behavior was intelligent in the sense that it was regulated to meet the demands of particular environments. The repertoire of instinctive tendencies with which each animal came into the world was modified in a trial-and-error fashion, the successful ones being strengthened and the unsuccessful ones inhibited. For example, the newly hatched chick, which at first pecked indiscriminately at a variety of objects, both edible and inedible, soon came to avoid some of them and to take others with increasing avidity. The instinctive behavior was mediated quite automatically (unconsciously) by centers in the spinal cord and brain stem, but cerebral centers were indirectly aroused and the animal thereby became aware, Morgan speculated, of the antecedent stimuli, the responses, and the consequences (pleasant or unpleasant). Only to the extent that consciousness accompanied instinctive activities did intelligent control of those activities become possible, he thought. Actually, however, *consciousness* and *activity of the cerebral hemispheres* were essentially synonymous for Morgan, whose view of learning could be stated in perfectly objective terms.

Reproduced as Fig. 4-1 is a diagram drawn by Morgan to illustrate his conception of the events involved in the development of cerebral control over the pecking response of the chick. The process began, he postulated, with the reflexive elicitation of pecking by a visual stimulus. The response was mediated by lower centers, but cerebral centers for vision, kinesthesis, and taste were aroused indirectly, which meant that the animal became 'conscious' of the visual object,

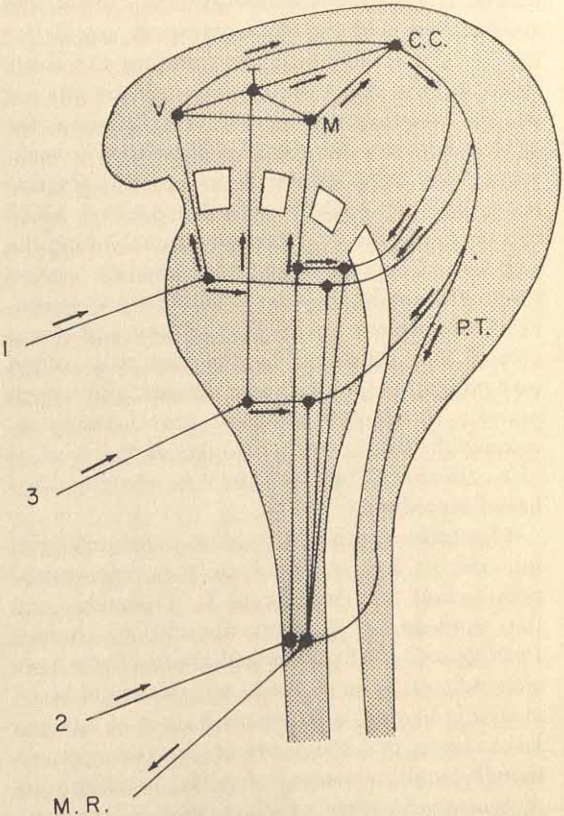


Fig. 4-1. Morgan's diagram of the neural events involved in the development of cerebral control over the pecking response in the chick. 1, visual stimulus; 2, kinesthetic stimulus produced by pecking; 3, gustatory stimulus; *M.R.*, pecking response; *V*, *T*, and *M*; cerebral centers for vision, taste, and kinesthesis; *C.C.*, cerebral control center for the pecking response; *P.T.*, pyramidal tract. Redrawn from Morgan (1894, p. 183) with permission of the publisher.

the movement, and the gustatory consequences. The kinesthetic center then aroused the cerebral control center for pecking, which augmented or inhibited pecking (by way of the pyramidal pathways) depending upon the taste. The contiguous arousal of the four cerebral centers (visual, kinesthetic, gustatory, and control) tended gradually to establish new connections among them and thereby to make it possible for the visual stimulus alone to reinstate the entire pattern. Thus, the taste center could be aroused via the visual center without stimulation of the taste receptors—the visual "impression" would suggest the "idea" of a pleasant or an unpleasant taste—and the control center for pecking would function accordingly, facilitating or inhibiting the response. That the higher (ideational) centers eventually might be short-circuited by new connections between lower-lying sensory and motor centers was suggested by the fact that certain learned patterns of behavior tended after much practice to become automatic, but learning inevitably began, Morgan thought, at the level of ideas. The animal learned about its world and behaved accordingly.

Thorndike. If the work of any one man can be said to have ushered in the experimental period, that man is Edward L. Thorndike, and the work is his doctoral dissertation (*Animal Intelligence: An Experimental Study of the Associative Processes in Animals*) which was published in 1898. In that work, the influence of Morgan looms large, and the views of the two men, although similiar in many respects, provide a set of contrasts in terms of which much of the work that followed may be understood.

Like Morgan, Thorndike was interested in the evolution of human intelligence. Like Morgan, he was critical of the anecdotalists and saw the need for careful, systematic observation. Thorndike's experimental situations resembled those with which Morgan had worked more informally. His method was to put a hungry animal into a small enclosure from which it could escape by the performance of some simple action or sequence of actions, such as pulling a loop of wire, pressing a lever, or stepping on a platform (Fig. 4-2). Escape was rewarded with food, which usually could be seen from inside the enclosure, and records were kept of the animal's responses and of the time required for escape on each of a series of trials. A cat or a dog in such a situation faced much the same problem, then, as did Tony at Morgan's garden gate, and the behavior observed by Thorndike was much like that reported by Morgan. There was nothing to suggest understanding, but only a mad scramble to get out, in the course of which the animal seemed to hit quite by accident upon the correct response. The pleasant consequences of the successful response somehow strengthened the animal's tendency to make it, while unsuccessful responses gradually dropped out, until (after a series of trials) the correct response appeared almost as soon as the animal was placed in the situation. It was in his interpretation of these observations that Thorndike differed sharply from Morgan. For Morgan's *cognitive* theory, he substituted a *response* theory.

Morgan thought in terms of the association of ideas, but Thorndike found it unnecessary to assume that ideas played any part in learning. The observed changes in behavior could be accounted for adequately, he thought, in terms of direct connections between sense impressions and motor impulses, without reference to intervening representations of the acts to be performed or the ends to be attained. From this point of view, the animal did not learn *about* the consequences of response and alter its behavior accordingly; the consequences of response were *conditions of learning*, and learning was conceived as a change in the strength of connection between contiguous sensory and motor processes rather than between contiguous sensory processes. In his famous Law of Effect, Thorndike held simply

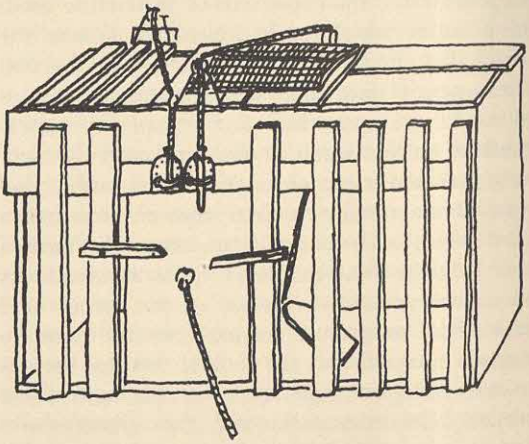


Fig. 4-2. One of Thorndike's problem boxes. Redrawn from Thorndike (1911, p. 30) with permission of the publisher.

that sensory-motor connections were strengthened by pleasant consequences and weakened by unpleasant ones (1911, p. 244).

The two theories are contrasted in Table 4-1, where three columns reflect three dimensions of difference between them. In column one, the nature of the postulated connection is shown, with S standing for a sensory process and *R* for a motor process. The S-S in Morgan's row represents his assumption that sensory centers were connected, and the *S-R* in Thorndike's row his assumption that learning was a change in the strength of connection between sensory and motor mechanisms. Column two shows that Thorndike considered effect to be a factor in learning, while Morgan did not. Both men assumed temporal contiguity to be an essential condition for learning (contiguously activated sensory centers were connected in Morgan's theory, contiguously activated sensory and motor centers in Thorndike's), and both agreed that contiguity was not all—that other factors, such as attention, had to be considered; but they disagreed on Effect. Column three shows that Morgan required a supplementary activation principle, while Thorndike did not. The function of the theories was to account for changes in behavior, but the S-S principle in itself made no reference to motor systems, and it had to be supplemented by an activation principle if the animal was not to be left—in Guthrie's words (1935, p. 172)—"buried in thought." Morgan postulated a control center for each response, which was brought into play by centers normally aroused by proprioceptive feedback from the response, and which excited or inhibited the response depending on the hedonic tone of its previously experienced (and now anticipated) consequences. For Thorndike, by contrast, no special activation principle seemed to be necessary; the observed change in behavior followed directly from the postulated change in the relation between sensory and motor systems.

TABLE 4-1

Two learning theories contrasted

	Postulated connection	Effect a factor in learning?	Supplementary activation principle?
Morgan	*S-S*	No	Yes
Thorndike	*S-R*	Yes	No

On purely formal grounds, there was little reason to prefer either of the theories to the other. In future years, the *S-S* concept often was to be treated as somehow less objective than the *S-R* concept, but that attitude was quite unjustified. While Morgan talked in phenomenological terms, so also did Thorndike, who had a "fairly definite idea" of what it felt like to be a cat or a dog in one of his problem boxes: it was "most like what we feel when consciousness contains little thought about anything, when we feel our own body, and the impulses we give to it" (1898, p. 83). Both of the postulated connections could, however, be expressed in neurophysiological terms, and behavioral equivalents could be found even for the notions of pleasantness and unpleasantness upon which both theories depended: a pleasant or satisfying state of affairs, wrote Thorndike, was simply "one which the animal does nothing to avoid, often doing such things as attain or preserve it," while an unpleasant or annoying state of affairs was "one which the animal commonly avoids or abandons" (1911, p. 245). Neither of the theories was bound even in the beginning, then, to any assumption about the consciousness of animals. If an S-S connection was not less objective than an S-*R* connection, was it perhaps more remote from observation? Certainly not. Neither connection was observed directly; each was inferred in an effort to account for observed changes in behavior. Was one of the two theories to be preferred on grounds of parsimony? Again, no. Each assumed a connecting process in which temporal contiguity played an important role, and each involved another major assumption—one about the role of effect in learning and the other about the way in which the product of learning was translated into action.

In support of his theory, Thorndike offered evidence from experiments on placement, on guidance, and on imitation. If, after doing whatever was necessary to open the escape door of a problem box, leaving the box, and eating the food outside, a cat was pushed gently in the direction of the door and encouraged to go back into the box (whereupon the door would be closed and new food provided which the animal could obtain by escaping once more), the animal soon would go back into the box of its own accord—it could not, in fact, easily be kept out, If, however, the animal was merely lifted into the box through an opening in the top, it did not learn to go back in by itself, either through the door or by climbing to the top. Even a monkey which was carried to some characteristic place in a room (such as a pile of boxes or the top of a sewing machine), and fed there, did not show any marked tendency to return to that place when subsequently given an opportunity to do so, although it could learn easily to go to a given place when it was rewarded for *going* there (Thorndike, 1901). The animals either could not progress from the idea of being in a certain place to the act of going there, or they had no ideas at all. It should be noted that Thorndike did not scoff at the notion of ideas in animals. If brain currents initiated by the stimulation of receptors were assumed to produce sense impressions, would it not be preposterous, he asked, to deny that similar excitations, centrally initiated, could produce ideas? The main question, he asserted, was whether ideas could be shown to play any part in learning—if not, it was of little importance whether they existed as epiphenomena or did not exist at all.

In his experiments on guidance, Thorndike took the paw of a cat or a monkey and with it manipulated the loop or lever of a problem box, but he was unable by so doing to facilitate learning. The efforts of the animal could be directed in such a way that it would itself perform the required action sooner—as when the animal was so held that its accidental pawing would be sure to operate the manipulandum—but an association leading to a given act could not be established, it seemed, unless an impulse to perform the act was somehow elicited. In his experiments on imitation, Thorndike's procedure was to train an animal to escape from a problem box, then to permit a second animal of the same species to observe the escape of the first, and finally to test the second animal in the box, comparing its performance with that of other animals not given the opportunity to imitate. There was no indication that opportunity to observe another animal facilitated learning, either in the cat or in the monkey.

Reactions to Thorndike's Theory. These findings did not, however, long go unchallenged. Dubious of Thorndike's conclusions, L. T. Hobhouse (1901) performed some further experiments with the cat, dog, otter, elephant, monkey, and chimpanzee, himself demonstrating a number of simple actions, such as opening a drawer containing food or upsetting a vase beneath which food was hidden; his subjects seemed to profit from the demonstrations. A rash of subsequent experiments on imitation, although crude in the extreme by modern standards, provided a number of indications that the correct locality of response, if not the correct response itself, could be conveyed by demonstration (Kinnaman, 1902; Haggerty, 1909; Porter, 1910). Hobhouse carried out a number of other simple tests which yielded evidence of what he termed "perceptual learning." A kitten, lifted repeatedly to the top of a desk and fed, soon began to climb up of its own accord. Thorndike's results on the effectiveness of placement were contradicted also by those of other early investigators (Cole, 1907; Hunter, 1912; Washburn and Abbott, 1912). It should be noted that results of this sort made difficulty for Morgan's theory as well as for Thorndike's, but the difficulty for Morgan's theory was not emphasized because interest centered on learning, and it was not Morgan's learning construct but his activation principle that these results called into question: with no response, there could be no kinesthetic associations, and therefore no activation of the control center for the appropriate response. The difficulty was not, of course, insurmountable; one had only to assume a previously acquired tendency to approach stimuli with pleasant associations.

Other observations which made difficulty for both theories, but which often were treated as though they made difficulty only for Thorndike's had to do with response equivalence. One had only to look at the instrumental behavior of animals, Hobhouse argued, to be convinced that they did not learn "merely to execute a certain movement, but to do a certain thing." Of the way in which a dog hauled up a bucket of food dangling from the end of a string, Hobhouse wrote: "We cannot in such a case apply the conception of a perceived object discharging a uniform motor reaction. There is rather a combination of movements which are not always the same except in this, that they are so adjusted as to produce a certain perceptible change in the external world" (1901, pp. 198-199). A number of other early investigators (Cole, 1907; Shepherd, 1910) called attention to this phenomenon, which had been noted by Thorndike himself. A dramatic demonstration of response equivalence was provided some years afterward by Lashley (1924), who trained a monkey to operate a mechanism with one hand and found it to be quite successful with the other hand when he deprived it of the use of the first. If either of the two theories was to deal with such a finding, it would have to be supplemented by some assumption about preexisting response equivalences.

Hobhouse also criticized Thorndike's theory on the basis of results obtained with the method of delayed reaction, which Hunter (1913) later refined. On a high shelf, Hobhouse placed three cards, each with a string tied to it by means of which it could be pulled down. One of the cards was baited with a biscuit in full view of a dog, which, after being delayed to the count of three, was permitted to choose among the strings. Due to the height of the shelf, the biscuit was not visible after having been placed on the card, and the animal therefore could succeed only by observing the placement and remembering what it had seen. Over a series of trials, with the position of the biscuit varying randomly, the animal's performance became nearly perfect. In Hunter's situation, the animal was restrained in a glass enclosure from which it had an unobstructed view of three compartments, one of which was lighted. Then the light was turned off, and, after some delay, the animal was released. If it entered the previously lighted compartment, food was given, but any attempt to enter one of the other compartments was punished with shock. Pretraining was required to establish the relation between light and food. First the animal learned to go directly to the lighted compartment; then the light was turned off before release, with the time between the two events gradually increased until the animal could no longer perform at a better than chance level.

Rats and dogs dealt with Hunter's problem in terms of gross bodily posture. They would orient to the light and maintain the orientation during the delay interval, after which they would simply go to the compartment at which they were pointed. The success of raccoons and of children, by contrast, had no postural concomitant, and Hunter was led to the conclusion that its basis was ideational; he argued that certain effects of the visual stimulus must somehow be retained and later selectively activated to take the place of the objective stimulus. Watson speculated that the performance of the children might have been based on "a group of throat movements" such as "the words 'go to the box in front when the door opens.' " The movements, "long since correlated with definite modes of response," would guide the child at the moment of release (1914, p. 226). In the case of the nonverbal raccoon, which presented a more formidable problem, Watson reserved judgment, but the assumption lay close at hand that the raccoon's success was mediated by other responses, similarly inaccessible to the naked eye. The simple device of substituting hypothetical for observed responses was sufficient to preserve the notion that learning always involved, as Watson put it, "a series of definite arcs beginning in a receptor and ending in an effector" (p. 227).

The Question of Understanding. Although the trial-and-error conception of problem solving in animals to which both Thorndike and Morgan subscribed was greeted with considerable skepticism, many observations supported it. Tested in problem boxes of one sort or another, most animals gave little indication of understanding, at least in the beginning. They would appear in the course of their explorations to stumble quite by accident upon the correct response, and they would be slow to eliminate useless movements. A raccoon studied by Davis (1907), having chanced to operate a lever with its foot while standing on its head, continued to do just that for several trials. A rat studied by Ulrich (1915) persisted in making a complete circuit of the problem box before approaching the latch. There would be considerable difficulty with a familiar fastening come upon from a new direction, and previously rewarded responses continued to be made after changes in the experimental situation had rendered them absurdly inappropriate. As Thorndike himself had reported, of course, highly appropriate patterns of behavior did emerge rather readily on occasion. In an early study of the effect of age on problem solving, Watson (1903) sometimes could detect not a single useless movement in the second-trial performance of older rats. Davis (1907) and Richardson (1909) reported that experience with one or two problems seemed to produce a general facility for dealing with new situations, while Kinnaman found in his monkeys an "increasing ability to pick out the fastenings as the essential points of difficulty in opening the box, and to direct their efforts upon them alone" (1902, p. 123). No serious challenge to the trial-

and-error conception was provided by rapid learning, which could be traced, as Thorndike asserted, to especially favorable circumstances—the congruence of innate or of previously acquired reaction tendencies with the demands of a new problem and the efficiency of the animal as a connecting system. There were, however, other findings which supported quite a different view.

The concept of perceptual learning was not intended by Hobhouse (1901) to imply any understanding on the part of his animals. Dog and cat learned by doing or by seeing things done, but there was no really good evidence of their ability to infer from previous experience the appropriateness of actions not before performed or witnessed. In the behavior of monkey and chimpanzee, however, something akin to originality began to show itself. For his experiments, Hobhouse developed a series of ingenious tests, suggested in part by the anecdotal literature. There were problems requiring the animal to reach a bit of food with the aid of a stick; to reach a more distant lure with a long stick which was itself reached with a short stick; to reach food placed on top of a table by mounting a stool or a box brought near the table (Fig. 4-3). A problem solved by the chimpanzee but not by the monkey involved a long rod with which a bit of food was to be pushed out of a heavy pipe in which it was lodged. A problem which proved too difficult for both animals involved a box containing a piece of banana which could be pulled toward the subject by means of one rope only after another had been unhooked from the cage. Although neither animal manipulated the hook properly, the chimpanzee would manage to shake the banana out of the box and to "swish it" near with lateral movements of the rope. Hobhouse was convinced by these experiments that properties of objects and relations among objects were to some extent understood by primates. Understanding might develop quite gradually, in the course of considerable fumbling —it seemed clear, for example, that monkeys had to *learn* how to reach with sticks—but in the end specific experience was transcended ("analogically extended"). The products of previous practice were selected and recombined in accordance with the demands of a new problem.

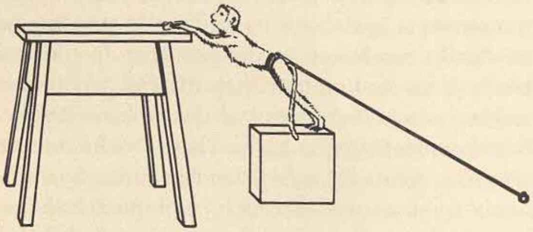


Fig. 4-3. One of Hobhouse's problem situations. Redrawn from Hobhouse (1901, p. 249) with permission of the publisher.

There were many subsequent studies of primate intelligence by methods like those of Hobhouse —informal, qualitative observations for the most part, involving small numbers of animals, widely varying in background, selected primarily on the basis of availability, and tested in intuitive fashion on haphazard series of problems. Watson (1908) early investigated the ability of monkeys to reach food with the aid of a stick or a length of cloth. He found no sign of comprehension, even after a good deal of coaching. Had Hobhouse worked with animals of greater native ability, or had he read into their behavior more than was actually there? There was the possibility, too, that the discrepancy was due to a difference in the past experience of the animals—that the "apparently new reactions" of Hobhouse's animals in reality represented "extremely slight modifications of associations already existing" (p. 174). The response of Shepherd's monkeys to stick problems also was unimpressive, but in certain simpler situations they showed "apparently instant understanding" (1910, p. 58). "A piece of twine was permitted to hang in front of and twelve inches away from the cage, beyond the reach of the longest armed animal which was tested. At the end of the twine a piece of banana was arranged; a thin piece of wood was pushed through the banana and turned so that one end could be grasped by an animal in the cage. By grasping and pulling the stick inwards the food was secured. . . . No efforts were wasted on random movements. It did not appear that any preliminary attempt was made to grasp or even to reach for the food, but there was an immediate movement toward the stick" (p. 54). Shepherd pointed out that evidence of understanding could be expected only in tests appropriate to the "mental range" of the animal; negative results obtained with some arbitrarily selected problem afforded no sound basis for sweeping negative conclusions. The need for further analysis in cases of "apparently instant understanding" was demonstrated by Holmes

(1916), who tested a monkey with a length of board placed outside its cage. When a piece of apple was placed on the far end of the board, which was beyond reach, the animal immediately drew in the board by the nearer end, and so obtained the prize. The same thing happened repeatedly, however, when the apple was placed on the floor, six inches away from the far end of the board.

Perhaps the most influential brief for animal understanding was offered by Wolfgang Köhler (1925) on the basis of his experiments with a group of chimpanzees during the First World War. In the course of his work, Köhler had occasion to record instances of purely accidental problem solution, as when Sultan, having given up attempts to obtain a lure in a situation which required that a long stick be made from two short sticks by fitting one into the hollow end of the other, later did just that during a period of aimless play with the sticks. Only after the longer stick had been constructed, apparently, did the animal appreciate the significance of what it had done. There were, however, other instances in which the solution of a problem seemed to develop, not as a consequence of the correct response, but in advance of it. The animal would survey the situation, seem to gain insight into its requirements, and act accordingly.

Might the animals have learned to deal with situations like those with which Köhler faced them before being brought into the laboratory? The uniqueness of many of the problems, as well as the fact that periods of unsuccessful effort often preceded the solution, weighed against an interpretation in terms of past experience. A further argument for some degree of direct understanding, however limited, was to be found in the nature of the mistakes that were made. Although there were many "stupid" errors (as when an animal, having learned to reach a suspended lure by standing on a box brought beneath it, dragged the box to the bars of the cage when the lure lay on the ground outside), there were also "good" errors—responses closely geared to the central features of the problem, yet inadequate, and for that reason unlikely to have been rewarded in the past. Here is Köhler's description of the use made by Sultan of two sticks, neither one long enough to reach a lure outside his cage: "he pushes one of the sticks out as far as it will go, then takes a second, and with it pokes the first one cautiously towards the objective, pushing it carefully from the nearer end and thus slowly urging it towards the fruit. This does not always succeed, but if he has got pretty close in this way, he takes even greater precaution; he pushes very gently, watches the movements of the stick that is lying on the ground, and actually touches the objective with its tip. Thus, all of a sudden, for the first time, the contact 'animal-objective' has been established" (p. 131).

Köhler insisted that insight was a perfectly objective property of behavior, and Yerkes (1916), whose extensive work on primate intelligence began at about the same time as Köhler's, agreed. Some problems were solved by "random actions and the selection of profitable acts," while others clearly were solved by "observation of essential features or relations in the situation and immediate adaptation" (1927, pp. 186-187). Köhler not only denied that the intelligent solutions he described might previously have been learned as such, but he implied that they were entirely independent of past experience, referring insight to the "optical apprehension" of a situation and failure of insight to "weakness of shape perception" (1925, pp. 277-278). Yerkes, like Hobhouse, was impressed with the growth of understanding in the course of experience. He emphasized the importance of genetic study and himself provided a number of valuable examples, among which was a detailed account of the way in which a young gorilla learned to use a stick as a tool (1927). Whether insightful behavior might ultimately be derived from Thorndikian principles, however much it might differ from the uninsightful behavior which suggested those principles, was for a time a widely debated question. The faith of the majority was affirmative, although not much was done to bridge the gap, or to show how it might be bridged, before interest in the problem waned.

Watson and Guthrie. Yet another theory of learning, which differed both from Morgan's and from Thorndike's, began in the first years of the new century to receive serious consideration. The theory took two forms. In an earlier form—as a *frequency* theory—it found for a time a number of advocates, of whom John B. Watson (1914) was perhaps the most influential and the most persistent. In a later form—as a *postremity* theory—it was closely associated with the name of Edwin R. Guthrie (1930, 1935). Table 4-2 shows the relation of the theory to those of Morgan and Thorndike. Like Thorndike's, it was an *S-R* theory, but it assigned no role in learning to the pleasantness or unpleasantness of the consequences of response; contiguity alone was assumed to be responsible for the establishment and strengthening of connections between sensory and motor systems (just as, in Morgan's view, contiguity alone was responsible for the establishment and strengthening of connections between sensory systems). The Watson-Guthrie theory could be classified, therefore, as an *S-R*-contiguity theory, distinguishing it both from Morgan's *S-S*-contiguity theory and from Thorndike's *S-R*-effect theory.

The theory of Watson and Guthrie was simpler than Thorndike's because it gave no place to effect in learning, and it was simpler than Morgan's because, as Table 4-2 shows, it was not tied to a supplementary activation principle. The formal advantage of the *S-R*-contiguity theory was, in fact, even greater than the table indicates, because both Morgan and Thorndike also found it necessary to assume that connections between sensory and motor systems were established and strengthened by contiguity. For Morgan, *S-R* contiguity was a secondary principle, added to *S-S* contiguity in order to account for the automatic, nonreflective character of long-practiced actions. For Thorndike, the principle seemed (at least occasionally) to have a status more closely coordinate with his Law of Effect; according to Thorndike's Law of Exercise (1911), the mere making of a response in a situation strengthened its connection to that situation, and, the more frequently it was made, the stronger the connection became. The essence of Watson's assertion (which Thorndike denied) was that the Law of Effect was unnecessary—that all of the available data could be understood in terms of the Law of Exercise alone.

TABLE 4-2

Three learning theories contrasted

	Postulated connection	Effect a factor in learning?	Supplementary activation principle?
Morgan	*S-S*	No	Yes
Thorndike	*S-R*	Yes	No
Watson, Guthrie	*S-R*	No	No

A rewarded response gained in strength more rapidly than any other, Watson argued, because it was made more frequently than any other; since each trial ended with the rewarded response, that response was made once on each trial, but no other response *necessarily* appeared once on each trial. "In applying this principle," he wrote, "let us suppose that the animal is capable of only two activities, A and B, when subjected to the stimulus offered by the problem box. B is the successful activity and leads to a *change in stimulation which prevents the occurrence of either* A or B (i.e., the animal is taken out by the experimenter, or he eats food and goes to sleep, . . . , etc.). Then if the order in which A and B occur is due to chance only, B will occur twice as often as A" (1914, pp. 263-264). The same argument had been made earlier by Stevenson Smith (1908) and had been refuted vigorously by Thorndike (1911). As Thorndike pointed out, the analysis was sound only on the assumption that the repetition of an incorrect response was prevented; if the animal began with a strong preference for some incorrect response which could be made repeatedly on each trial, the relative strength of the preference could only increase as training continued. Watson asserted that the animal's activity depended on the immediate conditions of stimulation, and that the performance of one action placed the animal under new conditions of stimulation which led to the performance of an action other than the first. As Thorndike had noted, however, animals did *in fact* "commonly repeat responses many times before changing them" (1911, p. 271).

To meet this difficulty, Guthrie rejected frequency in favor of a principle of postremity, as it later came to be known (Voeks, 1948). Watson had supplemented the principle of frequency with a principle of recency, according to which the last in a series of responses elicited by a given stimulus tended (all other things equal) to be evoked most readily when the stimulus next was presented. For Guthrie, it was *only* the last response in the series which was connected to the stimulus. He assumed that each connection between S and *R* was "an all-or-nothing affair, analogous to the setting of a switch" rather than to "the wearing of a path" (1930, p. 419). A single occurrence of an *R* in the presence of an S connected *R* to S and broke any connection that might have existed previously between S and any incompatible *R*; each S could be connected at a given time to only one *R*, and the last of a series of responses made in the presence of a stimulus was the one that remained connected to it. Reward terminated a training trial (that is, changed the stimulating conditions) and thus preserved the connection between the last response made (which was the rewarded response) and the trial stimuli. The gradual improvement in performance which could be observed in many learning situations, and which seemed to contradict the all-or-nothing notion, Guthrie attributed to variability from trial to trial in the conditions of stimulation. Many of the stimuli connected to the rewarded response on trial 1 were replaced on trial 2 by new stimuli, not previously connected to the response, and the response appeared with increasing readiness over a series of trials as it became connected to more and more of the stimuli which were likely to be present on any given trial. "It is entirely possible," Guthrie wrote, "that if Pavlov could have controlled all stimuli instead of a very few, conditioning would be definitely established with one trial instead of fifty or more" (1930, p. 420). Under ordinary circumstances, however, frequency was as important for Guthrie as for Watson, although it was not the number of times a given response was made that mattered, but the number of times that a trial was terminated by the response and the number of different stimuli that became connected to it.

Both Watson and Guthrie were tremendously impressed with Pavlov's work, which Watson helped to publicize (1916). The phenomenon of classical conditioning seemed to call for a contiguity theory, either of the S-S or the *S-R* variety. Certainly it did not seem to require any reference to effect; the conditioning procedure typically involved the presentation of some motivationally significant stimulus, such as food or shock, but the presentation was in no sense contingent upon the behavior of the animal. Pavlov himself was an *S-R*-contiguity theorist. In his formal statements (1927), he was not explicit as to the nature of the postulated connection—asserting only that the contiguous activation of two brain centers tended to establish a connection between them—but one of his blackboard diagrams (Fig. 4-4), which has been preserved by Frolov (1937, p. 46), shows a new *S-R* connection (*CS* center to *UR* center). Given the original (unconditioned) connection between the *US* and *UR* centers, conditioning could be explained also in terms of a new *S-S* connection (*CS* center to *US* center), but the *S-R* interpretation was satisfactory. When, however, one looked beyond the phenomenon of classical conditioning to the results obtained with problem box and maze, *S-R*-contiguity theory seemed less satisfactory. It was difficult to share the conviction of Watson and Guthrie that those results could be understood without reference to the motivational significance (the pleasantness or unpleasantness) of the consequences of response.

Watson (1917) trained two groups of rats to find their way into a box whose entrance was hidden in a pile of sawdust; one group, given food immediately on entry, learned no more rapidly than a second group, which found a covered food cup that was uncovered only after thirty seconds. Watson regarded these results as particularly damaging to Thorndikian theory—as Thorndike

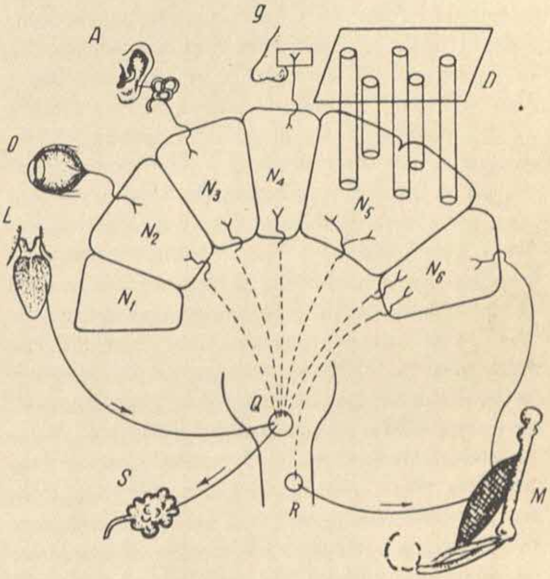


Fig. 4-4. Pavlov's diagram of the neural mechanism of conditioning. *A*, organ of hearing; *L*, organ of taste; *O*, organ of sight; *G*, organ of smell; *D*, organs of touch and temperature; *S*, salivary gland; *M*, skeletal muscle; *Q*, salivary motor center; *R*, skeletal motor center; N_1-N_6, cortical sensory areas; solid lines show unconditioned pathways and broken lines show pathways established by conditioning. Reproduced from Frolov (1937, p. 46) with permission of the publisher.

himself has recognized (1911), one of the implications of the effect principle was that immediate reward should produce better learning than delayed reward. It was possible, however, to account for Watson's results on the assumption that the delayed group had, in fact, been immediately rewarded by the smell of food and by other food-box stimuli (which acquired reward value by virtue of association with feeding). As Roberts (1930) later demonstrated, delays introduced between the correct response and entrance into the food box (rather than between entrance into the food box and access to food) markedly impaired performance. Would Watson's results have been the same if his delayed group had never been fed in the box at all? Grindley (1929) trained young chickens in a runway with the goal box containing food under glass, making the food unavailable; their running speed increased for a few trials but then fell off, while that of control animals, which did receive food in the goal box, continued to increase.

Watson (1914) also raised the question of the order of elimination of blinds in maze learning. It followed from the effect principle, he thought, that the order of elimination of blind alleys should be backward, the final portion of the maze being mastered first. Hubbert and Lashley (1917) agreed with Watson and provided an explicit deduction. They suggested that "the situation involved in the last turn before the food is reached may acquire from the getting of the food something (we cannot be more definite at present) which makes it capable of serving instead of food in the fixation of the next preceding activity" (p. 131), then the next to last turn from the last turn, and so forth. An experiment by Hubbert with Watson's circular maze did not, however, give clear evidence of a backward order of learning, which seemed to call the effect principle into question. Carr (1917) analyzed the data from several different mazes and found correlations ranging from −.88 to +.94 between the order of elimination of blind alleys and distance from the food box (with +1.00 indicating a perfect backward order). Although Carr was not favorably disposed toward effect theory, he pointed out that it was not discredited by such findings. There must be many factors affecting order of elimination, he argued, among which, certainly, was initial attractiveness. His own computation yielded uniformly negative correlations (from −.13 to −.89) between order of elimination and the number of entries into each blind on trial 1. A long series of subsequent experiments showed that order of elimination was indeed determined by many factors, of which the locus of reward was one (Munn, 1950).

However well suited to the facts of classical conditioning, and however attractive on purely formal grounds, *S-R*-contiguity theory did not in the end win many adherents. The Thorndikian facts simply made it impossible to support the fiction that *any* consequences of a response which removed the animal from the stimuli in whose presence the response was made would strengthen the tendency of the animal to make that response when the stimuli were encountered again. In some sense, the pleasantness or unpleasantness of the consequences of response had to be taken into account; on that point, agreement was almost complete. But how did pleasantness and unpleasantness operate—was Morgan correct, or was Thorndike?

A MIDDLE PERIOD

To answer that question was the principal concern of the thirties, forties, and early fifties, which were dominated by two men—Tolman and Hull—whose work we shall now consider. These years may be thought of as comprising a middle period, set off from the earlier one by the sharpness with which the issues had become drawn and by the methodological and theoretical sophistication of the protagonists. The period began with a rapid rise to ascendancy of *S-R*-effect theory and ended with its still more rapid decline.

Tolman. An influential opponent of Thorndike's theory was Edward C. Tolman (1932), whose view of learning was much like Morgan's, although he misunderstood Morgan, lumping him with Thorndike as a "trial-and-error" theorist. There were, of course, some important differences between Tolman and Morgan. For one thing, Tolman was willing, as Morgan was not, to assume certain inferential and inventive capacities in animals. For another, Tolman was impatient with neurophysiological speculation; like Morgan, he made free use of phenomenological terms, but the objective references which he sought for them were behavioral rather than neurophysiological: while *consciousness*, for Morgan, meant the activity of a certain portion of the nervous system, it meant, for Tolman, "an overt running-back-and-forth" which enhanced "differentiation and prediction" (1932, p. 216). Nevertheless, in his conception of learning (that is, of reproductive as distinct from what he characterized as "inventive" learning), Tolman had much in common with Morgan. An animal was assumed to learn *about* the world—it came on the basis of its experience to "expect" that a given stimulus would be followed by another or that a given action would have certain consequences. Tolman denied that sensory and motor systems were directly connected by rewards. Learning was one thing, performance quite another; having learned about the consequences of a given response, the animal would make that response only if it wanted ("demanded") the consequences. Although Tolman was not concerned with the mechanism by which expectancies and demands were translated into action, he did make certain quite explicit motivational assumptions which went beyond the earlier notions of pleasantness and unpleasantness. Demands originated in "disturbances" (produced either by noxious stimulation from without or by internal deficits and imbalances); demanded consequences were those which produced "quiescence" (cessation of pain, satiation), the demand for any given goal object at any given time depending on the nature of the prevailing disturbance. The animal made a response when it expected that the response would produce a demanded consequence.

In support of this view, Tolman cited the results of a number of experiments carried out in his own laboratory at the University of California. Elliott (1928) gave hungry rats one trial per day in a fourteen-unit multiple-T maze, rewarding an experimental group with bran mash and a control group with sunflower seeds for the first nine days, and both groups with sunflower seeds thereafter. Elliott's learning curves are reproduced in Fig. 4-5. The curves show that in the first stage of the experiment there was substantial improvement in both groups, more in the experimental than in the control; this result could be accounted for in Thorndikian terms on the assumption that bran mash was more rewarding than sunflower seeds and therefore produced stronger *S-R* connections. It was the results for the second stage which presented difficulties for *S-R*-effect theory. The performance of the control group in the first stage meant that sunflower seeds were rewarding, yet they produced only disruption (marked and immediate) in the performance of the experimental group (which previously had been given bran mash). Tolman took the results to mean that the animals were disturbed by the *change* from a more-preferred to a less-preferred food, and they could be disturbed by the change, of course, only if they remembered the food they had been given in previous trials. The results seemed to demonstrate, then, that the animals learned about the consequences of response.

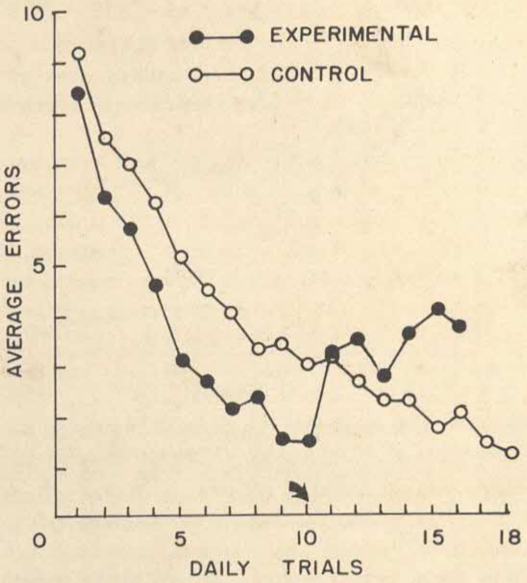


Fig. 4-5. The results of Elliott's experiment on shift of reward. The arrow indicates the day on which the experimental group found sunflower seeds for the first time. Redrawn from Elliott (1928, p. 26) with permission of the publisher.

Tolman and Honzik (1930) trained three groups of rats in the multiple-T maze, again with an intertrial interval of one day. A rewarded control group was fed in the goal box after each run; an unrewarded control group never found food in the goal box (the animals were fed two hours afterward in their living cages); while an experimental group was unrewarded for the first ten days and rewarded on each day thereafter. The learning curves are reproduced in Fig. 4-6. In the first stage of the experiment, the number of errors made by the rewarded control group was substantially less than the number made by the other two groups, but a single reward (given at the end of the eleventh trial) was enough to bring the error level of the experimental group on the twelfth trial down to that of the rewarded control group.

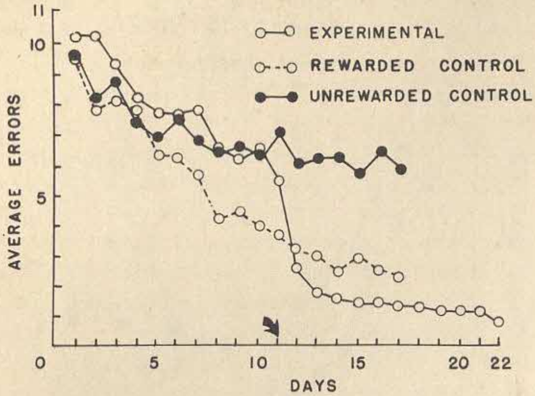


Fig. 4-6. The results of Tolman and Honzik on the introduction of reward. The arrow indicates the day on which the experimental group found food for the first time. Redrawn from Tolman and Honzik (1930, p. 267) with permission of the publisher.

From these results Tolman argued that, despite their poorer performance in the first stage of the experiment, the experimental animals were learning as much about the maze as were the rewarded controls; this "latent" learning was manifested with the introduction of a reward, which gave the experimental animals the same incentive as the rewarded controls to go directly to the goal box. If a goal box with food was more rewarding than an empty goal box, and if the unfed animals did indeed learn as much about the maze as the fed animals, then the results called into question the role of reward in learning. It was not reward, but stimulation, which produced learning, Tolman insisted. Rewards—learned about—influenced performance. Learning was one thing, performance quite another.

For almost thirty years after the publication of his *Purposive Behavior in Animals and Men* (1932), Tolman continued to defend this view. There were slight variations in emphasis and in terminology, transient hesitations and doubts engendered by unfavorable experimental outcomes, but an examination of the last formal statement of his theory (Tolman, 1959) reveals no fundamental change in conception. Throughout those years, however, Tolman's position was an unpopular one. The dominant theory of the middle period was an *S-R*-effect theory—not Thorndike's theory, but an extension of it by Clark L. Hull.

Hull. Hull's theory was developed in a series of papers in the *Psychological Review*, which began to appear in 1929, and which culminated in his *Principles of Behavior* (1943). Like Thorndike, Hull was an *S-R*-effect theorist, or (in what became the more common usage) an *S-R*-reinforcement theorist. The term "reinforcement" was used in several ways: sometimes it referred to the introduction of the *US* in a Pavlovian experiment (with no implication other than that the *US* was necessary for the appearance and the persistence of the *CR*); sometimes it referred to the motivationally significant consequences of response which were responsible for the changes in behavior that occurred in Thorndikian situations (with no implication of how the consequences produced the changes); and sometimes, like Thorndike's *effect*, it referred to motivationally significant consequences of response which were assumed to change the strength of *S-R* connections. Hull used the term in its Thorndikian sense. He preferred it to effect because it was more objective, and because he wished to avoid the implication that a contingent relation between response and reinforcement was necessary for learning; he meant to say only that a temporal relation was required. In salivary conditioning, he noted, the reinforcement (food) was in no sense contingent on the animal's response, yet it played an essential part in the learning. It should be evident from this that Hull found no difficulty in applying the *S-R*-reinforcement principle to classical conditioning. He simply assigned to the *US* a dual function—elicitation of the response to be connected to the *CS*, and reinforcement of the response. In salivary conditioning experiments with food as the *US*, food both elicited and reinforced salivation. In flexion-conditioning experiments with shock as the *US*, flexion was elicited by the onset of shock and reinforced by its termination.

In his characterization of reinforcing events, Hull was more explicit than Thorndike; for pleasantness, he substituted "drive reduction." (Hull, like Tolman, took pain and tissue need to be the mainsprings of behavior, and quiescence to be its goal.) In his *law of primary reinforcement* he held that, whenever a response occurred in temporal contiguity with the action of some stimulus on an animal's receptors, and the conjunction of stimulus and response was followed by a diminution in some prevailing need, there would result an increment ($\Delta_S H_R$) in the tendency for the stimulus on subsequent occasions to elicit the response. (Hull's term for the strength of an *S-R* connection was "habit strength," which he represented by the symbol $_SH_R$. Habit strength was assumed to increase as a simple positive growth function of the number of reinforcements whose asymptote varied directly with the amount of reinforcement (a simple positive growth function) and inversely with the delay of reinforcement (a simple negative growth function).

To account for results such as those of Watson on the delay of reward, Hull assumed that *S-R* connections could be strengthened by "secondary" as well as by "primary" reinforcers, that is, by stimuli which had acquired reinforcing properties by virtue of close and consistent association with drive reduction in the past. The acquisition of reinforcing properties by a stimulus was itself explained in *S-R*-reinforcement terms. The stimulus became connected to some fraction (g) of the consummatory or goal response (G) elicited by the primary reinforcer. The occurrence of g—the *fractional anticipatory goal response*—was assumed to have "a specific power of reinforcement in a degree proportionate to the intensity of that occurrence" (1943, p. 100); that is, the amount of secondary reinforcement was assumed to be a function of the magnitude of the fractional goal response elicited by the secondary reinforcer. In view of the time which elapsed between ingestion and absorption, Hull speculated that food itself might be a secondary rather than a primary reinforcer—that stimuli arising out of the ingestion of food became connected to terminal segments of the chain of internal activities leading to need reduction—and he proposed some sham-feeding experiments to settle the question.

On the basis of considerations reviewed thus far, it would seem appropriate to write Hull's name beside Thorndike's in the second row of our table of theories (and to write Tolman's beside Morgan's in the first row). Despite many differences in detail of terminology and of assumption, Hull's theory of learning, like Thorndike's, was an *S-R*-effect theory. Nevertheless, an extension of Table 4-2 is required; in Table 4-3, Tolman's name does appear beside Morgan's, but a fourth row has been added for Hull, because, unlike Thorndike, Hull had recourse to an activation principle. Although Thorndike had been able to assume that, given a strong connection between a stimulus and a response, the stimulus would elicit the response, that assumption was no longer tenable. Supplemented with an activation principle, *S-R*-reinforcement theory was, of course, placed at a formal disadvantage relative to S-S-contiguity theory (which involved no assumption about the role of effect in learning), but Hull did not even consider the merits of the S-S interpretation.

TABLE 4-3

Four learning theories contrasted

	Postulated connection	Effect a factor in learning?	Supplementary activation principle?
Morgan, Tolman	*S-S*	No	Yes
Thorndike	*S-R*	Yes	No
Watson, Guthrie	*S-R*	No	No
Hull	*S-R*	Yes	Yes

To say that Hull had recourse to an activation principle is, in fact, an oversimplification; he assumed that there were several important factors which determined whether a stimulus connected to a response would elicit that response. One of the factors was drive (D), which was assumed to convert habit strength into what Hull termed "reaction potential" ($_SE_R$). The postulated relationship was a multiplicative one: $_SE_R = {}_SH_R \times D$. It should be clear that each habit was assumed to be activated by all drives and not merely by the drive whose reduction had produced the habit. Given a bar-pressing habit established in a hungry rat by food reward, its $_SE_R$ at some instant of time could have a high value even if the animal was completely satiated for food; required only was that *some* need exist—for example, thirst. A thirst drive equivalent in intensity (D value) to the hunger drive as hunger would not, however, produce as much bar pressing, because the stimulating conditions would not be the same as those employed in training. The environmental stimuli might be the same, but the internal stimuli would not be the same because the drive was different. Although (by the principle of stimulus generalization) the new pattern of stimuli (S_b) would have some connection to the response because of its similarity to the training pattern (S_a), its generalized habit strength—written $_{S_b}\bar{H}_R$—would be less than $_{S_a}H_R$ (generalization decrement), and $_{S_b}E_R$ would therefore be less than $_{S_a}E_R$. Hull accounted in this way for some results reported by Elliott (1929), which Tolman (1932) had cited in support of the notion that rewards were learned about: the maze performance of rats trained hungry with food reward was disrupted immediately when the drive was shifted to thirst. Tolman, of course, had explained the disruption on the assumption that the animals expected food which was now inappropriate to their need.

It is instructive to compare Hull's conception of the role of drive in the translation of learning into performance with Tolman's conception as it was later formalized by MacCorquodale and Meehl (1954): ${}_{S_1}E_{R_1} = S_1R_1S_2 \times V$, with $S_1R_1S_2$ standing for the strength of the expectation that R_1 to S_1 would produce S_2, and V for the valence of S_2. Since the valence of a given goal object was assumed to vary with drive—that is, the valence of food would be higher for a hungry animal than for a thirsty one—an animal trained with food as S_2 would respond less readily when thirsty than when hungry. Drive-stimulus generalization also could be invoked by Tolman to produce a decrement in the strength of the expectation and a consequent decrement in reaction potential, but the primary burden was borne by V. Neither Hull nor Tolman was concerned with the mechanism of activation, each being content merely to specify the conditions under which a previously rewarded action would be performed; but those conditions were specified by both, and neither theory could be said to leave the animal any more deeply "buried in thought" than the other. It was not clear from the *Principles*, however, that Hull's modification of Thorndikian theory could meet all of the objections which Tolman had urged against it. The only data from the California laboratory cited by Hull in 1943 were those of Elliott on shift of drive. As we shall see, further modifications of the theory were required by the data on laten learning and by the data on shift of reward.

Whether ${}_SH_R$ would be reflected in behavior was determined, Hull assumed, not only by drive but also by a set of inhibitory factors. Although, in his original Law of Effect, Thorndike had assumed that *S-R* connections were strengthened by pleasant consequences and weakened by neutral or unpleasant ones, he was led subsequently by the results of his experiments on human learning (1931) to truncate the law—to deny that nonrewarding consequences weakened. Hull's effect principle also was truncated; ${}_SH_R$ was increased by drive reduction, but it was not decreased by drive increment. Hull assumed, in fact, that ${}_SH_R$, once developed, never disappeared. The tendency in a given situation to make a previously rewarded (but now unrewarded or punished) response declined, not because ${}_SH_R$ declined, but because an overriding inhibitory tendency developed. This aspect of the theory was borrowed from Pavlov (1927), who had been led to it by phenomena such as spontaneous recovery and disinhibition. For Hull, as for Pavlov, excitatory and inhibitory tendencies summated algebraically in the determination of behavior, although there were certain differences in the details of the two theories. Hull defined "effective reaction potential" (${}_S\bar{E}_R$) as total inhibitory potential ($\dot{I}_R$) subtracted from reaction potential: ${}_S\bar{E}_R = {}_SE_R - \dot{I}_R$. Total inhibitory potential was the sum of two components—a transitory, response-generated component, called "reactive inhibition" (I_R), which was dissipated by rest, and a permanent, conditioned component, which he called "conditioned inhibition" (${}_SI_R$). Pavlov had assumed that excitatory tendencies developed on reinforced trials (in his sense of the term "reinforced") and inhibitory tendencies on unreinforced trials, but Hull assumed that inhibition developed on all trials (I_R was produced by response and a certain proportion of it became conditioned), while ${}_SH_R$ developed only on reinforced trials (in his sense of the term). For Hull, a reinforced response was elicited more and more readily because each response produced more habit strength than inhibition; when reinforcement was withheld, the response was elicited less and less readily because each response produced only inhibition.

Hull's treatment of extinction was far more detailed than that of Tolman, who held simply that old expectancies gave way to new ones as the consequences of action changed. The expectation of food developed in the course of rewarded runs in a maze was supplanted in the course of subsequent unrewarded runs by the expectation of an empty goal box. A new-learning interpretation of extinction also was favored by Guthrie, who did much to demonstrate its plausibility as an alternative to the inhibitory interpretation. Tolman was not much concerned, however, with the phenomenon of extinction, and

Hull's inhibitory postulates are introduced here primarily for their bearing on other problems, which figured prominently in Tolman-Hull controversy.

Discriminative Learning. The Hullian movement derived considerable strength from its middle-period successes in the area of discriminative learning. Consider first the phenomenon of *transposition*, which had been demonstrated in experiments with a variety of species. Suppose that an animal was rewarded for going to the darker (G_1) of two simultaneously presented gray cards, but not for going to the lighter card (G_2), and suppose that, after it had learned to go consistently to G_1, it was required to choose between G_2 and a still lighter gray card (G_3). Under many circumstances, an animal so trained might show a strong preference for G_2, despite the fact that response to G_2 never had been reinforced. Tested with G_1 and a still darker gray card (G_0), it might show a strong preference for the new card despite the fact that it had been rewarded frequently in the past for response to G_1 and never for response to G_0. Results of this sort suggested that the animal had learned in its original training to respond, not in terms of the properties of the individual cards, but in terms of a relation between them. Tolman made much of the looking back and forth between one stimulus and another in which animals engaged at critical points in the learning of difficult discriminations; in this behavior, which Muenzinger (1938) labeled "vicarious trial and error" (*VTE*), one had direct evidence, Tolman asserted, that the animal was not "responding passively to discrete stimuli" but actively "selecting and comparing" the stimuli (1948, p. 200).

Although it was widely assumed that a demonstration of response to relations would provide support for Tolman's theory at the expense of Hull's, the assumption was without foundation; the question of relational learning actually was orthogonal to the principal difference between the theories. A demonstration of response to relations would require that any theory which made reference to the control of behavior by stimuli be supplemented with certain postulates about the relational determination of sensory properties, and there was no reason to believe that such postulates would be more difficult to incorporate in an *S-R* than in an *S-S* theory. Nevertheless, a claim by Kenneth W. Spence (1936, 1937), that all of the data which had seemed unmistakably to demonstrate relational learning could be derived from a theory like Hull's without any reference to a relational process, promised a striking victory for the Hullian way of thinking. Although it might be possible to develop S-S theory to the point at which it, too, could profit from Spence's insight, that was something for the future, while the Hullian derivation was already a fact. More damaging to Tolman's position, however, was the discredit that Spence's achievement brought to the anthropomorphic approach to the behavior of animals which Tolman espoused. Of course, Tolman suggested only that human experience might serve as a guide to the understanding of animal behavior—as a source of hypotheses which would be susceptible to objective, experimental test (Tolman, 1938)—but the danger existed that human experience would be weighted too heavily and the tests not carried far enough. It would be far better, perhaps, to ignore human experience altogether.

Spence's postulates varied to a certain extent from those which were later to be set forth in Hull's *Principles* (for example, Spence conceived of inhibition as developing only on unreinforced trials rather than on all trials, and his gradients of generalization were shaped differently), but the kinship of the two theories was extremely close; Hull's postulates of the same period (1937) were themselves different from those of the *Principles*. When an animal was rewarded for approaching a stimulus, Spence assumed, the tendency of that stimulus to excite an approach-response increased, and the increase in excitatory value generalized to other stimuli as a function of their similarity to the rewarded one. When approach to a stimulus was not rewarded, an inhibitory tendency developed, which, like the excitatory tendency, generalized to other stimuli as a function of similarity. The net excitatory value of any stimulus was given by the algebraic sum of its excitatory and its inhibitory values. Given a choice of two stimuli in a simple simultaneous problem, the animal would approach the one with the greater net excitatory value.

Suppose that an animal was trained to discriminate between two stimuli (*d* and *e*) different in size, with choice of the larger (*e*) being rewarded. The results of training might be represented, Spence suggested, by curves like those of Fig. 4-7. The abscissa was chosen to represent

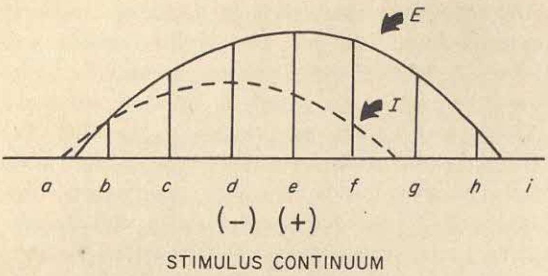


Fig. 4-7. Spence's derivation of transposition. E, excitatory tendency; I, inhibitory tendency. Redrawn from Spence (1937, p. 433) with permission of the publisher. In subsequent papers, the curves were drawn somewhat differently. See, for example, Spence (1942), p. 259.

the size continuum, and on it the rewarded (+) and unrewarded (−) stimuli were plotted, along with various other stimuli to either side of the training pair. The ordinate represented the excitatory and inhibitory values developed along the continuum in the course of training, the unbroken curve showing the spread (generalization) of excitation (E) about the positive stimulus, and the dashed curve showing the spread of inhibition (I) about the negative stimulus. The difference between the two curves at any point gave the net excitatory value (NE) at that point. Note that the net excitatory value of d was assumed to be positive at the end of training despite the fact that the choice of d had never been rewarded; the generalized E was assumed to exceed the I developed by reward. The animal chose e rather than d, because the excess of E over I was even greater at e than at d. By the same token, an animal thus trained should be expected to choose f rather than e, and d rather than c—Spence had derived transposition from a set of assumptions which made no reference to perception of relations!

The simple set of postulates that yielded this derivation, which was long to rank among the prettiest conceptual achievements of psychology, seemed to have enormous explanatory power. It explained not only the occurrence of transposition when tests were made with stimuli similiar to the training pair, but the failure of transposition when tests were made with stimuli far removed from the training pair. It also explained the finding (Spence, 1942) that in certain problems there was no transposition at all. A chimpanzee rewarded for choosing the largest of three stimuli might be expected to transpose when tested with a new triad similar to the training triad, but the results would be quite different if it was rewarded to begin with for choosing, not the largest, but the intermediate member of the triad. The theory suggested that the intermediate-size problem would be a difficult one, because of the generalization of I to the positive stimulus from the negative stimuli on each side of it, and the problem was, in fact, a difficult one. Lashley (1938) earlier had taken the relative difficulty of the intermediate-size problem for rats as evidence against an absolute theory and in favor of a relational one, arguing that, if the animals were responding to absolute properties, it should be no more difficult to train them to choose the intermediate stimulus than the largest one; he accounted for his results on the assumption that it was the *relation* of intermediacy that his rats found difficult to grasp. Spence's theory not only accounted for the relative difficulty of the intermediate-size problem without a relational assumption, but it led to the prediction that the solution of the problem, once achieved, would not be transposed; after training with $a-$ $b+$ $c-$ had produced greater NE at b than at a or c, the animal should continue to prefer b to c when confronted with the triad bcd, and this deduction, too, was confirmed.

It often happened that experimental results which seemed at first to contradict Spence's theory turned out upon further study to be capable of derivation from the theory. Consider, for example, Pavlov's finding (1927) that an animal which failed to discriminate two closely similar stimuli (for example, $d-$ $e+$) would succeed after training with two less similar stimuli on the same continuum (such as $c-$ $f+$). Lawrence (1952) took similar results obtained in a Thorndikian situation to mean that animals had to learn in such a problem about the significance of certain *differences* in stimulation as such, larger differences being perceived more readily than smaller ones. Spence could deal with such results, Lawrence thought, only by postulating unique generalization gradients and by making other unlikely assumptions, but the compatibility of the results with Spence's theory is easy to demonstrate. In Fig. 4-8, the I-curve of Fig. 4-7 has been shifted one unit to the left and the E-curve one unit to the right, representing the hypothetical outcome of training with $c-$ $f+$. Note that the difference between the *NE* values of *d* and *e* is greater in Fig. 4-8 than in Fig. 4-7; in other words, training with *c*− *f*+ would be expected to contribute more, under these circumstances, to the discrimination between *d* and *e* than differentially reinforced experience with those stimuli themselves!

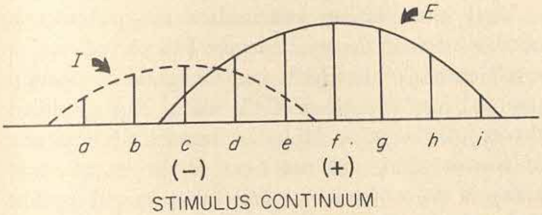


Fig. 4-8. Derivation of the Pavlov-Lawrence finding on the transfer of discrimination. A comparison of this plot with that of Fig. 4-7 shows that training with *f*+ *c*− produces better differentiation between *e* and *d* than does training with *e*+ *d*−.

Despite the many successes of Spence's theory, however, data began to appear with which the theory was indeed inadequate to deal, and which seemed after all to require the assumption of response to relations. In an experiment with rats, opportunity for comparison was found to facilitate discrimination between similar stimulus cards, an outcome which suggested that the animals were responding to perceived differences (Saldanha and Bitterman, 1951). Further work on the discrimination of intermediate size by chimpanzees produced clear evidence of transposition (Gonzalez, Gentry, and Bitterman, 1954), an outcome which Spence himself had insisted could not be deduced from his theory. Other experiments provided the basis for an even more fundamental criticism of the theory, calling into question the assumption that differential reinforcement was a necessary condition for discriminative learning. In an experiment with rats, nondifferentially reinforced experience with two stimulus cards was found to improve performance when the animals subsequently were required to discriminate between them (Bitterman, Calvin, and Elam, 1953).

Closely related to the question of relational learning was the question of continuity. Spence asserted that all stimuli falling on the animal's receptors at the time of a rewarded response became connected to that response (the *continuity* position). When, for example, an animal chose a black card in the left window of a jumping apparatus and was admitted to food in consequence, both blackness and leftness acquired excitatory value; if the response was not rewarded, both sensory components acquired inhibitory value. Tolman and his students, together with Lashley, took the position—again clearly derived from human experience—that animals dealt selectively with stimuli (*noncontinuity*). Lashley (1942) asserted that any complex of stimuli falling on the receptors was immediately organized, some elements becoming dominant, others becoming ineffective, and that only those elements which were dominant on a particular trial became "associated" with the response. As Spence would have put it, the noncontinuity theorists were assuming that, when an animal was rewarded for going to black on the left, blackness might gain in excitatory value and leftness be entirely unaffected, or the reverse, depending upon which component was dominant. The notion of perceptual selectivity seemed to be supported by some observations of Krechevsky (1932), who trained rats in a four-unit apparatus which presented both a spatial and a brightness difference at each choice point, but with no component consistently related to reward. Under such conditions, an animal might show a strong left-going preference for a series of trials, then shift rather abruptly to a strong brightness preference for another series of trials, and so forth. The preferences were taken by Krechevsky to reflect the perceptual dominance of one or another of the stimulus components, but Spence (1936) was able to derive them from his theory, which of course made no reference to perceptual selectivity.

Lashley (1942) reported some experiments which seemed to him to establish that, if an animal was given a set for response to one aspect of a stimulus situation, large amounts of training with other aspects would teach the animal nothing about them as long as the original set remained effective. For example, rats were trained to choose the larger of two circles, whereupon a large triangle was substituted for the larger circle and training was continued (with the large triangle positive) for 200 trials—more than twice the number of trials usually required for an initial discrimination between triangle and circle; yet when the animals subsequently were tested with a triangle and a circle of equal area, they showed no preference for the triangle. In one such experiment, Lashley did find a preference in the third stage for the new component of the stimulus situation which was associated with reward in the second stage, but he attributed this finding to a shift in perceptual organization resulting from the introduction of the new component. It was possible, however, for critics of Lashley to take this result as the valid one, and to explain the null outcomes of the other experiments in terms of the insensitivity of the preference tests which were employed. An experiment patterned after those of Lashley, but so designed that a third-stage preference could not reasonably be attributed to a second-stage shift in perceptual organization, gave continuity results; differential reinforcement of spatial cues in the context of what might be assumed to be a "set" for brightness produced substantial spatial preferences (Bitterman and Coate, 1950). Either Spence was correct, or the noncontinuity theorists had underestimated the perceptual flexibility of the rat.

Experiments of another design did, as it turned out, provide clear evidence of noncontinuity (Teas and Bitterman, 1952). Suppose that rats were trained in the jumping apparatus with two pairs of cards—two grays differing in brightness (with the brighter card always on the left) and two vertically striped cards differing in stripe thickness (with the thicker stripes always on the right)—and suppose that the animals were reinforced for choosing the brighter gray and the thicker stripes; how would animals which had reached criterion on such a problem respond when tested for the first time with the same cards in the alternative lateral arrangements? Spence's theory admitted only of two possible outcomes: one was that the animals would continue to go to the previously reinforced member of each pair; the other was that the animals would go in both cases to the same side, since it was conceivable that a spatial preference had persisted which was stronger than the weaker of the two visual preferences established by differential reinforcement. What actually happened, however, was that the animals went to the previously *un*reinforced member of each pair —the darker gray (now on the left) and the thinner stripes (now on the right). This outcome could be understood on the assumption that the animals had learned simply to go left when the cards were gray and to go right when they were striped. If, in the original training, each pair of cards was presented in both lateral arrangements, constituting a four-situational rather than a two-situational problem, the animals would begin to go consistently to the reinforced member of each pair, but the two-situational problem was dealt with in terms of the difference *between* the two *pairs* of cards, and the animals learned little about *within-pairs* differences despite a great deal of differential reinforcement (Turbeville, Calvin, and Bitterman, 1952).

More than fifteen years after the publication of the original theory, Spence (1952) offered a modification of it which was designed to encompass a broader section of the data. The modified theory, although lacking the beautiful simplicity of the original, did not even so begin to meet many of the objections which were raised against the original (Bitterman, 1953). Clearly, the problems of discriminative learning were more complex than earlier had been envisaged and, while there still was no reason to believe that *S-S*-contiguity theory promised an easier solution to those problems than did *S-R*-reinforcement theory, the special advantages which once could be claimed for *S-R*-reinforcement theory—and for the approach to problems of animal behavior which it represented—had been lost.

Latent Learning. A phenomenon that had a quite direct bearing on the central issues which divided learning theorists of the middle period was latent learning. Results such as those obtained by Tolman and Honzik (see Fig. 4-6), and by a number of investigators both before and after—see especially Blodgett (1929)—seemed to make a certain amount of difficulty for Hull, although they were not in themselves, as the cognitive theorists often implied, sufficient reason for scrapping the *S-R*-reinforcement theory. From the fact of improvement in the performance of the experimental animals prior to the introduction of food, it had to be inferred that some sort of reward was operating in the goal box—the inference was required of Tolman as much as of Hull—although the magnitude of reward (for Tolman, *valence*) was not as great for the experimental animals as for the rewarded controls; the growth functions for the two conditions should therefore have different asymptotes and so also should the corresponding performance curves. With an increase in the magnitude of reward (introduction of food) for the low-reward (previously unfed) group, its $_{S}H_{R}$ function should move sharply upward to a new asymptote appropriate to the greater magnitude. Hull did not take up the problem of latent learning in the *Principles*, but he considered a hypothetical case in which two groups of animals were trained with two different amounts of reward, following which the training of both groups was continued with the higher amount. The only difference between the curves drawn by Hull to illustrate the outcome of such an experiment (1943, p. 130) and those obtained in experiments like Tolman and Honzik's was that the shift in performance produced by the introduction of food in the latter experiments often seemed to be much more abrupt; in the Tolman-Honzik case, for example, the performance of the experimental group was as good or better than that of the rewarded control group on the very next trial after the first encounter with food. The abruptness of the shift suggested a change in ${}_SE_R$ rather than in habit strength, which was precisely the interpretation that Hull offered some years later, when, as we shall see, he extended his list of the determinants of ${}_SE_R$.

The irrelevant-incentive experiment was another kind of latent-learning experiment that attracted considerable attention in the middle period. Tolman and Honzik had asked essentially whether the animal could learn the path to an empty goal box. In the irrelevant-incentive experiment, the question asked was whether the animal could learn the path to a goal box whose contents, although potentially rewarding, were not at the moment of any interest. Consider, for example, an experiment by Spence and Lippitt (1946). Thirsty rats, satiated for food, were trained in a simple *Y*-maze with the right turn leading to a goal box which contained water, and the left turn leading to a goal box which contained food for one group (Group *F*) but was empty for a second (Group *O*). Under these conditions, of course, the animals quickly developed a right-going preference, and a system of forced choices was required to equate experience with the two goal boxes. The question which interested Spence and Lippitt was how the animals would respond when the prevailing drive was shifted from thirst to hunger. If Tolman was correct, it was argued, the animals of Group *F* should go to the left, since they had been given an opportunity to learn that food was on the left, and they now wanted food. According to *S-R*-reinforcement theory, by contrast, the hungry animals should continue to go to the right because of the differential reinforcement of the right-going response in the first phase of the experiment. The results obtained gave not the slightest reason to believe that the animals of Group *F* had learned anything about the location of food; every animal in both groups went to the right on the first trial after the shift in drive from thirst to hunger, nor was there any difference in the rate at which the two groups developed a left-going preference in subsequent trials, when both groups were fed in the left goal box.

Results of this sort, which were replicated several times, pointed rather clearly to an inadequacy of the *S-S*-contiguity principle. At the very least, the results required that the principle be supplemented (as both Morgan and Tolman had been led by other facts to supplement it) with an *S-R*-contiguity-frequency principle. The notion was that once an animal selected an appropriate mode of response in a situation—the selection being based on learning about the consequences of the various alternative responses—the representational or expectational process was somehow abridged or "short-circuited" (Morgan's term) in the course of further trials, with the response coming to be elicited much more directly by the stimulus. While differential frequency of occurrence "is not the cause of the initial selection" of a response, Tolman wrote, "continued exercise of the response, *after it has been selected*, will cause it to become excessively stamped in" (1934, p. 383, italics original). From this point of view, it was possible that the animals of Spence and Lippitt's Group *F* had learned something about the location of the food in the first stage, but that the learning was masked by the operation of a strong right-turning tendency which also was established in that stage.

To examine this possibility, Grice (1948) performed an irrelevant-incentive experiment of what could be called the *balanced* type (in contrast with the design of Spence and Lippitt, which could be characterized as *unbalanced*). Thirsty rats were trained for twelve days in a simple *T*-maze with forced choices to equate experience of the two sides; both goal boxes contained water and the left goal box contained in addition a large quantity of food. Under such conditions, the animals would not be expected by either theory to develop a strong preference for one or the other side. With drive shifted from thirst to hunger, however, there should be a higher percentage of runs to the left on the very first trial if the animals had indeed learned about the location of the food, while *S-R*-reinforcement theory led to the expectation that no preference for either turn would be manifested. The results obtained by Grice confirmed the prediction from *S-R*-reinforcement theory. Not only did the animals show no significant preference for the left side on the first trial after the shift (twelve of the twenty-three animals turned left) but a relearning design —which might have been expected to provide a more sensitive indication of food-location learning —also produced a null outcome: half the animals, for whom the food was shifted to the right, learned to go to the right at the same rate as the remaining animals, for whom the food was kept on the left, learned to go to the left.

In the hands of other investigators, experiments like Grice's did sometimes give evidence of irrelevant-incentive learning, a necessary condition for which seemed to be the spatial separation of relevant and irrelevant incentives (Thistlethwaite, 1952; McAllister, 1952). Evidence of irrelevant-incentive learning also was obtained in experiments of another balanced design: satiated animals trained with food in the left goal box and water in the right goal box made appropriate choices when tested hungry or thirsty (Spence, Bergmann, and Lippitt, 1950). It was possible, however, to deal with both outcomes in *S-R*-reinforcement terms by appeal to the concept of the anticipatory goal response (r_G). Consider the latter experiment. The left arm of the maze was assumed to become connected, in the course of training, to anticipatory eating (the response, elicited to begin with by the sight of food, would provide its own reward), while the right arm would become connected to anticipatory drinking. When the animal looked into the left arm, then turned into it, feedback from the anticipatory eating elicited by the sight of the arm would become connected to turning left (the connection would be reinforced, presumably, by the r_G), and, in a like manner, feedback from the anticipatory drinking would be connected, on other trials, to turning right. If, on the basis of its previous experience, a hungry animal could be assumed to be making anticipatory eating responses—that is, if the S_D for hunger had come in the past to evoke the r_G for food—appropriate behavior was to be expected, since feedback from anticipatory eating had been differentially connected in training to turning left; by the same token, a thirsty animal would be expected to turn to the right. In experiments like those of Thistlethwaite and McAllister, a thirsty animal finding food and water closely juxtaposed at the end of one arm of the maze would not be expected to do much anticipatory eating, especially if it could be assumed that anticipatory eating and anticipatory drinking were incompatible, and there would consequently be little opportunity for feedback from anticipatory eating to become connected to the turn into that arm. With the water moved further back into the arm, however, first the food itself, and then the entrance to the arm, might come to evoke anticipatory eating despite the prevailing thirst drive.

In experiments of yet another balanced design, animals were trained with an irrelevant incentive in one goal box and the second goal box empty, after which they were tested when motivated for the previously irrelevant goal object. A number of investigators, like Strange (1950), reported irrelevant-incentive learning under such conditions. In an interesting variation of this design, Johnson (1952) trained animals deprived of water for different periods of time (zero to twenty-two hours) with food in one goal box and nothing in the other; tested hungry, the groups less thirsty in training chose correctly, but the groups more thirsty in training did not. The *S-R*-reinforcement interpretation suggested that the higher thirst drives had prevented the elicitation by food of anticipatory eating responses. For Tolman and his group, Johnson's results, like those of Thistlethwaite and McAllister, seemed to call once more for some supplementary assumptions about the role of drive in perception—the results of Spence and Lippitt with the unbalanced design had earlier led to the speculation that a thirsty rat might simply not "perceive" food—but those assumptions never were put in testable form. On the whole, then, *S-R*-reinforcement theory came off rather well in the irrelevant-incentive experiments, while Tolman's theory was embarrassed, not because the data were incompatible with an expectational view, but because it had come off second best in an area in which its strength once had been thought to be maximal.

A third kind of latent-learning experiment—the placement experiment—was clearly reminiscent of one of Thorndike's early designs. Honzik and Tolman (1936) trained rats in a *T*-maze with one shorter and one longer arm leading to distinctive goal boxes, each containing food. When a preference for the shorter path to food had developed, the animals would be placed directly into the goal box at the end of that path and shocked, following which another measure of preference would be made. Under certain conditions, a consequent decrease in the preference for the shorter path was observed, despite the fact that the animals had not been punished for *choosing* that path. Maier (1932*a*, 1932*b*) allowed rats to explore three distinctive tables interconnected by elevated pathways, then carried them to one of the tables and fed them there; the test was to put the animals down on one of the other tables to see which way they would go. Maier reported that normal adult rats would show a strong preference for the path leading to the table on which they had been fed despite the fact that they never had been rewarded for choosing that path.

Two points should be made with respect to Maier's work. First, Maier's interest went beyond the controversy over the nature of learning; he was interested in reasoning, which he defined as the ability to recombine previous experiences in the solution of a new problem. Maier took it for granted that the animals learned about the situation (which path led to which table) in the first phase of the experiment, and the location of food in the second phase; what concerned him was the nature of the process which enabled the animal to utilize this information in choosing a path in the third stage. His finding that older rats were more successful than young ones (1932*c*), and that moderate cortical lesions would impair performance on this problem without impairing performance on what he regarded as tests of learning (which involved rewarded runs to a given table), suggested that the problem involved a function quite different from learning. A second point to be made with respect to Maier's work is that it involved repeated tests with the same animals (with the locus of feeding changed from test to test). This procedure apparently introduced some transient interference; the performance of the animals would be good to begin with, then fall off to chance, and then improve gradually, until a high level of accuracy was attained once more (Wolfe and Spragg, 1934).

Consider, finally, a more modern version of the placement experiment. Seward (1949) permitted rats to explore a gray *T*-maze with empty goal boxes, one black and one white, after which they were placed in one of the goal boxes and fed; brought back to the starting point, a substantial majority turned in the direction of the goal box in which food had been given. Could such results be understood in terms of *S-R*-reinforcement theory? One possibility to be considered was that some of the stimuli affecting the animals in the second stage of the experiment (while they were being fed in one of the goal boxes) might also affect them at the choice point. If so, the appropriate behavior of the animals on the test trial might be thought of simply as an approach to stimuli which produced anticipatory eating. Even if the distinctive properties of the goal box itself were not visible at the choice point, extra-maze stimuli might be. Brown (1939) found that the Honzik-Tolman design gave positive results in some rooms but not in others, which led him to believe that room lighting was important. Seward got positive results when the goal box in which the animals were fed in the second stage occupied its original position in the room, but the performance of his animals fell to chance when the box was placed behind the starting box in the second stage of the experiment. Extra-maze cues were not, however, sufficient in themselves to give better than chance performance, Seward reported; in a control experiment with undifferentiated goal boxes, the animals showed no preference for the path to the rewarded location. Maier (1932*b*) found that blind rats performed at a high level of accuracy in a tactually differentiated three-table situation, but extra-maze cues other than visual have been found to play a role in the maze learning of rats.

A second possibility to be considered was that the stimuli employed to differentiate the two goal boxes elicited characteristic responses which became connected in the first stage of the experiment to stimuli arising from the corresponding arms. In the second stage, proprioceptive stimuli from those responses were paired with food and came as a result to evoke anticipatory eating. In the third stage, the animal entered the alley which evoked a response whose feedback elicited anticipatory eating. The comfort which could be drawn from such an explanation by an *S-R*-reinforcement theorist was, however, rather scant. The assumption that any two discriminable goal boxes would, without special training being given, evoke responses with discriminable feedback was a dubious one, and the assumption that such responses became conditioned to maze stimuli in the course of unrewarded exploration, when there was not even removal from the maze to fall back upon as a source of reinforcement, carried little conviction. Here, then, was an area in which properly designed experiments might have some unique value.

The Nature of Conditioning. Although many of Hull's principles were suggested by Pavlov's findings, it was doubtful that the theory could deal with the fact of classical conditioning. In the interpretation of salivary conditioning with food as the *US*, there was the awkwardness of having to assume that the response to be conditioned was both elicited and reinforced by the *US*. In salivary conditioning with acid as the *US*, immediate reinforcement could be found only in the dilution of the acid by saliva, yet the administration of atropine, which prevented salivation, did not interfere with conditioning (Finch, 1938). Hull's consideration of flexion conditioning with shock to the limb as *US* was restricted to the case in which the termination of shock was contingent on flexion and therefore immediately followed flexion, but conditioning occurred even when the duration of shock was entirely unaffected by the animal's behavior, or was so brief that the shock terminated before much of any response to it could get under way.

Experiments on avoidance made it necessary for the Hullians to assume that neutral stimuli paired with noxious ones would acquire drive properties. If the conditions of training were such that a certain response to the *CS* forestalled shock, the tendency to make that response was strengthened. Hunter (1935) found that a response elicited by shock came more readily to be elicited by a signal for shock when the response avoided shock than when shock was unavoidable. It also was demonstrated that the tendency to make a given response to a stimulus could be maintained by shocking the animal for each failure to respond to the stimulus, even though the shock itself did not elicit the response (Finch and Culler, 1934). Such results could be accounted for on the assumption that a shock-avoiding response was reinforced by the reduction of a secondary drive, later called "anxiety" or "fear" (Mowrer, 1939). The *CS*-acquired drive properties by virtue of its pairing with shock on those trials on which the avoiding response was *not* made, and the termination of the *CS* by the avoiding response when it *was* made reinforced the response in much the same way as the termination of shock reinforced an escape response. It was not, then, the omission of shock which reinforced the avoiding response, but the withdrawal of a drive-inducing pattern of stimulation. As might be expected from this point of view, learning was better when the avoiding response terminated the *CS* than when it did not, but there was substantial success even when the duration of the *CS* was unaffected by response (Mowrer and Lamoreaux, 1942). To account for this success, it could be assumed that the animal learned to discriminate between two patterns of stimulation, one which did and one which did not include the proprioceptive and other sensory consequences of the avoiding response; since the *CS* was paired with shock only in the absence of feedback from the response, the *CS* acquired drive properties while *CS*-plus-feedback did not, and the change in stimulation produced by response was drive reducing.

How was the acquisition of drive properties by a *CS* to be understood in Hullian terms? One could only say that some fraction of the response to shock or other noxious stimulus, which itself had drive properties, became connected to any neutral stimulus contiguous with the noxious one, with the reinforcement allegedly essential for the connection being supplied by the termination of the noxious stimulus. Hull did not consider the problem in the *Principles*, but subsequently he made quite explicit his view that "secondary motivation" like secondary reinforcement could be derived as a corollary from the *S-R*-reinforcement postulate (1952, p. 6). A neutral stimulus followed by an abrupt increment in drive and then by an abrupt decrement in drive would acquire drive properties.

Unable to accept this contrived interpretation, yet convinced of the merit of the *S-R*-reinforcement principle, Mowrer (1947) was led to the conclusion that two different learning processes operated in avoidance situations. The acquisition of drive properties by stimuli paired with shock was attributed to an *S-R*-contiguity process (referred to as "conditioning") and the acquisition of the avoiding response to an *S-R*-reinforcement process (referred to as "problem solving"). Mowrer suggested that the two principles might apply to different classes of behavior—the contiguity principle to autonomic responses and the reinforcement principle to skeletal responses—although he did not make clear why the reinforcement interpretation was more adequate to the conditioning of leg flexion with unavoidable shock than it was to the conditioning of fear.

The notion that more than one process of learning would have to be postulated if all the data were to be accounted for was not, of course, a new one. Morgan had found it necessary to supplement his *S-S*-contiguity principle, as had Thorndike his *S-R*-effect principle, with an *S-R*-contiguity principle. In fact, both Skinner (1935) and Schlosberg (1937) already had taken dual-process positions quite similar to the one proposed by Mowrer, Skinner having been influenced primarily by procedural considerations—his comparison of the stimulus-pairing procedure with the reward procedure stands today as a classic—and Schlosberg by the results of some conditioning experiments with avoidable and unavoidable shock. Both stressed the difference in the nature and function of the responses thought to be governed by the two learning principles, contiguity controlling the acquisition of behavior characterized as diffuse and preparatory, reinforcement the acquisition of behavior characterized as precise and productive. Mowrer's contribution was to show that avoidance learning could be understood in terms of acquired drive and to point up the implausibility of the notion that an animal only learned to be afraid because being afraid was quickly followed by fear reduction.

One implication of the *S-R*-reinforcement interpretation of acquired drive was that fear-conditioning should be better with a brief shock than with one of longer duration—the briefer the shock, the briefer the delay of reinforcement (supposedly provided by shock termination). One should not reject the interpretation simply because it led to such an implausible prediction, Miller (1951) cautioned and he cited an unpublished dissertation by J. J. Sullivan on GSR-conditioning in human subjects as providing some confirmation of the prediction. An examination of Sullivan's paper revealed, however, that his results might have been due to a marked original inequality in the reactivity of his experimental groups (Bitterman, Reed, and Krauskopf, 1952). Subsequent work with rats by Mowrer and Solomon (1954), whose groups were properly matched, produced results contrary to those predicted from *S-R*-reinforcement theory. The possibility existed, of course, that the longer *US* tended to give better conditioning because it produced greater drive (by summation) and its termination consequently brought greater drive reduction—a possibility which Miller considered—but the burden of proof had once more been shifted to *S-R*-reinforcement theory.

Another line of research which bore on the validity of *S-R*-reinforcement theory was concerned with *sensory preconditioning*. The sensory-preconditioning paradigm was invented by Thorndike as a test of the *S-R* conception of learning. His plan was, first, to give an animal the opportunity to associate two sense impressions (*A* and *B*); next, to train the animal to make some response (*C*) when presented with *B*; and, finally, to test with *A*, which might be expected to suggest *B* and so to produce *C*. "Such a result would, if all chance to associate *C* with *A* had been eliminated, demonstrate the presence of representations and their association," Thorndike wrote (1898, p. 75). Unfortunately, Thorndike did not have the "resources" for such an experiment, and the variant which he found "practicable" did not, as he recognized, have the same "crucial" significance, but experiments like the one he did consider to be crucial were later performed many times. For example, Brogden (1939) simultaneously presented two neutral stimuli, light and bell, in the first stage of an experiment with dogs. In the second stage, one of the two stimuli (*A*) was paired with avoidable shock to the leg until the animal had reached the criterion of twenty avoidances in twenty trials. In the third stage, the *other* stimulus (*B*) was presented alone. Of eight animals trained in this way, *B* elicited flexion in six; extinguished to the criterion of twenty successive failures to respond, they made a total of eighty-three responses. Of eight control animals for whom the first stage of training was omitted entirely, only one responded with flexion to *B*, making a total of four responses to extinction. Brogden concluded that pairing the two neutral stimuli in the first stage of the experiment established a relation between them.

Positive results were obtained also in subsequent experiments with more adequate controls. For example, Silver and Meyer (1954) paired light and buzzer in the first stage of their experiment with rats—first one of the stimuli (*A*) would come on for 1 sec. and then, after an interval of 0.5 sec., the second stimulus (*B*) would come on for 1 sec. For one group of animals, *B* was paired with avoidable shock in the second stage, and training was continued to the criterion of seven avoidances in ten trials, after which, in the third stage, *A* was paired with avoidable shock and training carried to the same criterion. For a second group, *A* was used in the second stage and *B* in the third stage. The first group learned much more rapidly in the third stage than did the second group; that is, with stimuli paired in the sequence *A-B* and animals then trained to make some response to one of the stimuli, a learning measure showed more transfer from *A* to *B* than from *B* to *A*. There was a greater tendency, apparently, for the first of two stimuli presented in sequence to produce some surrogate of the second than for the second to produce some surrogate of the first. This was precisely the outcome to be expected if the same principles applied to the pairing of neutral stimuli as to the pairing of a neutral stimulus with food or shock; when a *CS* *followed* the presentation of food in a salivary conditioning experiment, little if any tendency developed for the *CS* to elicit salivation.

Although Thorndike conceived the sensory-preconditioning experiment as a crucial test of *S-R* theory, the subsequent invention of hypothetical mediating responses deprived it of much of its significance. The *S-R* theorist had only to assume that *B* evoked some unobserved response (R_b) which became connected to *A* in the first stage of the experiment, and that the proprioceptive feedback from R_b, like *B* itself, became connected to the indicator response (*C*) in the second stage of the experiment; then, in the third stage, *C* would be expected to follow *A* because *A* would evoke R_b and the feedback from R_b would evoke *C*. For the *S-R*-reinforcement theorist, however, there was not only the problem of specifying a mediating response but also the problem of specifying the drive reduction allegedly required to connect that response to *A*. The assumption was necessary that the *B* of any sensory-preconditioning experiment which gave positive results had drive or reward properties, and the assumption in many cases was not an easy one to make.

There were, of course, certain sensory-preconditioning experiments (or related experiments—it might be argued that the designation *sensory preconditioning* should be reserved for experiments on the effect of pairing neutral stimuli) in which *B* was far from neutral. Prokofiev and Zeliony (1926) conditioned a dog with a metronome as the *CS* and acid as the *US* until the salivary response to the metronome was well established, after which the acid alone was paired with shock to the limb until it began to elicit flexion. Then the metronome was reintroduced. Here was a case in which a quite real mediating response was available—*A* came in the first stage to elicit a response which also was elicited by *B*—yet, in the third stage, the metronome elicited salivation but not flexion. This result was embarrassing not only to the *S-R* theories, which now depended on the efficacy of mediating responses, but also to S-S theory: if auditory and gustatory centers had been connected in the first stage, and if the gustatory stimulus had come, by whatever process, to evoke flexion in the second stage, why should the auditory stimulus not have evoked flexion in the third stage?

There were other bits of evidence which seemed to contradict the *S-S* interpretation of conditioning. Harlow (1937), working with monkeys, paired a bell with a fear-eliciting stimulus (e.g., a party favor called a "snake blowout") until the bell elicited fear, then adapted the animal to the blowout, and found that the bell continued to elicit fear; in fact, fear of the blowout could be reestablished by pairing it with the bell! Clearly, the bell had come to evoke fear quite independently of any sensory linkage to the blowout. A similar experiment was reported by Konorski (1950). Two dogs were trained to respond to a tone with flexion of the leg, which avoided a puff of air to the ear, and the air puff alone then was paired with food until it elicited salivation; in subsequent tests, the tone alone continued to produce flexion while the puff alone continued to produce salivation. Whether or not anticipation of the puff played a role in acquisition, it was not responsible for maintaining the response to the tone.

In general, then, the experiments on conditioning did not provide conclusive support either for an *S-S* or for an *S-R* interpretation. Indeed, there was some basis for the conclusion that *both* *S-S* and *S-R* connections might be formed (Hilgard and Marquis, 1940). For the assumption of need reduction as an essential factor in conditioning, there was, however, no substantial evidence at all, and few theorists (whatever their persuasion with respect to the data of Thorndikian experiments) were willing in the end to conclude that the data of Pavlovian experiments could be fitted into the Hullian mold.

A Major Revision of Hullian Theory. In 1949, Hull began to issue a series of revised postulate sets which culminated in the publication, a few months after his death, of *A Behavior System* (1952). The new book differed from the *Principles* in many respects, both formal and substantive, but most important from the point of view of the controversy with Tolman was a change in the conception of the role of reward in learning, which represented a major concession to the traditional cognitive position. Reward now had two functions for Hull: it produced ${}_{S}H_{R}$ by virtue of its occurrence (the new learning postulate said simply that habit strength was a function of the *number* of reinforcements) and it contributed to reaction potential by virtue of its magnitude (the new activation postulate made reaction potential a function of ${}_{S}H_{R} \times D \times K$, with K representing the magnitude of reward). Consider again the case of two groups of rats trained in a runway, one with a smaller and the other with a larger amount of food, whose speeds approached different asymptotes, higher for the group trained with the larger amount of reward. In 1943 (*Principles*), the difference in speed was attributed to a difference in habit strength; in 1952, habit strength (a function only of the number of reinforcements) was assumed to be the same for the two groups, the difference in speed being attributed to a difference in K.

In an experiment reminiscent of Elliott's early work with bran mash and sunflower seeds, Crespi (1942) studied the effects on runway performance of increases and decreases in amount of reward. Some representative results are plotted in Fig. 4-9. As the curves show, training with 4, 16, and 64 units of reward per trial produced different asymptotic running speeds. Animals shifted from 4 to 16 units showed an abrupt increase in speed to a level which exceeded that of animals trained from the outset with 16 units (Crespi referred to this overshooting as an "elation" effect); animals shifted from 64 to 16 units showed an abrupt decrease in speed to a level below that of animals trained from the outset with 16 units (Crespi referred to this undershooting as a "depression" effect). The depression effect was, of course, directly analogous to the effect on Elliott's rats of a shift in kind of reward (from the more-preferred bran mash to the less-preferred sunflower seeds).

To the theory of the *Principles*, Crespi's data, like Elliott's, seemed to present some insurmountable difficulties. The abruptness of the change in performance was puzzling, as was the fact that the performance of the animals shifted from higher to lower amounts of reward declined at all (since ${}_{S}H_{R}$ was assumed to be irreversible and D was unchanged, ${}_{S}E_{R}$ could not decline, while the inhibitory determinants of behavior were independent of reward); and the contrast effects (elation and depression) implied a learning about reward with which the older theory certainly could make no contact. In 1952, the changes in performance as well as the differences in asymptotic level originally produced by different amounts of reward were attributed to K, and, since K implied some learning about reward, the contrast effects could be treated as emotional in nature; Hull did not spell out the additional postulates from which they might be derived, but at least they had been brought into range of attack. The question of the abruptness of the change in performance with change in reward now was put in different perspective; while the approaches of the shifted groups to the asymptotes of the sixteen-unit control groups were abrupt, the approaches to the elated and depressed levels, which now could be viewed unflinchingly, looked like simple growth functions. Hull could deal with the latent learning data of Tolman and Honzik in exactly the same terms as with Crespi's data—the

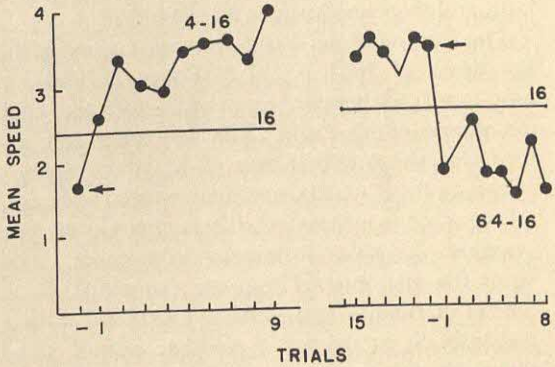


Fig. 4-9. Crespi's results on shift of reward. The left panel shows the performance of a group shifted (arrow) from 4 to 16 units of reward. The left panel shows the performance of a group shifted (arrow) from 64 to 16 units of reward. The horizontal lines show the control levels (for groups trained throughout with 16 units). Redrawn from Crespi (1942, p. 508) with permission of the publisher.

addition of food to whatever reward had produced some improvement in the performance of the hitherto unfed experimental group was treated simply as an increase in the amount of reward.

Still to be specified, however, was the source of *K;* the symbol had been added to the equation for $_{S}E_{R}$, but it had not yet been given any functional meaning. If the amount of reward on trial N was to enter into the activation of the instrumental response on trial $N + 1$ twenty-four hours later, it must produce some stable change in the animal. If the animal was to react emotionally on trial $N + 1$ to the difference between the amount of reward on that trial and the amount on trial N, some surrogate of the trial-N amount must be present on trial $N + 1$. Morgan and Tolman had assumed, of course, that the properties of reward were recalled as such, an assumption that *S-R* theory was intended from the beginning to circumvent, yet Hull's resort to K was all but an open admission that the concept of learning about reward could not be dispensed with in the analysis of selective learning. The admission seemed to deprive *S-R*-reinforcement theory of what once was its principal reason for being.

RECENT DEVELOPMENTS

Hull's final revision of his theory marked the end of an era of widespread allegiance to the *S-R*-reinforcement principle and the beginning of a transitional period, which is already about fifteen years old. We must look now at the major developments of this period, their relation to the past, and their implications for the future.

The Decline of S-R-Reinforcement Theory. With the death of Hull, it fell to Spence (1956), a long-time collaborator, to preside over the final dissolution of his theory. The first order of business was the interpretation of K, which Spence traced to the fractional anticipatory goal response, taking K as a measure of the magnitude of that response. Consider a runway experiment, with R_i as the instrumental response (running); S_i as the set of runway stimuli to be connected to that response; S_b as the set of goal-box stimuli; G as the food; R_G as the consummatory or goal response; and r_G as the anticipatory goal response, which would be connected, not only to S_b, but to S_i (with which it also was contiguous). The magnitude of r_G would increase with that of R_G, which in turn would increase with the amount of food in the goal box. The excitatory potential or E (indexed by running speed) then would depend, according to Spence, on *two S-R* connections—$_{S_i}H_{R_i}$ and $_{S_i}H_{r_G}$—as well as on drive: specifically, the relation assumed was $E = H \times (D + K)$, with H representing the instrumental connection and D the level of drive. A larger reward would produce faster running than a smaller one because the magnitude of the r_G elicited by S_i would be greater in the first case than in the second. Spence was unable to decide, however, whether the greater r_G should be attributed to a greater $_{S_i}H_{r_G}$ (more time spent eating the larger reward might be assumed to produce greater $_{S_b}H_{r_G}$, and, by stimulus generalization, greater $_{S_i}H_{r_G}$), or whether the r_Gs of different magnitude should be treated as different responses which were acquired as such; either alternative was fraught with difficulty.

With learning about reward reduced to r_G-conditioning and assigned an important role in the activation of instrumental behavior, Spence no longer saw any reason to assume that reward was necessary for the development of instrumental habit strength, and he went over to an *S-R*-contiguity interpretation. In the case of classical conditioning, however, he continued to favor an *S-R*-reinforcement interpretation. Thus, while the mere contiguity of S_i and R_i was assumed to be sufficient for the development of $_{S_i}H_{R_i}$, the development of $_{S_i}H_{r_G}$, which was treated as an instance of classical conditioning, was assumed to require reinforcement (supplied, presumably, by the *US*). In conditioning with shock, the *termination* of the *US* was assumed to be responsible for connecting the neutral stimulus to whatever responses were elicited by the shock. An *S-R*-contiguity interpretation of classical conditioning would have been more parsimonious, of course, since the principle already had been admitted in the instrumental case; but Spence clung *at the cost of parsimony* to the Hullian explanation of classical conditioning, which many subscribers to the original Hullian explanation of instrumental learning had rejected *at the cost of parsimony* as implausible.

Quite apart, however, from the question of whether reward was necessary for conditioning, there was the question of whether the concept of r_G-conditioning did full justice to the role of reward in instrumental learning. Consider, for example, a difficulty arising from the assumption that K, like D, was a perfectly generalized activator—that it activated all instrumental responses connected to S_i: why, then, should the animal be more likely to make a rewarded response rather than an unrewarded response with an equal or a greater frequency of previous occurrence? Hull had been willing to assume that the development of ${}_{S_i}H_{R_i}$ was dependent upon the *occurrence* of reinforcement if not on the amount and delay of reinforcement. The assumption was an awkward one, but it did provide a mechanism for overriding the effects of frequency. Spence's equally awkward assumption was that reinforcement, although unnecessary for the development of instrumental habit strength, was essential to forestall extinction. In contrast to Hull's notion that inhibition developed with each evocation of a response and habit strength only with each *reinforced* evocation, Spence's notion was that habit strength developed with each evocation and inhibition (I_n) only with each *unreinforced* evocation. Unfortunately, certain facts of response selection still remained to be accounted for (Bitterman, 1957). To appreciate the inadequacy of the interpretation, one had but to consider, for example, a situation in which two incompatible responses were equally often made and rewarded, one with a larger amount of food than the other. Even if S_i came to evoke, not one, but two different r_Gs (one for each amount of food), each K should tend to activate both of the responses, and no preference for the more abundantly rewarded alternative should develop.

A peculiar implication of the r_G interpretation of K was that instrumental performance would improve with the similarity of S_i to S_b. S_i could, of course, be connected directly to r_G, but, quite independently of such a connection, S_i would be expected to elicit r_G by stimulus generalization; the greater its similarity to S_b, the greater would be its tendency to elicit r_G, the greater the K, and the greater the ${}_{S_i}E_{R_i}$. It was in these terms, for example, that Spence made certain predictions about the results of placement. Suppose that hungry rats, after a number of unreinforced trials in a *black* runway leading to an empty *black* goal box, are placed directly either in a *black* goal box or in a *white* one, and fed. Tested again in the runway, Spence deduced, the animals fed in black should run faster, because, by the principle of stimulus generalization, the black runway should elicit more vigorous anticipatory goal responses in those animals than in the animals fed in white. An experiment by Stein (1957) did not, however, show any substantial difference between the two groups, while an experiment by Gonzalez and Diamond (1960) gave some indication of a difference opposite in direction to the predicted one.

Gonzalez and Diamond also trained rats in a runway which was different in color from the empty goal box to which it led. For example, after some unrewarded trials in a white runway with a black goal box, the animals were placed directly either in a black or in a white box and fed; tested again in the white runway, the group fed in black ran much more rapidly than the group fed in white. This outcome was quite the opposite of that which was predicted from the r_G interpretation—the runway should have generated more ${}_SE_R$ when it was the same color as the feeding box than when it was a different color—but quite predictable, of course, from the cognitive view. If the animals had learned that the white runway led to a black box, feeding in a white box would scarcely be expected to produce rapid running; if anything, it would be expected to increase the tendency to loiter in the white runway, and there was some evidence that it did just that.

The results for all four of the groups studied by Gonzalez and Diamond are reproduced in Fig. 4-10. The captions are to be interpreted as follows: $R_xB_x - B_x$ means that the runway, the goal box, and the feeding box, are the same color (black for half the animals and white for the rest); R stands for the runway, the first B for the empty goal box, and the second B for the feeding box, while the subscripts x and y show similarities and differences in color. In Fig. 4-10, the results for the test trials (all unreinforced) are plotted (as are those for the training trials) in blocks of four, but the results for the very first test trial (uncomplicated by secondary reinforcement) are identical to those for the first block. The curves show clearly that performance was facilitated when the color of the feeding box was that of the goal box rather than that of the runway. Feeding in a box of the same color as the runway tended, in fact, to impair performance, which seemed to mean that the hypothetical r_G (to the extent that it was elicited in the runway) in some way *competed* with the instrumental response rather than facilitating it as the theory required.

There were other difficulties to be met before learning about reward could be reduced to r_G-conditioning. Consider the problem of escape: what consummatory response was elicited when

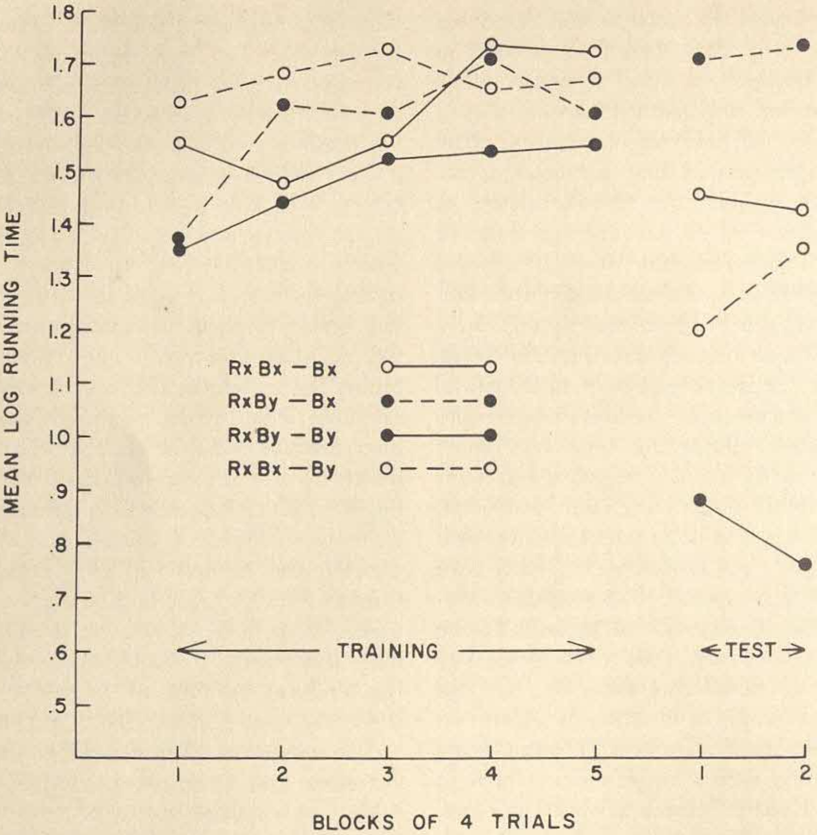


Fig. 4-10. The results of Gonzalez and Diamond on placement. The *R* in the key to each curve represents the runway, the first *B* represents the empty goal-box, and the second *B* represents the box in which the animals were placed and fed; *x* and *y* denote colors (black or white). Thus, $R_xB_x - B_y$ means that the runway and goal box were of one color, while the feeding box was of the other color. Redrawn from Gonzalez and Diamond (1960, p. 400) with permission of the publisher.

an instrumental response was rewarded by the termination of some aversive stimulus such as shock, and to what r_G was one to look for *K*? Spence admitted that he could not deal with escape training and speculated that its laws might be "quite different" from those of instrumental training with food as reward, but escape was not the only problem. There were stimuli whose onset was rewarding but which were not consumed in any meaningful sense of the term. What, for example, was the r_G in rats rewarded with a flash of light (Marx, Henderson, and Roberts, 1955), or in monkeys rewarded with an opportunity to look out of the experimental enclosure into the laboratory (Butler and Harlow, 1954) or to hear a recording of colony sounds (Butler, 1957)? Observations of this sort had, in fact, been reported very early. One of Thorndike's monkeys, chancing to hit a projecting wire in such a way as to make it vibrate, repeated the response hundreds of times in the next few days; the animal did not "eat" or "make love to" the sound, Thorndike (1901) remarked, but seemed simply to like to listen to it. Both Kinnaman (1902) and Watson (1908) had monkeys that spent a great deal of time peering through small openings, and almost every subsequent student of primate behavior made similar observations. Exponents of the need-reduction theory of reward could and did assert that sights and sounds were rewarding only to the extent that they had become connected in the past life of the animal to fractions of consummatory responses evoked originally by primary need-reducing stimuli; but there was renewed respect in the fifties for the idea that, just as certain stimuli were aversive in themselves, so others could be attractive in themselves. While an *S-R*-reinforcement theorist was not committed necessarily to the need-reduction interpretation of reinforcement, if stimuli were rewarding *qua* stimuli, then learning about reward meant learning about stimuli.

Whatever the validity of Spence's view of learning about reward, he had also to face the question of how animals learned about consequences of response which had no motivational significance, and which therefore could not be assumed to evoke consummatory responses. Consider again the Gonzalez-Diamond experiment. On the first test trial, the running time of the $R_xB_y - B_y$ animals was much lower than that of the $R_xB_x - B_y$ animals. Since extra-maze cues and intra-maze cues were identical for the two groups—the room location of the apparatus was the same, and the color of the goal box could not be seen before the instrumental response was made—the difference in performance suggested that the $R_xB_y - B_y$ animals somehow learned in the first stage of training that the runway led to a goal box of a particular color. It might be assumed that the color of the goal box evoked some characteristic response which became connected to runway stimuli in the first stage, and that the proprioceptive feedback from the response became connected to anticipatory eating in the second stage, but something more was required. If the speed of the $R_xB_y - B_y$ animals increased only to the extent that r_G was evoked (via feedback from the hypothetical mediating response) in the runway, the increase in running speed should have been at least as great in the $R_xB_y - B_x$ animals, which would not have been dependent on the mediating response. Clearly, the Gonzalez-Diamond results presented the gravest sort of challenge to Spence's theory.

Two S-R-Contiguity Theories. As the weakness of the *S-R*-reinforcement principle became increasingly evident in the fifties, interest in the *S-R*-contiguity principle revived on two fronts: Estes (1950, 1959) resurrected Guthrie's theory, which he cast into mathematical form, while Mowrer (1956, 1960*a*, 1960*b*) reconsidered his earlier two-factor theory and decided that the *S-R*-reinforcement principle could be dispensed with entirely.

Estes' theory is very similiar to Guthrie's, although the intimacy of the relation between them often is unappreciated. Estes assumes that the occurrence of *R* in the presence of S, or in the first half-second after its termination, connects *R* to S and disconnects all incompatible responses. When the consequence of *R* is to terminate S, and when no response incompatible with *R* occurs in the next half-second, the connection between S and *R* is preserved. The half-second assumption, which is to be found also in the formalization of Guthrie's theory by Voeks (1950), extends Guthrie's strict contiguity principle. The extension makes it possible for the theory to deal with the fact that *R* is not immune from extinction simply because the stimuli that elicit it terminate upon its occurrence; for a brief moment after their termination, their connection with *R* may give place to a connection with some incompatible response elicited by a stimulus which is produced by *R*.

Like Guthrie, Estes assumes that *S-R* connections do not vary in strength; S and *R* either are connected or they are not. The gradual change in performance typically observed in learning experiments is explained by Estes (as it was by Guthrie) in terms of stimulus sampling. Each learning situation is represented as a population of stimulus elements which is sampled on each trial. The probability of *R* is assumed to vary directly (and its latency inversely) with the proportion of the stimulus elements in any given sample which are connected to *R*, and it is the gradual change in that proportion from trial to trial which the gradual change in performance reflects. The proportion of sample elements connected to *R* is directly related to the proportion of population elements connected to *R*. The rate at which the proportion of connected population elements increases over trials (indexed by the slope of the learning curve) is determined by θ, the ratio of the number of elements in the average sample to the number of elements in the population; when $\theta = 1$, training is complete in a single trial, but when θ is small, performance improves slowly.

The Guthrian interpretation of the role of reward in instrumental learning is no more tenable in the sixties than it was in the thirties. Even Estes must admit that it does not "stand on its own feet as a determinate and testable" proposition, because there is no basis for deciding "in advance of an experiment" whether or not a particular response tendency will be strengthened by a given consequence (1959, p. 434). Clearly, the Guthrian interpetation is circular in a way that the Thorndikian is not. If saccharine is found to strengthen the tendency to make some response (R_1) which produces it, the Thorndikian predicts that it also will strengthen the tendency to make any other response (R_2) which produces it—with the rewarding properties of saccharine established by the first experiment, no other outcome is to be expected in the second—but the Guthrian cannot make a prediction. The first experiment tells him that the response to saccharine is compatible with R_1, but nothing about its compatibility with R_2. If he finds in a long series of experiments that saccharine strengthens the tendency to make a large variety of *Rs* instrumental in producing it, he can only conclude that the response to saccharine, however avid, is singularly specific, and he must reach the same conclusion about the response of sexually deprived male rats to females in heat, however incompatible the sexual response may appear to be with traversing a runway or with operating a lever; but any further change in the nature of the instrumental response still must leave him in doubt as to whether learning will occur.

The sort of evidence which led Hull and Spence to the concept of *K* presents great difficulty for Guthrian theory. Consider Crespi's experiment (which Estes does not). Even before the shifts in amount of reward are introduced, there is the faster running to the larger reward which must be explained. In a discussion of reinforcement parameters (1959, pp. 433-434), Estes attributes faster running to a larger amount of food to the fact that a larger amount takes longer to consume and therefore delays the occurrence of responses incompatible with running which might otherwise be connected to runway stimuli. When the amount of food is small enough to be eaten rapidly, interfering responses may occur soon enough to be connected to runway stimuli, and the effect may be that of having given no food at all. With increasing amounts of food, the probability that this will happen decreases, and performance must vary just as it must be predicted to vary when the probability of reward increases; that is, the asymptotic probability of response must increase, and the asymptotic latency must decrease. (If, for example, a response is rewarded only on a random 50 per cent of trials, the asymptotic proportion of population elements connected to the response will be 0.50, and the expected proportion of sample elements connected to the response becomes 0.50. If response is rewarded on 75 per cent of trials, the corresponding proportions become 0.75, and so forth.)

It follows from this interpretation that increases in amount of food beyond the amount which cannot be consumed in less than half a second should have no effect on performance, since (by the modified contiguity assumption) runway stimuli cannot be connected to responses which occur more than half a second after the animal has left the runway; but this derivation is patently contrary to fact. Furthermore, runway experiments from Estes' own laboratory (Weinstock, 1958) show that large variations in probability of reward may have no marked effect on asymptotic running speed. Weinstock's results not only contradict Estes' analysis of amount of reward (because they show that variations in probability of reward do not act like variations in amount), but they cast considerable doubt on the validity of the stimulus-sampling notion, which (as has already been noted) requires an inverse relation between latency of response and probability of reward. With the differences in asymptotic running speed as a function of amount of reward unaccounted for, it is perhaps not surprising that the theory provides no basis for understanding the subsequent changes in performance with changes in amount of reward which Crespi reported. Those changes in performance were precipitous, while the sampling conception requires that they be gradual, because (as Estes notes) θ cannot be affected by amount of reward. The elation and depression effects are obviously beyond the reach of a theory which makes no provision for learning *about* reward.

Although the stimulus-sampling model was developed by Guthrie as a supporting framework for the *S-R*-contiguity principle, Estes believes it to have considerable merit in its own right. He suggests, in fact, that he would continue to work within that framework even if he should be required after all to adopt the reinforcement principle. Actually, however, the sampling model which Estes finds so attractive is contradicted by the data at a variety of points. Since many of the contradictions have been reviewed by Estes (1959) himself (who often seems to see them clearly enough, although they cause him surprisingly little concern), it will be sufficient here to consider a major failure of the theory which Estes does not see clearly, but instead regards as an "achievement"—"the successful prediction of asymptotic response probabilities under a variety of random reinforcement schedules" (p. 410). This alleged achievement has indeed, as Estes notes, aroused wide interest, and it is responsible in large measure for whatever favorable reception the theory is accorded.

If, on any trial, one of two alternative responses is rewarded with the probability π and the other is rewarded with the probability $1 - \pi$, Estes deduces that the percentage of population elements connected to the first response will approach π as training continues. In consequence, the mean proportion of sample elements connected to the first response will approach π, and the mean probability of that response on any trial will approach π—that is, the choice ratio will approach the reward ratio. In support of this deduction, Estes cites a *T*-maze experiment conducted in collaboration with Lauer in which rats trained with a 75:25 reward ratio came *as a group* to make the more frequently reinforced turn on about 75 per cent of trials. What Estes does not note, however, is that this outcome is an artifact of averaging. Little evidence of probability matching appeared in the data for individual animals, which tended either to 'maximize' (that is, to go on all trials to the more frequently rewarded side) or to 'minimize' (that is, to go on all trials to the less frequently rewarded side); the mean asymptote of 75 per cent for the group as a whole simply reflected the fact that maximizing was more common than minimizing (Bitterman, Wodinsky, and Candland, 1958, p. 107, ftn. 18). In a more recent report from Estes' laboratory by Hickson (1961), evidence is presented of a correspondence between choice ratio and reward ratio which holds for individual rats, but a further analysis of the data indicates that the correspondence was produced by reward following (the animals tending to make on each trial the rewarded response of the preceding trial). A still more recent study of monkeys (Wilson, Oscar, and Bitterman, 1964) yields another nonrandom correspondence: superimposed on a preference for the more frequently rewarded alternative, there was a tendency to *avoid* on each trial the rewarded alternative of the preceding trial—a tendency often shown by human subjects. In the main, however, rats and monkeys have been found to maximize in these experiments.

The data on which Estes relies primarily for evidence of probability matching comes from work with human subjects, in which matching often is found; but so also is maximizing. Which result will be obtained seems to depend on a complex of attitudinal factors with which the theory makes no contact at all (Goodnow, 1955; Morse and Runquist, 1960). One dependable finding is that human subjects match when they are looking for a principle which will permit correct choice on all trials; under such circumstances, they try out a succession of strategies which are geared to the observed probabilities of reinforcement. It should be evident that Estes' derivation of matching is not confirmed by just any correspondence of choice ratio to reward ratio. The derivation leads us to expect a distribution of choices which is essentially random. In fact, the theory is contradicted sharply by nonrandom patterns of choice that appear under conditions in which the pattern of reward is random.

Random probability matching does occur in fishes (Behrend and Bitterman, 1961), in birds (Bullock and Bitterman, 1962; Graf, Bullock, and Bitterman, 1964), and in insects (Longo, 1964), as well as in extensively decorticated rats (Gonzalez, Roberts, and Bitterman, 1964), which suggests that the model at least may have some application at lower neural levels; but matching has been found in some of these animals even under conditions in which it cannot be predicted from the model—that is, in *discriminative* as contrasted with *spatial* problems. The failure of the theory to account for matching in discriminative problems is perhaps best understood in terms of its failure to account for perfect discrimination under conditions of consistent differential reinforcement (the 100:0 case). In a spatial problem, better termed a *generalized* problem (Bitterman, 1962), the population of stimulus elements sampled typically remains the same from trial to trial. In a discriminative problem, by contrast, samples are drawn from two overlapping populations of stimulus elements, $S_1 + S_c$ and $S_2 + S_c$ (with S_1 and S_2 representing the unique features of each population, and S_c the common elements). In the 100:0 case, the animal is reinforced consistently for making one response to samples of stimuli drawn from the first population, and for making an alternative response to samples of stimuli drawn from the second population. If the two populations are sampled equally often (as typically they are), and if the number of common elements is large relative to the number of unique elements in each population (as must be assumed, for example, in a color discrimination), then the asymptotic proportion of the elements in each population connected to each response alternative should be close to 0.50, and the accuracy of performance should not deviate markedly from chance; yet errorless performance in such problems is easy to demonstrate. In the 70:30 case, 70 per cent of samples drawn from the first population and 30 per cent of samples drawn from the second population are connected to one response, while 30 per cent of samples drawn from the first population and 70 per cent of samples drawn from the second population are connected to the alternative response. Matching in the 70:30 case, like matching (= errorless performance) in the 100:0 case, can be expected only when the number of common elements is zero.

Consider an experiment in which a pigeon is trained to peck at one of two keys, both of which are green on half the trials and red on the remaining trials (Graf, Bullock, and Bitterman, 1964). If the left response is consistently rewarded when the keys are green and the right response is consistently rewarded when the keys are red, the animal soon comes to peck the left key whenever the keys are green and the right key whenever the keys are red. If the reward ratio is 70:30 when the keys are green (left response rewarded on 70 per cent of trials) and 30:70 when the keys are red (left response rewarded on 30 per cent of trials), the animal comes to make the left response on 70 per cent of green trials and on 30 per cent of red trials. That is, the animal behaves in each case as though there were no stimulus elements common to red and green trials and (because the transition from 100:0 to 70:30 performance is gradual) as though red and green were substantial populations of stimulus elements with low sampling probabilities. Not only the pigeon, but also the fish and the decorticated rat show probability matching in discriminative problems, although the pigeon and the decorticated rat tend to *maximize* in spatial problems (where matching, if it occurred, *could* be accounted for by the theory). We may conclude, then, that experiments on probability learning cast considerable doubt on the validity of the sampling model, whose empirical failures at many other points are even more apparent. To the extent that pure *S-R*-contiguity theory (or any other theory) must depend for its existence on the sampling model, it does not seem to have much future.

The *S-R*-contiguity theory recently developed by Mowrer (1956, 1960*a*, 1960*b*) is a frequency theory and therefore does not require a sampling framework. It is not subject to the difficulty on which Watson's theory foundered (the dominance of rewarded instrumental responses over unrewarded alternatives with equal or greater frequency of previous occurrence), because it makes no reference to instrumental learning as such. All learning for Mowrer is affective conditioning. In the early version of his theory, the "fear" evoked by aversive (need-increasing) stimuli was assumed to be connected to contiguous neutral stimuli. In the present version of his theory, what he calls "hope"—the conditionable affective component of all responses to rewarding (need-reducing) stimuli—also is assumed to be connected to contiguous neutral stimuli. Skeletal responses are not connected to stimuli, but their feedback (exteroceptive and proprioceptive) is conditioned to the affective processes aroused by rewards or punishments which follow them; the tendency to make some skeletal response is increased by reward or decreased by punishment *only* to the extent that its feedback becomes connected to hope or to fear. Since there can be no feedback to elicit hope or fear until the animal has begun to make the response which produces it, Mowrer assumes that the animal constantly scans its entire repertory of responses—actually begins to make each response of which it is capable to the point at which the feedback can be evaluated—continuing responses with hopeful feedback and inhibiting responses with fearful feedback.

To anyone who has observed a well-trained, highly motivated rat streak through a complex maze, Mowrer's account of instrumental response selection must be implausible in the extreme; there simply is not time enough for the scanning and evaluating process which he postulates. Low-latency behavior of this sort seems to point unmistakably, as Morgan early realized, to the establishment of direct connections between stimuli and instrumental responses. (Morgan's idea was that a selective process based on evaluation of the anticipated consequences of response operated only early in instrumental learning, being short-circuited eventually by an *S-R*-contiguity-frequency process.) There are many other reasons to question Mowrer's selective principle, quite apart from its failure to account for highly practiced behavior. Consider, for example, the case in which one response (R_1) to a given stimulus is rewarded with x units of food, while an incompatible response (R_2) is rewarded with $2x$ units: why should R_2 prevail? Since either response, once initiated, engenders hope, the preference for R_2 suggests that the animal must in some sense remember, when considering R_1, that there is another response which has more satisfactory consequences; but Mowrer makes no reference, as did Morgan, to learning *about* response. Experiments on irrelevant-incentive learning suggest, furthermore, that learning about the consequences of response is not restricted to affective properties. Consider again the case in which satiated rats are trained in a maze with food in the left end box and water in the right, or with food in the left end box and nothing in the right; depriving the animals of food now produces a marked increase in the probability of a left turn. Mowrer's theory does not explain why the change in drive should increase the hope evoked by feedback from the scanned left turn.

Of the many other kinds of experiment that contradict Mowrer's theory, it is perhaps most important to look at placement, if only because Mowrer leans heavily on placement results in support of the idea that instrumental responses are not learned as such. Consider the Gonzalez-Diamond experiment. Increases in the running speed of groups $R_xB_xB_y$ and $R_xB_yB_y$ may be accounted for by Mowrer (as he accounts for other such results) on the assumption that running toward the end box increases the incidence of extra-maze stimuli to which the hope was conditioned during the placement-feedings (which were carried out with the feeding box in the position usually occupied by the end box). The difference between the two groups—the greater increase in the speed of $R_xB_yB_y$—whose treatment differed only with respect to the color of the end box encountered in the first stage of the experiment, cannot, however, be accounted for in these terms, since the color of the end box on test trials was not visible until the animal had reached it. In his discussion of other such results, Mowrer talks vaguely of response-produced mediating stimuli, but he does not make clear how (if learning is restricted to the conditioning of hope and fear) mediating responses of the sort that might represent a black box as compared with a white one can be learned.

Mowrer's notion that the function of reward is to facilitate ongoing behavior (rather than to strengthen *S-R* connections or to activate previously established connections) has found substantial acceptance in one form or another, although his proposal to dispense entirely with instrumental habits has not. Neal D. Miller (1963), exponent for many years of a loose *S-R* reinforcement theory, now takes a position which differs (he says) from Mowrer's mainly in the assumption that instrumental responses are connected to eliciting stimuli. According to Miller, reward serves primarily to trigger a "go mechanism" (which intensifies "current activities") and only secondarily to strengthen *S-R* connections—all responses, including the activity of the "go mechanism," may be connected to stimuli by contiguity alone. Miller is eclectic about what is connected as well as about the conditions of connection: the *R* in *S-R* may be a movement, he notes, but it also may be a percept or an image! F. D. Sheffield (1965), long a supporter of Guthrie, recently has proposed that anticipation of reward may serve to facilitate whatever behavior is in progress at the time. Anticipation of reward—a conditioned consummatory response—can be initiated only by the correct response, he explains, because the reward is presented and the consummatory response to it is made only in the presence of feedback from the correct response. The facilitation of the correct response by anticipation of reward ensures that it will be the most likely response to occur and eventually will dominate through "sheer frequency" of occurrence. Behavior is guided by anticipation of reward in the first stage of instrumental training, Sheffield concludes in a passage reminiscent of Morgan, then later is reduced by motor conditioning to an habitual form.

The New Empiricism. Where, then, do matters now stand? It should be clear that none of the three basic principles which have been advanced to account for learning will itself permit us to deal comfortably with all of the data. The S-S-contiguity principle, from which we can get to instrumental activity only on the assumption of an intervening process of anticipation and evaluation, is inadequate to the automatic, nonreflective character of highly practiced behavior and to certain facts of classical conditioning. The *S-R* reinforcement principle is inadequate to learning about reward as well as to certain facts of classical conditioning. Unbuttressed by an activation model, the *S-R*-contiguity principle does not do justice to the role of reward in instrumental learning; the frequency version is contradicted directly by the prevalence of rewarded responses over unrewarded responses with greater frequencies of prior occurrence, while the postremity version is tied to a treatment of reward that is circular at best and to a stimulus-sampling conception whose relation to reality is undeniably remote. With an activating role assigned to reward, new ways are opened to *S-R*-contiguity theory which remain for the most part to be charted, but certain limitations already are apparent. Sheffield's use of a more general *S-R*-contiguity principle avoids some but not all of the difficulties facing Mowrer—he cannot, for example, explain the development of a preference for a more-rewarded as compared with a less-rewarded response when both are made equally often, or learning about the color of an empty end box.

Will any *pair* of the three principles permit us to deal with all of the data? Combination of the two *S-R* principles (Thorndike's combination) seems still to leave the problem of learning about reward, although Spence's theory may not be the best that can be done with it. It also leaves the problem of learning about motivationally insignificant consequences of response. Combination of the two contiguity principles (Morgan's combination) is, however, without any obvious empirical shortcoming. The one remaining possibility of combination is *S-S* contiguity with *S-R* reinforcement (Maier and Schneirla, 1942), to which Morgan's combination is preferable on formal grounds alone: the substitution of *S-R* reinforcement for *S-R* contiguity offers no empirical advantage to compensate for the loss of parsimony entailed in the assumption that reward is necessary for the conection of *S* and *R*. Of the three combinations, furthermore, Morgan's also is the most promising with regard to the simplicity of the underlying mechanism. The two contiguity principles reduce to the general law that the contiguous activation of brain centers tends to establish a connection between them.

It would be a mistake, nevertheless, to suppose that there has been any substantial recent growth of interest in the potentialities of dual-process (*S-S, S-R*) contiguity theory. The bias against the *S-S* principle still is too strong. With the inadequacies of the *S-R* principles becoming increasingly apparent, there has been instead a tendency to turn away entirely from the broad questions about basic processes which have for so long given direction and purpose to research on learning. One of the more vocal followers of Hull and Spence even has gone so far as to conclude that questions about basic processes are, after all, meaningless—that answers to such questions cannot be evaluated as "right or wrong," but are to be treated only as expressions of "personal taste" (Kendler, 1952, p. 276). While little respect has been shown for this odd view, delightfully parodied by Ritchie (1953), the feeling certainly is widespread that the debate over basic processes might well be suspended for a time pending a more detailed study of some of the major variables which affect performance in learning situations.

The most influential advocate of a nontheoretical psychology of learning has been B. F. Skinner, who argues that theories lead to much "useless" experimentation; when the theories eventually are "overthrown," he says, a great deal of the research associated with them is "discarded." This waste would be justified if it were true, as is often assumed, that productive research requires theory —that research without theory is of necessity aimless and disorganized—but Skinner thinks otherwise. It is much more likely, he suggests, that the most rapid progress toward an understanding of learning will be made by research that is designed, not to test theories, but simply "to obtain data showing orderly changes characteristic of the learning process" and to map the relation of those changes to variables "selected for study through a common-sense exploration of the field" (1950, p. 215). When "order and consistency" appear in behavior, Skinner asserts, we are less likely to be concerned with causes. "A datum emerges which takes the place of theoretical phantasy" (1959, p. 375).

Skinner does not give us any examples of the "useless" research to which theories of learning have led. It is difficult to understand, in fact, how that adjective can be applied to research informative enough to convince us of the incorrectness of some conception of the learning process which once seemed reasonable. One might argue, on the contrary, that research is useful only to the extent that it does permit us to improve upon our conceptions of the learning process. Unfortunately, the conceptions which guide the "common-sense exploration" advocated by Skinner are so informal that they are immune to critical scrutiny and unlikely therefore to undergo much refinement in the course of research. Skinner's assertion that theories "lose their point" in the face of order and consistency in behavior also is difficult to accept. One might argue, on the contrary, that order is much more likely than disorder to stimulate inquiry into cause.

Whatever the validity of these arguments, the task of mapping functional relationships in learning has been facilitated enormously by the technological contributions of Skinner. As a graduate student at Harvard in the thirties, Skinner (who seemingly had not yet heard of Thorndike) independently developed an apparatus in which pressing a lever was rewarded with food (Skinner, 1959). Instead, however, of admitting the animal to a feeding area when the correct response was made and then returning it by hand to the response compartment (as was the usual procedure), Skinner caused the response to activate a magnetic feeder which delivered a pellet of food to the response compartment; the necessity of handling the animal between trials thus was eliminated, and the stage was set for automated experiments.

With the animal rewarded in the response compartment, it was ready for a new trial as soon as the reward was consumed, and Skinner permitted it to set its own pace. His measure of performance was not latency of response in discrete trials but rate of response to a continuously available lever, which he wrote out on a cumulative recorder. Skinner claims that the rate of response under these conditions is a far more reliable measure than the latency or the probability of response under discrete-trials conditions, but there is no evidence to support this claim. Certainly his blunt assertion that *T*-maze and jumping-stand scores "hop about from trial to trial almost capriciously" (1959, p. 375) can reflect only his lack of experience with those situations. What is unquestionable, however, is that Skinner has shown the way to an enormous saving of effort in all learning experiments, his techniques of automation being applicable to discrete-trials procedures as well as to the continuous procedures which he favors (Bitterman, 1966). To give ten rats ten trials each in a runway, for example, is an extremely demanding task, but it is no trouble at all to collect the same amount of data in a problem box equipped with a retractable lever, a pellet dispenser, and a printing counter activated by a pulse generator which provides a trial-by-trial record of latency. The way now is clear to the mapping of functional relationships on a previously unheard-of scale.

A striking manifestation of the new spirit of empiricism is to be found in a revival of interest in the role of species (Bitterman, 1960, 1965). Although many different animals were brought into the laboratory in the first years of the century, attention soon became concentrated on a small number of mammalian forms which were treated as representative of animals in general—primarily, of course, the cheap and docile rat. Behind this narrowing of the scope of research lay the growing conviction that the laws of learning were the same everywhere in the animal series. Working first with chicks, then with cats and dogs, and finally with monkeys, Thorndike himself came early to this conclusion. However much animals might differ in the particular connections formed (a reflection of their particular sensory, motor, and motivational properties) or in the efficiency of connection, all could be regarded as "systems of connections" subject to modification according to identical laws—the laws of exercise and effect (1911, pp. 280-281). Thorndike's hypothesis encountered considerable skepticism at first, but skepticism gave way to belief as experiments with a variety of animals began to turn up functional similarities far more impressive than differences. Before long, there was substantial disagreement only as to the nature of the laws that were presumed to hold over the entire animal series, and the choice of subjects for experiments designed to resolve those disagreements was dictated exclusively by considerations of custom and convenience. In recent years, characterized above all by the reopening of closed questions, the phyletic base of research on learning has been broadened once more, and already evident are the first faint glimmerings of some orderly patterns of functional similarity and difference which promise to contribute much to our understanding of the evolution of intelligence.

REFERENCES

BEHREND, E. R., AND BITTERMAN, M. E. Probability-matching in the fish. *Amer. J. Psychol.*, 1961, **74**, 542-551.

BITTERMAN, M. E. Spence on the problem of patterning. *Psychol. Rev.*, 1953, **60**, 123-126.

———. Review of Spence's *Behavior Theory and Conditioning. Amer. J. Psychol.*, 1957, **70**, 141-145.

———. Toward a comparative psychology of learning. *Amer. Psychologist*, 1960, **15**, 704-712.

———. Techniques for the study of learning in animals: analysis and classification. *Psychol. Bull.*, 1962, **59**, 81-93.

———. Phyletic differences in learning. *Amer. Psychologist*, 1965, **20**, 396-410.

———. Learning in animals. In J. Sidowski, *Experimental Methods and Instrumentation in Psychology.* New York: McGraw-Hill Book Company, Inc., 1966.

———, CALVIN, A. D., AND ELAM, C. B. Perceptual differentiation in the course of nondifferential reinforcement. *J. comp. physiol. Psychol.*, 1953, **46**, 393-397.

———, AND COATE, W. B. Some new experiments on the nature of discrimination learning in the rat. *J. comp. physiol. Psychol.*, 1950, **43**, 198-210.

———, REED, P., AND KRAUSKOPF, J. The effect of the duration of the unconditioned stimulus upon conditioning and extinction. *Amer. J. Psychol.*, 1952, **65**, 256-262.

———, WODINSKY, J., AND CANDLAND, D. K. Some comparative psychology. *Amer. J. Psychol.*, 1958, **71**, 94-110.

BLODGETT, H. C. The effect of the introduction of reward upon the maze performance of rats. *Univ. Calif. Publ. Psychol.*, 1929, **4**, 113-134.

BROGDEN, W. J. Sensory pre-conditioning. *J. exp. Psychol.*, 1939, **25**, 323-332.

BROWN, W. The positive effect of punishment. *J. comp. Psychol.*, 1939, **28**, 17-22.

BULLOCK, D. H., AND BITTERMAN, M. E. Probability-matching in the pigeon. *Amer. J. Psychol.*, 1962, **75**, 634-639.

BUTLER, R. A. Discrimination learning by rhesus monkeys to auditory incentives. *J. comp. physiol. Psychol.*, 1957, **50**, 239-241.

———, AND HARLOW, H. F. Persistence of visual exploration in monkeys. *J. comp. physiol. Psychol.*, 1954, **47**, 258-263.

CARR, H. A. The distribution and elimination of errors in the maze. *J. Anim. Behav.*, 1917, **7**, 145-159.

COLE, L. W. Concerning the intelligence of raccoons. *J. comp. Neurol. Psychol.*, 1907, **17**, 211-261.

CRESPI, L. P. Quantitative variation of incentive and performance in the white rat. *Amer. J. Psychol.*, 1942, **55**, 467-517.

DARWIN, C. *The Descent of Man and Selection in Relation to Sex.* Vol. I. New York: D. Appleton & Co., Inc., 1871.

DAVIS, H. B. The raccoon: A study in animal intelligence. *Amer. J. Psychol.*, 1907, **18**, 447-489.

ELLIOTT, M. H. The effect of change of reward on the maze performance of rats. *Univ. Calif. Publ. Psychol.*, 1928, **4**, 19-30.

———. The effect of change of "drive" on maze performance. *Univ. Calif. Publ. Psychol.*, 1929, **4**, 185-188.

ESTES, W. K. Toward a statistical theory of learning. *Psychol. Rev.*, 1950, **57**, 94-107.

———. The statistical approach to learning theory. In S. Koch, ed. *Psychology: A Study of a Science*, Vol. 2. New York: McGraw-Hill Book Company, Inc., 1959.

FINCH, G. Salivary conditioning in atropinized dogs. *Amer. J. Physiol.*, 1938, **124**, 136-141.

———, AND CULLER, E. Higher order conditioning with constant motivation. *Amer. J. Psychol.*, 1934, **46**, 596-602.

FROLOV, Y. P. *Pavlov and His School: The Theory of Conditioned Reflexes.* (Translated by C. P. Dutt.) New York: Oxford University Press, 1937.

GONZALEZ, R. C., AND DIAMOND, L. A test of Spence's theory of incentive-motivation. *Amer. J. Psychol.*, 1960, **73**, 396-403.

———, GENTRY, G. V., AND BITTERMAN, M. E. Relational discrimination of intermediate size in the chimpanzee. *J. comp. physiol. Psychol.*, 1954, **47**, 385-388.

———, ROBERTS, W. A., AND BITTERMAN, M. E. Learning in adult rats with extensive cortical lesions made in infancy. *Amer. J. Psychol.*, 1964, **77**, 547-562.

GOODNOW, J. J. Determinants of choice-distribution in two-choice situations. *Amer. J. Psychol.*, 1955, **68**, 106-116.

GRAF, V., BULLOCK, D. H., AND BITTERMAN, M. E. Further experiments on probability-matching in the pigeon. *J. exp. Anal. Behav.*, 1964, **7**, 151-157.

GRICE, G. R. An experimental test of the expectational theory of learning. *J. comp. physiol. Psychol.*, 1948, **41**, 137-143.

GRINDLEY, G. C. Experiments on the influence of the amount of reward on learning in young chickens. *Brit. J. Psychol.*, 1929, **20**, 173-180.

GUTHRIE, E. R. Conditioning as a principle of learning. *Psychol. Rev.*, 1930, **37**, 412-428.

———. *The Psychology of Learning.* New York: Harper & Brothers, Publishers, 1935.

HAGGERTY, M. E. Imitation in monkeys. *J. comp. Neurol. Psychol.*, 1909, **19**, 337-345.

HARLOW, H. F. Experimental analysis of the role of the original stimulus in conditioned responses in monkeys. *Psychol. Rec.*, 1937, **1**, 62-68.

HICKSON, R. H. Response probability in a two-choice learning situation with varying probability of reinforcement. *J. exp. Psychol.*, 1961, **62**, 138-144.

HILGARD, E. R., AND MARQUIS, D. G. *Conditioning and Learning.* New York: D. Appleton-Century Company, Inc., 1940.

HOBHOUSE, L. T. *Mind in Evolution.* London: Macmillan and Company, Ltd., 1901.

HOLMES, S. J. *Studies in Animal Behavior.* Boston: Richard C. Badger, 1916.

HONZIK, C. H., AND TOLMAN, E. C. The perception of spatial relations by the rat: A type of response not easily explained by conditioning. *J. comp. Psychol.*, 1936, **22**, 287-318.

HUBBERT, H. B., AND LASHLEY, K. S. Retroactive association and the elimination of errors in the maze. *J. Anim. Behav.*, 1917, **7**, 130-142.

HULL, C. L. A functional interpretation of the conditioned reflex. *Psychol. Rev.*, 1929, **36**, 498-511.

———. Mind, mechanism, and adaptive behavior. *Psychol. Rev.*, 1937, **44**, 1-32.

———. *Principles of Behavior.* New York: D. Appleton-Century Company, Inc., 1943.

———. *A Behavior System.* New Haven: Yale University Press, 1952.

HUNTER, W. S. A note on the behavior of the white rat. *J. Anim. Behav.*, 1912, **2**, 137-141.

———. The delayed reaction in animals and children. *Behav. Monogr.*, 1919, **2**, 1-86.

———. Conditioning and extinction in the rat. *Brit. J. Psychol.*, 1935, **26**, 135-148.

JOHNSON, E. E. The role of motivational strength in latent learning. *J. comp. physiol. Psychol.*, 1952, **45**, 526-530.

KENDLER, H. H. "What is learned?"—a theoretical blind alley. *Psychol. Rev.*, 1952, **59**, 269-277.

KINNAMAN, A. J. Mental life of two *Macacus rhesus* monkeys in captivity. *Amer. J. Psychol.*, 1902, **13**, 98-148, 173-218.

KÖHLER, W. *The Mentality of Apes* (translated by E. Winter). New York: Harcourt, Brace and Company, Inc., 1925.

KONORSKI, J. Mechanisms of learning. In *Physiological Mechanisms in Animal Behavior.* Fourth Symposium of the Society for Experimental Biology, New York: Academic Press, Inc., 1950, 408-431.

KRECHEVSKY, I. "Hypotheses" versus "chance" in the pre-solution period in sensory discrimination-learning. *Univ. Calif. Publ. Psychol.*, 1932, **6**, 27-44.

LASHLEY, K. S. Studies of cerebral functions in learning. IV. The theory that synaptic resistance is reduced by the passage of the nerve impulse. *Psychol. Rev.*, 1924, **31**, 369-375.

———. The mechanism of vision: XV. Preliminary studies of the rat's capacity for detail vision. *J. gen. Psychol.*, 1938, **18**, 123-193.

———. An examination of the "continuity theory" as applied to discriminative learning. *J. gen. Psychol.*, 1942, **26**, 241-265.

LAWRENCE, D. H. The transfer of a discrimination along a continuum. *J. comp. physiol. Psychol.*, 1952, **45**, 511-516.

LONGO, N. Probability-learning and habit reversal in the cockroach. *Amer. J. Psychol.*, 1964, **77**, 29-41.

MACCORQUODALE, K., AND MEEHL, P. E. Edward C. Tolman. In W. K. Estes et al., *Modern Learning Theory.* New York: Appleton-Century-Crofts, Inc., 1954.

MAIER, N. R. F. The effect of cerebral destruction on reasoning and learning in rats. *J. comp. Neurol.*, 1932*a*, **54**, 45-75.

———. Cortical destruction of the posterior part of the brain and its effect on reasoning in rats. *J. comp. Neurol.*, 1932*b*, **56**, 179-208.

———. Age and intelligence in rats. *J. comp. Psychol.*, 1932*c*, **13**, 1-6.

———, AND SCHNEIRLA, T. C. Mechanisms in conditioning. *Psychol. Rev.*, 1942, **49**, 117-134.

MARX, M. H., HENDERSON, R. L., AND ROBERTS, C. L. Positive reinforcement of the bar-pressing response by a light stimulus following dark operant pretests with no aftereffect. *J. comp. physiol. Psychol.*, 1955, **48**, 73-76.

MCALLISTER, W. R. The spatial relation of irrelevant and relevant goal-objects as a factor in simple selective learning. *J. comp. physiol. Psychol.*, 1952, **45**, 531-537.

MILLER, N. E. Comments on multiple-process conceptions of learning. *Psychol. Rev.*, 1951, **58**, 375-381.

———. A new alternative to drive reduction. In M. R. Jones, *Nebraska Symposium on Motivation.* Lincoln: Univ. of Nebraska Press, 1963.

Morgan, C. L. *An Introduction to Comparative Psychology*. London: Walter Scott, 1894.

Morse, E. B., and Runquist, W. N. Probability-matching with an unscheduled random sequence. *Amer. J. Psychol.*, 1960, **73**, 603-607.

Mowrer, O. H. A stimulus-response analysis of anxiety and its role as a reinforcing agent. *Psychol. Rev.*, 1939, **46**, 553-565.

———. On the dual nature of learning: A re-interpretation of "conditioning" and "problem-solving." *Harv. educ. Rev.*, 1947, **17**, 102-148.

———. Two-factor learning theory reconsidered, with special reference to secondary reinforcement and the concept of habit. *Psychol. Rev.*, 1956, **63**, 114-128.

———. *Learning Theory and Behavior*. New York: John Wiley & Sons, Inc., 1960*a*.

———. *Learning Theory and the Symbolic Processes*. New York: John Wiley & Sons, Inc., 1960*b*.

———, and Lamoreaux, R. R. Avoidance conditioning and signal duration: A study of secondary motivation and reward. *Psychol. Monogr.*, 1942, **54**, 1-34.

———, and Solomon, L. N. Contiguity vs. drive-reduction in conditioned fear: The proximity and abruptness of drive-reduction. *Amer. J. Psychol.* 1954, **67**, 15-25.

Muenzinger, K. K. Vicarious trial and error at a point of choice. I. A general survey of its relation to learning efficiency. *J. genet. Psychol.*, 1938, **53**, 75-86.

Munn, N. L. *Handbook of Psychological Research on the Rat*. Boston: Houghton Mifflin Company, 1950.

Pavlov, I. P. *Conditioned Reflexes: An Investigation of the Physiological Activity of the Cerebral Cortex* (translated by G. V. Anrep). London: Oxford University Press, 1927.

Porter, J. P. Intelligence and imitation in birds. *Amer. J. Psychol.*, 1910, **21**, 1-71.

Prokofiev, G., and Zeliony, G. Des modes d'associations cérébrales chez l'homme et chez les animaux. *J. de Psychol.*, 1926, **23**, 1020-1028.

Richardson, F. A study of sensory control in the rat. *Psychol. Monogr.*, 1909, **12**, 1-24.

Ritchie, B. F. The circumnavigation of cognition. *Psychol. Rev.*, 1953, **60**, 216-221.

Roberts, W. H. The effect of delayed feeding on white rats in a problem cage. *J. genet. Psychol.*, 1930, **37**, 35-58.

Romanes, G. J. *Animal Intelligence*. New York: D. Appleton & Company, Inc., 1881.

Saldanha, E. L., and Bitterman, M. E. Relational learning in the rat. *Amer. J. Psychol.*, 1951, **64**, 37-53.

Schlosberg, H. The relationship between success and the laws of conditioning. *Psychol. Rev.*, 1937, **44**, 379-394.

Seward, J. P. An experimental analysis of latent learning. *J. exp. Psychol.*, 1949, **39**, 177-186.

Sheffield, F. D. Relation between classical conditioning and instrumental learning. In W. F. Prokasy, *Classical Conditioning: A Symposium*. New York: Appleton-Century-Crofts, 1965.

Shepherd, W. T. Some mental processes of the rhesus monkey. *Psychol. Monogr.*, 1910, **12**, 1-61.

Silver, C. A., and Meyer, D. R. Temporal factors in sensory preconditioning. *J. comp. physiol. Psychol.*, 1954, **47**, 57-59.

Skinner, B. F. Two types of conditioned reflex and a pseudo type. *J. gen. Psychol.*, 1935, **12**, 66-76.

———. Are theories of learning necessary? *Psychol. Rev.*, 1950, **57**, 193-216.

———. A case history in scientific method. In S. Koch, ed., *Psychology: A Study of a Science*, Vol. 2. New York: McGraw-Hill Book Company, Inc., 1959.

Smith, S. The limits of educability in Paramecium. *J. comp. Neurol. Psychol.*, 1908, **18**, 499-510.

Spence, K. W. The nature of discrimination learning in animals. *Psychol. Rev.*, 1936, **43**, 427-449.

———. The differential response in animals to stimuli varying within a single dimension. *Psychol. Rev.*, 1937, **44**, 430-444.

———. The basis of solution by chimpanzees of the intermediate size problem. *J. exp. Psychol.*, 1942, **31**, 257-271.

———. The nature of response in discrimination learning. *Psychol. Rev.*, 1952, **59**, 89-93.

———. *Behavior Theory and Conditioning*. New Haven: Yale University Press, 1956.

———, Bergmann, G., and Lippitt, R. A study of simple learning under irrelevant motivational-reward conditions. *J. exp. Psychol.*, 1950, **40**, 539-551.

———, and Lippitt, R. An experimental test of the sign-gestalt theory of learning. *J. exp. Psychol.*, 1946, **36**, 491-502.

Stein, L. The classical conditioning of the consummatory response as a determinant of instrumental performance. *J. comp. physiol. Psychol.*, 1957, **50**, 269-287.

Strange, J. R. Latent learning under conditions of high motivation. *J. comp. physiol. Psychol.*, 1950, **43**, 194-197.

Teas, D. C., and Bitterman, M. E. Perceptual organization in the rat. *Psychol. Rev.*, 1952, **59**, 130-140.

THISTLETHWAITE, D. Conditions of irrelevant-incentive learning. *J. comp. physiol. Psychol.*, 1952, **45**, 517-525.

THORNDIKE, E. L. Animal intelligence: An experimental study of the associative processes in animals. *Psychol. Rev. Monogr. Suppl.*, 1898, **2**, 1-109.

———. The mental life of monkeys. *Psychol. Rev. Monogr. Suppl.*, 1901, **3**, 1-57.

———. *Animal Intelligence: Experimental Studies.* New York: The Macmillan Co., 1911.

———. *Human Learning.* New York: Century Company, 1931.

TOLMAN, E. C. Purposive behavior in animals and men. New York: Century Company, 1932.

———. Theories of learning. In F. A. Moss, ed., *Comparative Psychology.* Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1934, 367-408.

———. The determiners of behavior at a choice point. *Psychol. Rev.*, 1938, **45**, 1-41.

———. Cognitive maps in rats and men. *Psychol. Rev.*, 1948, **55**, 189-208.

———. Principles of purposive behavior. In S. Koch, ed., *Psychology: A Study of a Science*, Vol. 2. New York: McGraw-Hill Book Company, Inc., 1959.

———, AND HONZIK, C. H. Introduction and removal of reward, and maze performance in rats. *Univ. Calif. Publ. Psychol.*, 1930, **4**, 257-275.

TURBEVILLE, J. R., CALVIN, A. D., AND BITTERMAN, M. E. Relational and configurational learning in the rat. *Amer. J. Psychol.*, 1952, **65**, 424-433.

ULRICH, J. L. Distribution of effort in learning in the white rat. *Behav. Monogr.*, 1915, **2**, 1-51.

VOEKS, V. W. Postremity, recency, and frequency as bases for prediction in the maze situation. *J. exp. Psychol.*, 1948, **38**, 495-509.

———. Formalization and clarification of a theory of learning. *J. Psychol.*, 1950, **30**, 341-362.

WASHBURN, M. F., AND ABBOTT, E. Experiments on the brightness value of red for the light-adapted eye of the rabbit. *J. Anim. Behav.*, 1912, **2**, 145-180.

WATSON, J. B. *Animal Education.* Chicago: University of Chicago Press, 1903.

———. Imitation in monkeys. *Psychol. Bull.*, 1908, **5**, 169-178.

———. *Behavior: An Introduction to Comparative Psychology.* New York: Henry Holt and Company, Inc., 1914.

———. The place of the conditioned reflex in psychology. *Psychol. Rev.*, 1916, **23**, 89-116.

———. The effect of delayed feeding upon learning. *Psychobiol.*, 1917, **1**, 51-60.

WEINSTOCK, S. Acquisition and extinction of a partially reinforced running response at a 24-hr. intertrial interval. *J. exp. Psychol.*, 1958, **56**, 151-158.

WILSON, W. A., Jr., OSCAR, M., AND BITTERMAN, M. E. Probability-learning in the monkey. *Quart. J. exp. Psychol.*, 1964, **16**, 163-165.

WOLFE, J. B., AND SPRAGG, S. D. S. Some experimental tests of 'reasoning' in white rats. *J. comp. Psychol.*, 1934, **18**, 455-469.

YERKES, R. M. The mental life of monkeys and apes: A study of ideational behavior. *Behav. Monogr.*, 1916, **3**, 1-145.

———. The mind of a gorilla. *Genet. Psychol. Monogr.*, 1927, **2**, 1-193.

CHAPTER 5

Verbal Learning

GORDON H. BOWER[1]
Stanford University

Science is an intellectual discipline. That is to say, it is an enterprise which creates its own questions and problems, devises methods for attacking those problems, and along the way discovers yet more new questions and problems. The information gathered around an intellectual question is relevant and interesting only because of the question itself; pragmatic criteria regarding the utility of the information for other purposes (e.g., for constructing better teaching-machine programs) are secondary, indeed if they are considered at all. This is a fact about the sociology and ethics of science. Only by understanding the ethos and mores of an intellectual scientific discipline can one understand the diverse and myriad branchings, peculiar twists, turns, and intricate interweavings, the fads, fancies, and foibles in the history of experimental studies of verbal learning. Although the title of the research area suggests it, most of the experimentation is not, in fact, concerned with the language learning of humans; otherwise, there would be more studies of language development in children or second-language learning in adults. Instead, the study of language functioning in the human has been left more to the province of other research areas, e.g., psycholinguistics. Since its inception, the research in verbal learning has held more kinship with the areas of conditioning or learning theory than with linguistics or child development. That is, the emphasis in verbal learning research has been more on the learning than on the verbal part of that descriptive phrase.

The founder of this branch of psychology was Ebbinghaus, dating from the publication of his treatise *Über das Gedächtnis* (Concerning Memory) in 1885. How does one found a new branch of science? By asking new questions and devising new methods for gathering reliable information to answer those questions. Because good questions tend to beget more and then more, the character of the developing science is molded to a considerable extent by the initial questions asked and the initial methods used to answer them. The influence of Ebbinghaus on the developing science of human verbal learning has been profound and pervasive (some would say devastating). Ebbinghaus created a new experimental situation in which a multitude of variables can and do act in concert in determining the behavior observed. One major job of the continuing research has been to tease out these variables, count them, measure them, determine their functional laws to the behavior of interest; and then modify the situation slightly and proceed to do the whole job over again. In this way, a tremendous empirical backlog of information is accumulated about how human subjects learn verbal materials in experimental situations resembling the type created initially by Ebbinghaus.

---

[1] The material reviewed herein extends to 1962, with updating in 1963. References with later publication dates were read in preprints. The author's research reported here was supported by grants M-3849 from NIMH and HD-00954 from NICHHD.

As the empirical backlog accumulates around particular questions, local hypotheses take form which attempt to integrate the information into a reasonable schema, with a parsimony of terms and concepts, and possibly to lead to predictions about the outcome of experiments yet to be done. Given the doubting stance and ingenuity of most scientists, the act of making a hypothesis automatically begets at least one or two alternative hypotheses, which leads to more experimentation, and so on and on it goes. It has been claimed, by some of its critics, that research in verbal learning has been crassly empirical, to the point of being atheoretical. The plethora of local hypotheses to explain close-knit facts shows otherwise, if one simply counts noses of hypotheses. What the critic usually means is that investigators of verbal learning do not interpret their results in terms of theoretical concepts which the critic cherishes as being of global significance to interpretations of learning and behavior. The theoretical heritage of research in human learning has been the functionalism of Carr and Robinson (cf. Boring, 1950) and the analytic framework of stimulus-response associationism. But beyond this general framework, the structure and concepts of local hypotheses vary, as they should, depending upon the local requirements imposed by the particular facts to be explained. There have been few if any attempts to give a precise yet comprehensive theoretical integration of the facts from the whole area. The lack of global theories in verbal learning may be a blessing in disguise, because they tend to be more vague and/or contentious than useful for effecting orderliness in specific detail of data. For this latter job, local hypotheses seem called for and local hypotheses abound in the verbal learning field. There is a common core of concepts used throughout the area (e.g., similarity of units, meaningfulness, associative interference) but these appear to stem rather directly from the common stimulus-response-association framework within which nearly all the investigators interpret their findings.

The development of a science is like the expanding tail of a peacock, fanning out wider and wider in a number of directions. A given time slice taken across this development can count a number of distinct experimental situations or categories, each being studied more or less intensively, each having its own associated cluster of local hypotheses and experimental findings which attempt to answer the corresponding intellectual questions. The accelerated development and branching process of present-day research in verbal learning is enormous, enough so that a thorough-going review of all the material is a job beyond the capacity of sane men. The present review of this material is selective and incomplete, as are all reviews. For example, there is no coverage of the extensive literature on transfer of training and distribution of practice (cf. McGeogh and Irion, 1952). The general level and brevity of the review also enforces curtailment in coverage of the subtle details in experimental design and theoretical rhetoric so dear to the heart of the practicing researcher. The customary classification of problem areas will be followed, each area having its associated experimental techniques. The classifications to be covered in this chapter include studies of immediate memory, free recall, paired-associate learning, and serial learning; then a section on studies of forgetting, followed by a terminal section on reinforcement factors in human learning.

SHORT-TERM MEMORY

Experiments on immediate memory typically involve a single presentation of a small amount of information which the subject attempts to recall immediately. The classical experiments used sequences of decimal digits as the learning materials. Adult subjects can immediately recall seven or eight digits about 50 per cent of the time. This "digit span" is less for children, and it increases with age, which explains why it is incorporated in several IQ test batteries. In the digit-span test, the subject must recall the digits in the exact order in which they were presented. As might be expected, about half of the errors in recall are due to faulty ordering of the digits (Conrad, 1959).

Although untutored subjects have a digit span of around seven, the apparent span can be increased through recoding devices. That is, via some dictionary, the original digits are encoded into a smaller number of information units by the learner (Miller, 1956). For example, if for binary digits the subject has established the dictionary A = 101, B = 001, etc., then the digit string 001101 would be encoded as BA at the time of learning. Recall of BA, of course, enables the subject to translate back into digit strings via the dictionary. The apparent memory span that can be established in this way would seem to be limited only by the size and number of dictionary entries that a person can manage simultaneously.

One can make a conceptual distinction between the fact of storage of material and the ability to retrieve all this material in recall. Retrieval in recall takes time and possibly material yet to be recalled suffers further loss while prior material is being recalled. Thus, the digit span may give a spuriously low estimate of the amount of information in storage at a particular time. Sampling or cueing procedures for selective recall of portions of an entire message have yielded higher estimates of storage in Sperling's experiment (1960). Another method for measuring storage without involving retrieval is the missing digit span devised by Buschke (1962). In this test, the person listens to *N*-1 items in random order selected from a known set of *N* items, and then he indicates the one item which was not presented. In line with Sperling's results, Buschke found much higher estimates of retention with this procedure than with the alternate digit-span recall which measures both storage and retrieval.

A time-honored account of immediate recall supposes that the material presented activates a perseverating stimulus trace and, at recall, the person reads off the information from this internal image; hence, the appellation "memory trace." The trace fades and weakens in time, and it can be disrupted by intervening events. There is no doubt that short-term stimulus traces exist (Sperling, 1960; Mackworth, 1962). What is doubtful is that such stimulus traces per se have much relevance to recall of materials after intervals of more than one or two seconds (Sperling, 1960). At longer recall intervals, the subject apparently relies mainly upon remembering his implicit (verbal) encoding of the stimulus display (Haber, 1963; Harris and Haber, 1963). Encoding is the operation by which the subject constructs some internal representation of a stimulus at the time he sees it (Lawrence, 1963). In recall, he tends to recall only items that were encoded at the time of presentation.

A prominent line of recent research on immediate memory follows procedures devised by Brown (1958) and Peterson and Peterson (1959). Good reviews of this material are in Peterson (1963) and Melton (1963). In Peterson's studies, single verbal items are presented at a slow pace (e.g., two seconds each); the subject reads the item aloud and is tested for recall after a delay of several seconds. To prevent rehearsal, during the delay interval the subject is required to count backwards rapidly by threes from some random number provided to him. With this procedure, forgetting is considerable over only a few seconds. Results of one experiment with three-letter consonant syllables are shown in Fig. 5-1.

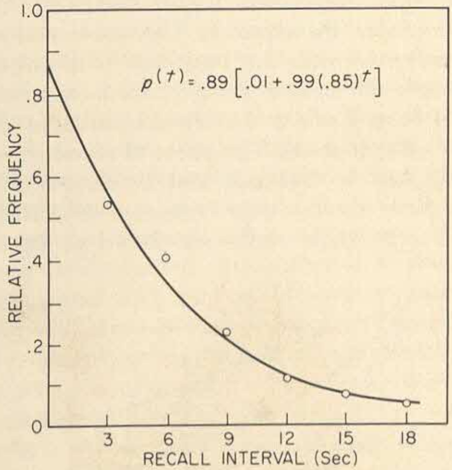


Fig. 5-1. Percentage correct recalls with latencies below 2.83 sec as a function of recall interval. (Peterson and Peterson, 1959)

Suppose that the evocation of the subject's verbal response (by the material presented) is viewed as a trial of learning. Then one can distinguish certain antecedent stimulus events to which the verbal response is conditioned. One class of events are specific cues introduced by the experimenter as part of the materials. A strengthening of the conditional relationship between these cues and responses is called "cue learning" by Peterson (1963). Increasing dependencies between pairs of words in "paired-associate" learning is one example. A second example occurs with verbal items of very low initial integration (e.g., CHQ); cue learning would refer to strengthening of the interassociations between the letter units of such compound responses.

A second class of antecedent events identified by Peterson consists of background contextual cues associated with the subject's situation. These may include, for example, internal cues from physiological processes, from postural adjustments and attitudes of the subject, from the experimental apparatus and physical surroundings of the subject, and so forth. Many of these cues are beyond the control of the experimenter. Strengthening of conditional relationships between these unspecified events and responses is called "background conditioning."

Each repetition of the verbal response can be viewed as producing an increment in background conditioning, and in cue learning to the extent that it is involved. The asympote of the forgetting curve thus should be higher following more repetitions of the response. This prediction may be contrasted with the view that rehearsal only postpones the onset of decay of the memory trace but does not affect the characteristics of such decay once it starts. The effect of repetition was investigated by Peterson and Peterson (1960), and their results were extended by Hellyer (1962). Hellyer's results are shown in Fig. 5-2 illustrating the forgetting curves obtained following 1, 2, 4, and 8 repetitions of a consonant trigram before beginning the retention interval. The asymptote of the forgetting curve increases directly with number of repetitions.

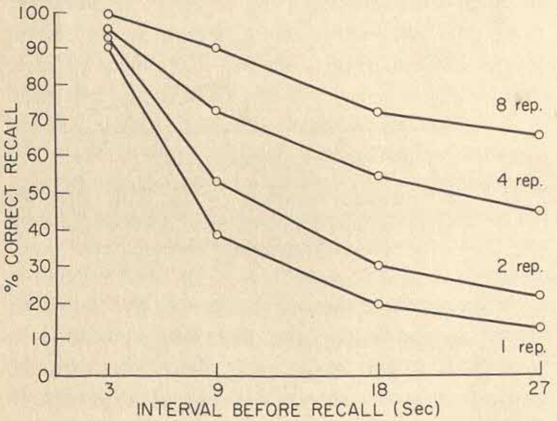


Fig. 5-2. Percentage of correct recall as a function of the number of repetitions of a trigram before beginning the retention interval. (Hellyer, 1962)

The amount of cue learning required by a verbal unit will depend upon the number of subunits (letters) involved and the strength of preestablished interletter dependencies between the subunits. The effect of size of unit is shown in Fig. 5-3, obtained in an experiment by Melton, Crowder, and Wulff (in Melton, 1963). The units, either 1, 2, 3, 4, or 5 consonants in length, were presented once before the subject counted backwards by threes over the retention intervals shown in Fig. 5-3. Despite perfect immediate retention for the various-sized units, subsequent forgetting proceeded faster the greater was the amount of input information.

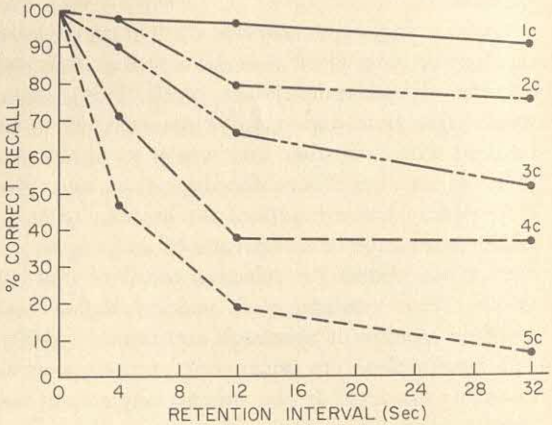


Fig. 5-3. Percentage of completely correct recall as a function of the number of consonants in the presented unit. (Melton, 1963)

The effect on recall of preestablished associations among the subunits of an item has been shown by Peterson, Peterson, and Miller (1961) and by Murdock (1961*b*). Three-letter words are better recalled than three-letter nonsense syllables. The difference was due to the poorer integration of the syllables: subjects could often recall the first letter but not the remaining two. In fact, recall of first letters was equal for syllables and words. Because of prior interletter dependencies with the words, recall of the first letter was usually followed by recall of the remainder of the word. In brief, the syllable contained three chunks of information (Miller, 1957) whereas the word involved only one. When chunks are equated for sense and nonsense, as by comparing recall of three words with recall of a three-letter nonsense syllable, retention curves are similar (Murdock, 1961*b*).

Background conditioning would lead to interference when two verbal responses are learned, even though specific cues for them may be absent or differential. In one experiment (Peterson and Peterson, 1962), subjects studied two word-word pairs in succession; in each pair, one word was the instructed cue to which the other word was to be associated. After a delay interval of counting backwards, the cue word of one of the pairs was presented and the subject tried to give the associated response word. Percentage correct recall on one of two presented pairs was consistently poorer than recall of a singly presented pair. The poorer performance was due in large part to errors of giving the uncalled-for response of the untested cue word. Such "confusions" could result only from background conditioning since the cue words of the two pairs were dissimiliar in all respects. As would be expected also, the more different responses conditioned to the background cues, the poorer the recall of any particular one. Murdock (1961*a*) showed this by varying the number of paired-associate items presented before and after a critical item that was later tested for recall. He found a decline in recall of the critical item as the number of prior and/or succeeding items increased.

If one assumes that background stimuli fluctuate over time in their effects on behavior, then an account of forgetting is obtained. Assume a large population of background stimuli partitioned at any moment into exclusive subsets which are (S) or are not (S') active, available and effective on the subject's performance. The verbal response is conditioned only to that subset of stimuli that are available and effective at the time the response is evoked by the presentation. Assume that over time the elements fluctuate randomly between S and S′, like the diffusion of dye molecules between fluid compartments separated by a permeable membrane. At a recall test the available set of elements will be composed of some old conditioned elements and some new unconditioned elements, the extent of change varying exponentially over time since the initial presentation. The probability of the response on a test equals the proportion of available elements that are conditioned to it. From these assumptions, Estes (1955) derived the following equation to describe the time course of change in response probability. In Eq. 1, p_o and p_o' are the proportions of the elements in S and S′ that are conditioned by the time the retention interval begins, $p(t)$ is the probability of response after a delay interval of length t, J is the fraction of elements in the population that are in the available set, S, and j and j' are diffusion probabilities characterizing the fluctuation of elements between S and S′ in each small unit of time. In the study by Peterson and Peterson (1959), a consonant syllable was presented only once, and hence p_o' would be assumed to be zero. The equation fit to the results in Fig. 5-1 is $p(t) = .89\,[.01 + .99(.85)^t]$ in which t is in seconds, p_o is .89, J is .01 and $j + j'$ is .15.

$$p(t) = p_o[J + (1 - J)(1 - j - j')^t] + p_o'(1 - J)[1 - (1 - j - j')^t] \quad (1)$$

As mentioned earlier, the model predicts and one finds that repetition of the verbal response increases retention because p_o' is larger with repetition. Suppose that we vary the time interval between the first and second evocation of the response, and ask how this interval will affect $p(t)$ in Eq. 1, where t is measured from the end of the second evocation. If the second reinforcement follows immediately upon the first before the set S has changed its constituency of elements, then very few new elements will be conditioned by the second reinforcement. If the second follows the first evocation by a long time, then many new elements will be conditioned by the second evocation. Thus, the longer the interval separating the two reinforcements, the greater the overall percentage of elements conditioned (i.e., $p_oJ + p_o'\,(1 - J)$ is greater). Hence, recall at t seconds after the second presentation should be higher the greater the interval between the two reinforcements. Peterson and Peterson (1960) confirmed this prediction using intervals of 1, 3, 6, or 11 seconds between presentations of the same nonsense syllable, and a 6-second interval between the second presentation and recall. Subjects counted backwards by threes during both intervals. Recall probability increased directly with the time separating the first and second presentation. The result has been repeated using paired-associate word pairs (Peterson, Saltzman, Hillner, and Land, 1962).

In Peterson's studies, recall of an item was measured after an interval during which the subject engaged in an interpolated activity. Murdock (1961*b*) has asked whether the forgetting is primarily a function of the time interval or of the amount of interpolated activity. He varied the presentation rate of interpolated material. Lists of 4, 7, 10, 13, 16, or 19 words were read at rates of 0.5, 1, or 2 words per second. The first word in the list was the critical one. It was always presented for one second; the remaining, interpolated items varied in number and in the time taken to be read to the subject. The subject was required to recall the first and last three words from each list immediately after the reading. Comparing recall of the first word in the various lists, Murdock found that the amount of interpolated material was the critical factor determining its retention. The presentation rate of the interpolated material, of course, determines the total delay interval between presentation of the first item and its test for recall, yet rate per se had little consistent influence on recall probability. These results are inconsistent with a fluctuation process which ascribes the turnover of background stimuli to events that occur at a constant rate in time. The fluctuation itself must be tied to "set-breaking" interpolated activity rather than to the mere passage of time.

RECOGNITION MEASURES OF MEMORY The experiments above used a recall test for retention. Shepard and Teghtsoonian (1961) have used a recognition index. Their subjects saw a long series of numbers, some being duplicates of ones presented earlier, and to each indicated whether it was an old or new number in the series. The probability of correctly recognizing an old number dropped rapidly from 1.00 to about .75 when up to seven items intervened between presentation and test. Beyond seven, the curve dropped more slowly, reaching a value of .57 after 50 intervening numbers. The curve departs from the exponential decay curve of fluctuation theory and Shepard (1961) has provided an alternative model for the process. The recognition probability of .57 reached after 50 interpolated items was still considerably higher than the probability of saying "old" to a new item, which increased to about .25 over the series. The difference in retention measures in this and Peterson's studies (cf. Fig. 5-1) is likely due to the use of the recognition method. Much less information need be learned for recognition than for complete recall. For example, recall usually requires response integration whereas recognition does not. Additionally, the class of response alternatives is restricted in recognition (either yes-no or multiple choice) so that each correct response conveys less information than does the process of recall in which the response is being selected from a large set of possibilities. When the size of the selection set is equated, recall and recognition give quite equivalent outcomes (Davis, Sutherland, and Judd, 1961).

In the Shepard-Teghtsoonian procedure, the probability that the subject says "old" to an old item ("recognizes it") will vary directly with his overall probability of saying "old" even to the novel items. Let $P(O/O)$ and $P(O/N)$ represent the conditional probabilities of the subject saying "old" to old and new items, respectively. Within the context of an experiment on signal detection in psychophysics (cf. Swets, Tanner, and Birdsall, 1961), $P(O/O)$ would be identified as the "hit rate" and $P(O/N)$ as the "false-alarm rate." In signal-detection experiments, these two probabilities are known to convary systematically, tracing out what has been called the "Receiver Operating Characteristic" or ROC curve. Following the original work by Egan (1958), Galanter (Personal communication, 1962) has shown that the same two quantities covary in experiments on recognition probability with verbal items. Subjects were shown a list of L nonsense syllables which they were to learn. On the recognition test, K of these L syllables were randomly distributed throughout a list containing N new syllables. The subject checked for each syllable in the test list whether it was old or new. Before the test the subject was told the quantities K and N, and thus he could establish a certain level of bias in his "old" responses. Using a variety of values of K and N, Galanter found that the quantities $P(O/O)$ and $P(O/N)$ varied together, tracing out an ROC curve of the type found in signal-detection experiments.

In terms of the notions of Signal Detectability Theory (Swets, Tanner, and Birdsall, 1961), we may suppose that each test syllable produces a certain "feeling of familiarity" which varies in a more or less continuous manner, generally being higher for items read in the training list. Since the response required to each test syllable is a dichotomous "old" or "new," the subject must partition the familiarity continuum into a region of acceptance and a region of rejection. This is done by selecting a criterion and calling an item "old" only if its familiarity exceeds the criterion. Given stable distributions on the continuum for old and new items, the selection of the criterion determines $P(O/O)$ and $P(O/N)$. As the criterion is varied, the quantities vary together. In Galanter's experiment, prior information about the composition of the test list (K and N) presumably affected the subject's setting of a criterion. For example, if K is very large relative to N, then most items in the test list are old. Hence, the subject should set his criterion low, so that he reaches the decision "old" when uncertain about an item. The low criterion ensures a high hit rate, $P(O/O)$, but it also ensures a high false-alarm rate, $P(O/N)$, which was the experimental finding. Conversely, if N is large relative to K, the criterion is set at a high value to reject the new items, with a resulting drop in the probability that old items are "recognized."

The application of detection theory to recognition learning seems a fruitful approach which will yield useful data. A number of experimental facts may be interpreted in line with the theory. For example, the average difference (d') between the distributions of old and new items on the familiarity continuum should depend on (*a*) the similarity of the two sets of items (Postman, 1951), and (*b*) the number of presentations and the study time per presentation of the old items prior to test. Differential monetary payoffs for hits, false alarms, etc., would be expected to modify the subject's choice of a criterion. This approach also holds promise of being able to interrelate measures of learning obtained from yes-no and multiple-choice tests. One may question the assumption in this approach that the subject can make fine discriminations (and judgments) along a graded continuum of familiarity. However, Egan (1958) obtained results consistent with this assumption. Similarly, Nachmias and Sternberg (1963) obtained confirmatory results and, at the same time, were able to reject a model which assumed that the subject could make only two or three rough category judgments along the familiarity scale.

FREE VERBAL RECALL In most of the studies reviewed above, the unit of presentation and test is the individual verbal item. New aspects come to light in studies of immediate "free recall," in which a list of words is read and the subject attempts to write down in any order as many words as he can remember. When the words are read in random order, the items near the end of the list are recalled first and most frequently over a group of subjects (Deese and Kaufman, 1957). Such "recency" results would be expected on the basis of background conditioning since the available elements at immediate recall are those conditioned to the immediate preceding verbal responses.

Cue learning factors are major determinants of performance in free recall. If there are preestablished associations between words in the list, the words are recalled in associated clusters. Deese (1959*a*) indexed such preexperimental associations by tabulating the frequency with which subjects responded with another word in the list when asked to free-associate to each item of the list as a stimulus. The average interitem association scores for various lists correlated very highly with the percentage of words recalled after one presentation of the list. When a word was recalled, it appeared to act as a cue for its associates in the list. Rothkopf and Coke (1961) have added quantitative evidence on the increase in probability that a particular word would be recalled as the number of its cue associate words in the list was increased. Deese (1959*b*) has also found that nonlist words imported into the subject's recall could be predicted by the number of recalled words which were cues for the imported word.

A number of investigators (e.g., Miller, 1956; Garner, 1962) have stressed the relation between immediate memory and the information contained in the material to be memorized. Information is viewed in the technical sense of Shannon's mathematical theory (1948). The less information contained in the material the easier it is learned. For some (but not all) materials, the information measure is easily derived and the hypothesis can be tested. Adelson, Muckler, and Williams (1955) tested it by having their subjects recall strings of fifteen alphabetic letters in a fixed order. In one study, the number of different letters (2, 4, 6, or 15) composing the 15-letter sequence was varied. The information measure (uncertainty) for a 15-letter list is greater the more alternatives used and when the alternatives used are equiprobable. Information content can be reduced by introducing sequential dependencies between letters in the list (e.g., *d* always follows *x*). The surprising result was that trials to learn a list was an invariant (linear) function of its amount of uncertainty whether the level of uncertainty was produced by manipulation of number of alternatives, the probability bias in the letters used, or nonrandom sequential dependencies between letters. Bower and Batchelder (unpublished) have replicated the essentials of this finding. Miller (1958) has reported further evidence on the facilitative effect of nonrandom sequential dependencies in letter sequences upon their free recall.

Miller and Selfridge (1950) have applied an informal uncertainty analysis to the recall of lists of English words with varying sequential dependencies. Ordinary language has a statistical structure; the preceding words place some contextual constraints upon what the next word will be in a sensible English sentence. Strings of words of varying approximations to sensible English can be constructed, going from random strings (zero order) to English text. Higher-order strings are less informative in the technical sense of that term; if the past words place strong contextual constraints on the next word in the string, then the next word is not likely to be surprising (informative) and it could just as well have been left out. Because higher-order strings contain less information, recall of them will be better; that is, less novel material must be learned to achieve high recall scores. The effect in Miller and Selfridge's experiment is shown in Fig. 5-4. This shows the percentage of list words freely recalled after one reading. For any list length, recall increases with the order of approximation to English. The leveling-off of the curves in Fig. 5-4 around the fifth order of approximation is an artifact because of the method used to generate strings of various orders. Their higher-order strings appeared increasingly more ungrammatical and composed of more complex words. When these confusing variables were eliminated, Coleman (1963) found an increase in recall between the fifth and higher orders of approximation.

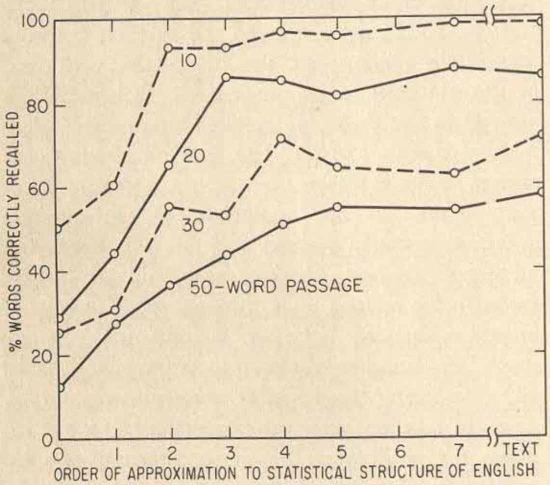


Fig. 5-4. Per cent of words correctly recalled as a function of the order of approximation to the statistical structure of English. (Miller and Selfridge, 1950)

When unrelated words are used, it is known that individual learning curves averaged over the list in free recall have negative acceleration (Murdock, 1960*a*). However, Waugh (1961) finds that in *serial* recall, learning curves are linear. In Waugh's studies, a long list of words is read always in the same fixed order and the subject has to recall the words in the exact order. Though poorer on the early trials, subjects doing serial recall eventually catch and surpass subjects doing free recall (any order) of the same list of words. The linear learning curve for serial recall suggests that subjects acquired a constant number of new items per trial to append to those learned before, and that once an item was recalled, it was perfectly retained. Waugh's detailed results deviate slightly from this simple picture, but it is a good approximate description of what happens. The linear learning curve appears when recall is serial; free recall with fixed serial presentation of the words produces a negatively accelerated learning curve indistinguishable from free recall with random presentation orders.

If the average number of new words serially recalled per trial is a constant m independent of the length of the list, then the trials to learn a serial list of length L should be L/m. That is, a linear learning curve implies a linear relation between L and trials to learn. Waugh (1962) confirmed this relation; she also validated the underlying assumption that m was constant irrespective of the length of the list. Murdock (1960*a*) has found slightly different results with free recall. Suppose k is an empirical constant (6.1 words in Murdock's study) and R_1 is the number of words freely recalled after a single presentation of a list of L words at a presentation rate of t seconds per word. Murdock found the following relation between these quantities:

$$\frac{R_1 - k}{L} = ht$$

where h is a proportionality constant. Fixing presentation time, the percentage of freely recalled words exceeding k is a constant independent of list length. In absolute terms, this means that R_1 increases with the product of L and t. The effect of t can be conceived in terms of the strength of the background conditioning of the words; the more time allotted to study a word, the more strongly it is conditioned. The effect of L upon R_1 might be thought of in terms of increased opportunities for cued associations; the more words learned, the more likely that some of them will have some cue-associative strength to later words to be recalled.

PAIRED-ASSOCIATE LEARNING

In paired-associate learning (PAL), an explicit stimulus is provided for each response term. Normally a number of items are learned concurrently by the subject and training is by the anticipation method (i.e., stimulus presented, subject responds, show correct response). The order of the items is usually randomized from one presentation to the next to make serial cues unreliable bases for responding. The unit of analysis is the individual item, and the belief is that the learning characteristics of a list of *N* items are determined in a simple additive manner from the properties of the items composing it. The assumption is probably wrong in detail (cf., Irion, 1959) but is close enough to serve as a convenient approximation and heuristic. Paired-associate learning has become increasingly popular probably because of its obvious face validity for the stimulus-response-association view that dominates research on human learning. The simplicity of the situation is only apparent, however, since the results rapidly become complex. To handle the complexities, recent theories (McGuire, 1961; Newman, 1961; Restle, 1964) have appealed to multiple processes which determine the course of PA learning. The three processes commonly assumed are stimulus discrimination, response learning, and association formation. An experiment by McGuire (1961) is exceptionally clear in illustrating these processes and, in addition, permits an assessment of the contribution of each factor to performance of the complex behavior exhibited in PAL. Here some of the material relating to each of the assumed processes will be reviewed.

DISCRIMINATION At the least, PAL involves multiple stimulus discrimination. Gibson (1940) recognized this and first applied to verbal learning the concepts of stimulus generalization and differentiation. If the stimulus members of two items are similar, the response learned to one will generalize to the other; confusion errors and possibly response blocking will result. Similarity of the stimuli in a list produces slower PAL (Gibson, 1942; Underwood, 1952; Shepard, 1958). The concept of similarity in this context has sometimes been attacked as vague, but in recent measurement models (Shepard, 1962) it is defined precisely enough in terms of distance between pairs of points (stimuli) located in a metric space.

In recent accounts, during discrimination, the subject is supposed to acquire mediating responses to the stimuli; these responses select or encode some aspect of the stimulus which distinguishes it from similar stimuli to which other responses must be made. Evidence that subjects in learning do encode stimuli, and then only partially, comes from studies of stimulus predifferentiation and of transfer following "concept" learning. The predifferentiation effect refers to the positive transfer produced by pretraining the subject to attend to discriminating aspects of the stimuli. The pretraining might consist simply of having the subject make active comparisons between similar stimuli and to notice in what ways they differ; later paired-associate learning of new responses to such stimuli proceeds faster than if the predifferentiating experience is absent (Gibson and Gibson, 1955; Goss, 1953). If the aspects that are used to distinguish the stimuli in the pretask are absent on the criterion task, no transfer is expected and none is obtained (Hake and Erickson, 1956). If the pretrained aspect is present but irrelevant in solving the criterion task, then negative transfer is obtained (Kurtz, 1955; Kendler and D'Amato, 1955).

Gibson's ideas of stimulus and response differentiation have been formalized into an information-processing model of verbal learning by Feigenbaum and Simon (Feigenbaum, 1961). In their EPAM model, familiarization with the verbal items is represented as a process of adding test nodes to a discrimination net. Each node contains a test question (e.g., "What is the middle letter?") followed by two or more further branches (to lower nodes) that are taken depending upon the outcome of the node's test. Recognition of an item consists of sorting it through this network to some, perhaps temporary, terminal node. If two items are confused (sorted to the same terminus) when they should not be, then a difference between them is found and this difference is added as a further node at that point in the net. Stored at each terminal node (to which a stimulus is sorted) is a coded representation of the response item or the response image. The subject then attempts to retrieve the response item by sorting this coded information through the same discrimination network. If this process fails (the model errors), then more response information is stored in the response image. Space does not permit further elaboration and interested readers should consult the Feigenbaum and Simon papers. This ambitious model has been realized in an IPL-V computer program, and the program has been run to simulate several classic verbal learning experiments. The fit of the model's predictions to data have been extremely encouraging so far. Models of this type are likely to be playing increasingly important roles in the future as our theories of verbal learning become increasingly complex.

RESPONSE LEARNING Response learning refers to the possibility that the nominal response may itself be a novel chain of units that must be learned. In PAL with nonsense-syllable responses, the largest portion of the subject's learning time is probably taken up with response integration. Typically, a preponderate majority of his "errors" represent failures to respond, or incomplete or garbled versions of the appropriate response terms. To perform effectively, the subject must recall the units (letters), the serial order in which they occur, and he must overcome confusions with and interference from other novel strings of letters that are being learned concurrently to other stimuli in the list. His task might be called discriminated rote serial learning. Since serial learning is required with novel compound responses, letter-sequence habits acquired through familiarity with the English language can aid or hinder specific letter combinations; in information terms, a familiar sequence requires less new information to be learned.

The most potent factor controlling response learning is the meaningfulness and/or familiarity of the units. Meaningfulness is a difficult term to define, though Noble (1952, 1953) has proposed an operational index (number of associates, *m*) that parallels intuitive rankings. There is no claim that the number of associates gives the epistemic meaning of a verbal unit. The claim is that the index permits a lawful ordering of results on verbal learning, which is the major requisite for useful scientific concepts.

The effect of response-*m* on serial and paired-associate learning is consistently large. More to the point, Parker and Noble (1960) demonstrated faster PAL when high *m*-values were artificially built into the response terms. Subjects were taught 0, 3, 6, or 9 arbitrary associations to paralogs (e.g., neglan, meardon) of initially low *m*-value. After pretraining, these paralogs were used as response terms in PAL; speed of learning an item increased with its induced *m*-value.

Extensive research on meaningfulness in verbal learning has been done by Underwood and Schulz (1960). They first report high intercorrelations among the following measures of three-letter verbal units (trigrams): frequency of occurrence in printed English text, rated familiarity, rated pronounceability, number of associates produced (*m*), and judgments of how difficult the unit would be to learn. They chose frequency of experience as the independent variable which underlies these intercorrelations and determines differences in verbal learning.

Underwood and Schulz propose a "spew" hypothesis to relate the frequency of experience with a verbal unit to the speed with which it is learned as a response. The hypothesis supposes that the order of availability and order of emission of verbal units is directly related to their frequency. Thus, input frequency determines speed and order of output. The hypothesis has derived prior support from experiments on verbal production. For example, Bousfield and Barclay (1950) asked subjects to produce all the names they could think of referring to objects in specific categories (bird, fruit, male names, etc.). The responses produced had high cultural commonalities and correlated highly with their frequency of occurrence in an English text. Moreover, the order in which the responses were produced by subjects paralleled their decreasing frequency of occurrence in the population. Common, high-frequency names were emitted first; esoteric productions occurred later. In PAL, Underwood and Schulz apply the spew hypothesis to the response learning phase: high-frequency verbal units become available sooner to the subject so that they can enter into associative connections to the stimulus terms.

In their experiments on response meaningfulness, Underwood and Schulz uncover some related facts about learning that are of general interest. For example, they report that in PAL requiring response learning, subjects quickly come to confine their overt responses to just those letters involved in the list responses; the remaining difficulties stem from interference between poorly integrated responses in the list. Such results suggest fast background conditioning of the response elements followed by slower cue learning. The gradual learning and integration of letters (in nonsense trigram responses) over trials is shown clearly in Fig. 5-5. The abscissa in Fig. 5-5, generated value of the trigram, is a measure derived from interletter association frequencies. The higher the generated value, the less new cue learning required to integrate the response term. Another fact of some interest is that when the integrated response is first given, it is nearly always given to the correct stimulus. Thus, the primary measure affected by response meaningfulness is the trials required to emit the integrated response; in one experiment reported, this measure correlated −.98 with number correct in twenty trials.

Throughout a number of experiments varying the estimated preexperimental frequency of single letters, bigrams, and trigrams on the response side, Underwood and Schulz reported high correlations between frequency and learning rate. However, it was noted in the later trigram experiments that rated pronounceability of the unit correlated still higher with learning rate (correlations in the high .80's and .90's). Partialing out the pronounceability differences, the partial correlation between printed frequency and learning of trigrams was reduced to insignificant levels. However, firm conclusions regarding the relative potency for learning of various frequency measures, pronounceability ratings, and meaningfulness ratings of a verbal item will be hard to tease out. The measures are highly correlated in general, cases of discrepant ratings of an item on two scales occur rarely, and they are likely to be instances of unreliability of the measures. Thus, using their scales, Noble (1963) and Johnson (1962) came to different conclusions regarding the relative potency of these factors when they analyzed the Underwood and Schulz data.

Trigram frequency is important, by hypothesis, because the response term becomes available sooner for associative hookups. Thus, the beneficial

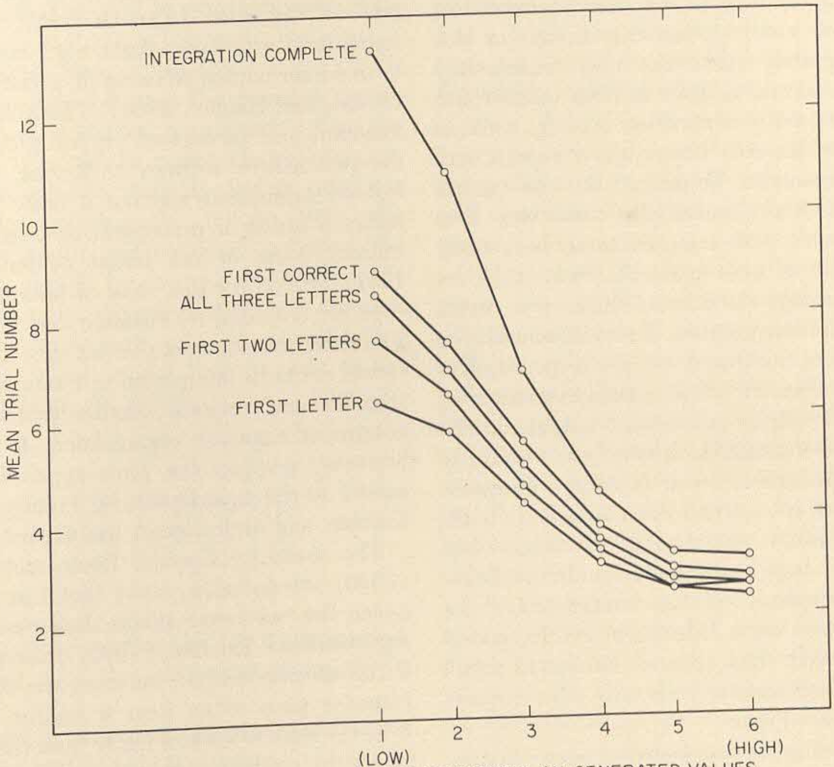


Fig. 5-5. Stages of learning with trigrams as responses as a function of generated values. The graph shows the average trial number on which various subparts of the response terms were given. "First correct" means the trial on which the complete response was given to the correct stimulus for the first time. (Underwood and Schulz, 1960)

effect on PAL of pretraining subjects on the response terms is ascribed to their earlier availability (Underwood, Runquist, and Schulz, 1959). But this is not the whole story; highly similar responses (i.e., nonsense syllables with many common letters) become available quickly but yet retard *S-R* associative learning because they are confused (Horowitz, 1961, 1962). Thus, prefamiliarizing subjects with the responses may produce positive transfer to PAL because the responses are better discriminated. Saltz (1961) had evidence for this in a study which obtained an effect of response prefamiliarization even though the responses were available (on cards) to the subjects during the criterion PA task.

ASSOCIATION FORMATION Suppose the stimuli are well differentiated and the responses have become well-functioning units in the subject's repertoire: how, then, does the subject come to hook up the response to the stimulus? If subjects are asked, most report the use of mediators that serve to organize the two terms. In learning the pair RZL–CAT, the subject may report that Z sounds like the hissing of a cat. Mnemonic devices of this sort are frequently used; they vary considerably within and between subjects in their content and elaborateness. With nonunitary stimuli, such as nonsense syllables, only one or a few aspects may enter the mnemonic. Mnemonic devices encode the material into "meaningful" relations that bring a person's past associations to bear upon the acquisition of new materials. Items with reported mnemonics are learned faster than items for which none are reported. If several mnemonics relate aspects of the stimulus to the response, then recall probabilities are compounded. In connecting RZL–CAT, multiple mnemonics might be Z–hissing–cat, R–rat–cat, L–leopard–cat, RaZzLe–dazzle–CATtle, and so on. If organized mnemonics of this sort are learned, then so-called "backward" associations (giving the S term when shown the *R* term) are understandable. Backward recall depends on the integration of the nominal stimulus term (Murdock, 1956); poorly integrated stimuli lead to poor backward recall because the mnemonics encode only a few aspects of the full stimulus term.

If association is viewed in terms of the organiization of the materials into mnemonics, then the relationship between the two terms (to be associated) is a contributing factor. Epstein, Rock, and Zuckerman (1960) have proposed that meaningful terms are easier to learn because they are easier to organize into mnemonics. For example, pairs of concrete nouns (lamp–bottle) are easier to learn than pairs of abstract nouns, prepositions, conjunctions, and verbs, and the differences do not parallel differences in frequency of experience with the terms. If a small connective is inserted between concrete nouns to be associated (lamp–in–bottle), establishing a "sensible" relation, the associations are better learned. Similarly, learning is quicker if the two objects are shown perceptually in some kind of interaction (a picture of a *hand* plunged in a *bowl of water*). The perceptual coherence of the terms presumably affects the ease with which they become organized together.

Other studies have shown how ease of associating two terms depends upon their perceptual relationship at the time of presentation. Pairing of two figures is easily learned if (*a*) they "fit together" in some pictorial sense, e.g., a piece of pie, and a pie with a piece missing (Prentice and Asch, 1958), (*b*) one figure is a "good continuation" of the other (Kaswan, 1957), or (*c*) the terms form a unitary whole, e.g., *beads* are used to make an outline drawing of a *teardrop* (Asch, Ceraso, and Heimer, 1960). These studies on association and perceptual organization stem from the provocative writings of Kohler (1929). For Kohler, association was not a basic concept but rather a result of perceptual or cognitive organization. Some of the initial evidence (Kohler, 1941) offered for this view of learning has been cogently criticized by Postman and Riley (1957). The latter investigators showed that Kohler's early results could be interpreted in terms of elementary stimulus and response factors without invoking notions of cognitive organization. It is doubtful, however, whether the same type of arguments extend to the experiments by Prentice and Asch, Kaswan, and Asch, Ceraso, and Heimer.

The study by Epstein, Rock, and Zuckerman (1960) attempted to show that the relation between the two terms affects their ease of becoming associated. Erickson (1963) demonstrated that if the specific relation between the stimulus and response term of an item is unique in the list, then the item is learned faster than the others. He offers this fact as a possible explanation for the von Restorff effect in PAL. This effect refers to the faster learning of an item that is different or isolated in some way from the other items in the list. For example, in the list below, the third item has a different stimulus term than the other three items. It is routinely found that the isolated item is learned faster than the others.

S—R	S—R
CUY—MIH	375—KUX
DEQ—TIV	YIK—ZAG

The effect is not peculiar to the odd term being in the stimulus position. The third item could be turned around, so that 375 is the response to KUX, and it still is learned faster than the others (Nachmias, Gleitman, and McKenna, 1961). The main efforts at explaining the isolation effect have utilized notions of stimulus or response generalization: that is, the isolated item is thought to be learned faster because it is subject to less generalization from the other stimulus-response connections being learned (e.g., Newman and Saltz, 1958).

Erickson's experiment demonstrated that the isolation effect could be obtained simply by having a unique relation between the stimulus and response terms. The way in which this was done is schematized in the lists below. In both lists A and B, the third item is isolated because of the unique relationship between the S and *R* terms. In each case, it is learned more quickly than the other items. The result cannot be explained in terms of differential stimulus and/or response generalization favoring the unique item. On grounds of stimulus and response confusion, the isolated item should be learned slower than the other items (i.e., for the unique item, the stimulus and response could each be confused with two similar items, which is not true for any of the other items). Thus, the results

List A	*List B*
SWJ—217	SWJ—DXR
RKD—764	RKD—JPZ
KSC—ZNH	KSC—472
186—DXR	186—217
305—JPZ	305—764

lead to the conclusion that the rate of associating units depends upon the relationship between the two items.

The preceding discussion regarding meaningful mnemonics in PAL should be tempered, however, by evidence that some materials are learned without reportable mnemonics. The typical reply to queries is "I couldn't think up any association for those two items, so I just memorized them by rote." Although the question has not been investigated, we may presume that the probability that subjects use mnemonics will vary with the materials. In associating names of concrete objects, probably most learning is by mnemonics. On the other hand, Battig (1962) has reported low incidence of mnemonics when the task was to associate random nonsense figures with two-digit numbers. Because mnemonics utilize past associations, materials with reportable mnemonics are learned faster. The actual form of the learning curve may depend on this factor. Items susceptible to organization into mnemonics may be learned all at once when the mnemonic occurs to the subject; items without reported mnemonics may be acquired gradually. The distinction may be of use in the discussion of the next topic.

THE TIME COURSE OF ASSOCIATION FORMATION

The question is how to describe accurately the course of formation of an association over successive practice trials. If precise measurements could be made of the probability on each trial that a single stimulus evokes the correct response from a subject, what would this curve of response probability look like when plotted over successive practice trials? Would it increase gradually trial by trial or would it consist of one or more discrete jumps from one probability value to a higher one each level being maintained for several trials? The answer to this question cannot be obtained directly since the precise measurements cannot be made. On a single trial for a single S-*R* pair, we observe a success or an error, but neither gives sufficient information to infer much about the underlying response probability (except to know that it cannot be zero or one, respectively). Consequently, the attack on this question has been indirect. One assumes a particular answer to the question (a theoretical hypothesis) and then collects evidence relevant to the implications of the assumption.

The two opposing views on this question have been labeled incremental and all-or-none theory. As in most controversies, the caricature of the opposing point of view tends to become polarized, though logically there is a wide range of intermediate positions that can and have been taken on the theoretical continuum. A basic statement of an incremental theory is contained in the postulates of Hull (1943). The associative connection (habit, *sHr*) between a stimulus and its reinforced response varies in strength from zero to a maximum, increasing with each reinforced trial in a negatively accelerated fashion towards its maximum. The increase in sHr per trial is deterministic; probabilities enter later in the chain of reasoning. In Fig. 5-6 is shown graphically the hypothesized accumulation of excitatory potential (proportional to sHr) to an *S-R* connection resulting from successive practice trials. Inserted into Fig. 5-6 are two mechanisms that Hull assumed. One of these is the threshold for response evocation, sLr; it was assumed that the habit had to exceed a threshold value before the response could occur. The second mechanism graphed in Fig. 5-6 is what Hull called "oscillatory inhibition." The true habit to an *S-R* connection was, on any trial, subjected to a randomly variable amount of inhibition, subtracting from the effective strength on that trial. The determinants of this oscillating inhibition were not specified by Hull, although others have assumed that it represents the effect of variability in the stimulus or in the stimulus-reception process. This random factor varies from moment to moment, yielding a normal distribution of values (the bell-shaped distributions in Fig. 5-6). At the moment the experimental stimulus is presented, a particular value of inhibition is present to subtract from the true habit strength. The response rule of the theory is simple: If the momentary effective habit strength is above the threshold, the response occurs; otherwise not. This rule enables one to transform habit into response probability. The probability of the response is equal to the probability that the subtraction of the randomly variable inhibition from true habit yields a suprathreshold value. In Fig. 5-6, this probability is represented by the shaded portion of the bell-shaped normal distribution subtracting from the habit curve.

Hull's response rule is essentially that proposed earlier by Thurstone (1927) for handling psychophysical judgments. The threshold notion is taken over directly; the normal distribution of oscillatory inhibition is taken over from Thurstone's notion of "discriminal dispersion." The threshold and oscillation functions have no observational correlates; they are simply calculational devices, permitting one to translate sHr into statements about an observable, namely response probability. In Fig. 5-7 is shown the type of probability curve implied by the apparatus in Fig. 5-6; it is S-shaped between 0 and 1.00.

This theory is flexible. There are at least four unspecified parameters in Fig. 5-6 that can be varied to generate a variety of curve forms of the type illustrated in Fig. 5-7. The four parameters

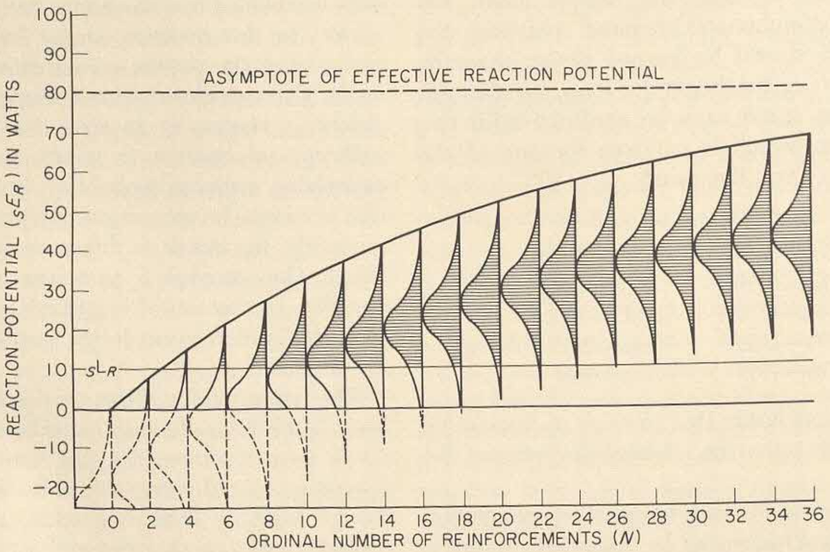


Fig. 5-6. The smooth curve represents the development of reaction potential over successive trials. The up-ended bell-shaped distributions represent the random oscillatory inhibition that may subtract from the true reaction potential on each trial. The area of this bell-shaped distribution that is above the threshold (sLr) is shaded. The shaded area represents the probability of the response on each trial. (Hull, 1943, p. 327)

are (1) the initial starting value of habit, (2) the rate at which habit increases over trials, (3) the value of the threshold, and (4) the range or variance of the oscillation function. If the habit starts high or if the threshold is low, then the resultant probability curve will be nearly negatively accelerated throughout rather than exhibiting the initial positive acceleration seen in Fig. 5-7. If the rate of learning is large relative to the range of oscillation, then the resultant probability curve will approximate a step function, going from zero to unity in essentially one trial. Because of the flexibility induced by the surplus of arbitrary parameters, the theory is difficult to disprove, which may account for its survival value.

The theory as presented is an oversimplification of the apparatus needed to make it relevant to, say, paired-associate learning. The discussion above refers to learning of a single stimulus-response connection in isolation. The normal paired-associate situation involves multiple stimulus-response pairs being learned concurrently. Thus, when calculating the course of performance on the pair S_a-R_a, one must consider (1) the generalized tendencies (habits) for S_a to evoke responses R_b, R_c, . . . due to stimulus generalization from S_b, S_c, . . . to S_a, and (2) the inhibition (see Hull, 1943) not to give responses R_b, R_c, . . . to S_a, and the generalized inhibition (to S_a) of response R_a to stimuli S_b, S_c, ... Also the response rule is changed because, to S_a, the subject has a net habit (*sHr* minus generalized inhibition) to make R_a and generalized net habits to make R_b, R_c, . . . , and these habits oscillate independently of one another. It is assumed that when S_a is presented, whichever net habit is momentarily dominant determines the response if that habit is suprathreshold. When these modifications are added, the theory becomes unwieldly and the number of arbitrary parameters nearly exceeds the number of observations taken in the ordinary experiment. Some theorists (e.g., Runquist, 1962) have attempted to make enough simplifying assumptions so that the system can be used to analyze simple paired-associate data, but the quantitative predictions of the system have not been very rewarding in spite of the free parameters.

In lieu of this, the theoretical system has been used in practice as a qualitative source for explaining and ordering experimental results. A variety of qualitative predictions apparently follow from the theory. The qualifier "apparently" is inserted because the steps of the derivation from the expanded system appropriate to multiple paired-associate learning are usually not filled in. To illustrate the ambiguity of the theory on particular points, consider an example. Suppose a subject is learning paired associates with successive study (reinforced) trials and test trials (to stimuli alone). It has been claimed (Wollen, 1962; Battig, 1962; Williams, 1962) that the

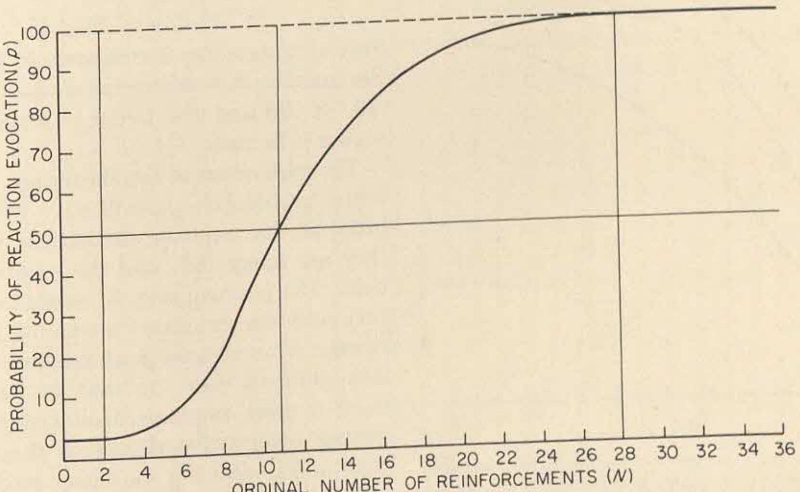


Fig. 5-7. Curve showing the theoretical probability of response over practice trials derived from Fig. 5-6. The values plotted are the proportions of the bell-shaped distribution exceeding the threshold value, i.e., shaded area in Fig. 5-6. The curve is a distorted S-shape with an elongated tail to the S. (Hull, 1943, pg. 331)

theory implies that the probability of the first correct response following n consecutive errors (and reinforcements) to a given item should increase with n. Data have been offered supporting this notion and it has been concluded that the theory was confirmed. But is it? The prediction does follow from the assumptions exhibited in Fig. 5-6 when considered by themselves. But consider the expanded theory necessary to take account of multiple learning of concurrent habits. First, in selecting items missed n times in a row (for calculating conditional probabilities), one selects differentially items whose parameters make for slower growth of habit. This selection will have an unknown effect on the outcome. Second, the greater n is, the stronger will be the generalized habits to make other responses to the reference stimulus term. Thus, although the correct habit is increasing in strength with trials, so also are the numbers and/or strengths of the generalized habits which lead to errors on the reference item. Depending upon the precise balancing between the parameters of these effects, it would seem that practically any prediction could be obtained from the theory regarding the point under investigation, viz., the way in which the conditional probability of the first correct response varies with the number of consecutive prior failures. Thus, in terms of the expanded theory, the prediction is ambiguous. Other sources of difficulty in deriving unambiguous predictions from Hull's theory for even simpler situations have been pointed out by Cotton (1955).

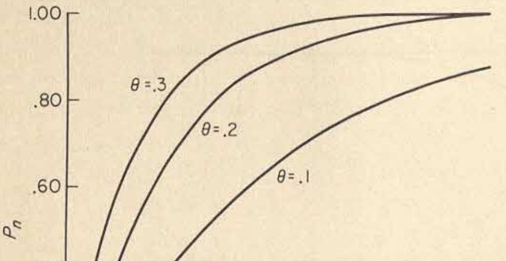


Fig. 5-8. Theoretical curves of probability of correct responses over practice trials derived from Eq. 3 for the linear model. It has been assumed that p_1 is zero. The three curves differ in the value of the learning rate constant, θ. The larger the value of θ, the faster the curve rises towards unity.

An alternative version of an incremental learning theory is contained in what has come to be called the linear model. The model stems from papers by Estes (1950), Bush and Mosteller (1951), and Estes and Burke (1953). The effect of practice is not conceived as affecting some hypothetical habit strength variable but rather operates directly upon response probabilities. Let p_n represent the probability of the correct response by a subject to some particular paired-associate item on its nth presentation. The effect of reinforcing the correct response on the nth presentation is to increase the probability of the correct response by a linear transformation, where θ is a fraction between 0 and 1:

$$\begin{aligned} p_{n+1} &= (1-\theta)p_n + \theta \\ &= p_n + \theta(1-p_n). \end{aligned} \tag{2}$$

The top line emphasizes the linearity of the transformation. The second line exhibits the assumption that the increment in probability (transforming p_n into p_{n+1}) is a constant fraction, θ, of the amount that the probability could have increased towards unity. By successively increasing the value of response probability according to the operator in Eq. 2, one obtains the general equation for p_n:

$$p_n = 1 - (1-p_1)(1-\theta)^{n-1}. \tag{3}$$

Several plots of Eq. 3 are shown in Fig. 5-8 where the learning rate parameter, θ, has the value .10, .20, or .30 and the initial probability p_1 is assumed to be zero.

The parameters of this theory are p_1 and θ. The initial probability, p_1, will vary with the availability of the response alternatives (i.e., whether they are integrated) and the number of alternatives. The learning rate, θ, would be expected to vary with the conditions promoting intralist interference. One advantage of the linear model is its computational ease, so that in its applications there is more emphasis upon fitting and/or predicting quantitative details of the data. A few illustrations applying the linear model to paired-associate learning have been published by Estes (1959*a*).

The alternative class of theories about learning have gone under the name of "all-or-none" theories. The single classificatory name does not, however, do justice to the broad range of possible models of this type (Estes, 1959*b*; Restle, 1964). The assumption is that the learning of a single item by a single subject can be characterized by a step function rather than a smoothly increasing curve. The various models differ in their assumptions regarding the number of steps (performance levels) and the laws governing transitions among the performance levels. Figures 5-9*a* to 5-9*c* illustrate some of the varieties of step-functions that have been proposed.

Figure 5-9*a* represents the most elementary form of the step models. It assumes that a subject-item combination can be characterized as being in either of two "states," with response probabilities p_0 and 1.00. The subject begins in state p_0; on each reinforced trial, there is some fixed probability c that the subject learns the appropriate association. The models exhibited in Figs. 5-9*b* and 5-9*c* differ in that they identify one or more intermediate stages in learning, intervening between the starting value p_0 and the terminal asymptote of unity. The appropriate mathematical representation of each of these theories is a finite Markov chain. The states of the Markov chain are the different levels of response probability, and the learning rate parameters represent the probability of a transition from one state to the next. Because finite Markov chains have been well analyzed mathematically, it is a simple matter to derive predictions for a variety of features of a given set of experimental results.

Because of the uniqueness and vulnerability of the elementary one-step model in Fig. 5-9*a* (with constant probability c of a transition), much experimentation on the "incremental versus all-or-none" issue has concentrated on this model. The implicit ideas were first suggested by Rock (1957) to account for some of his experimental results; subsequently, Estes (1959*b*, 1961) and Bower (1961*a*, 1962*a*) gave mathematical statements to the theory and added corroborating evidence.

The experiments stemming from Rock's work will be discussed first since they have chronological priority. In the basic experiment, two conditions of PAL were compared. In a control condition, subjects learned twelve randomly selected letter-number pairs by repeated study-test trial cycles: on study trials, each *S-R* pair was shown for five seconds; on test trials the S term was shown, the subject tried to provide the *R* term, and he was given no information regarding the correct response. The same twelve pairs were repeated through a number of study-test trial cycles until the subject answered all twelve items correctly in one trial. In the experimental condition, Rock adopted the novel procedure of replacing with new items those items the subject missed on each test trial. Each subject in this replacement group was continued until he achieved one errorless test on the twelve items presented on the previous study trial.

Rock's rather surprising finding was that the mean learning curve and average trials to reach criterion were nearly identical for the repetition and replacement groups. He concluded that a repeated reinforcement to a missed item must have had about the same effect as the first reinforce-

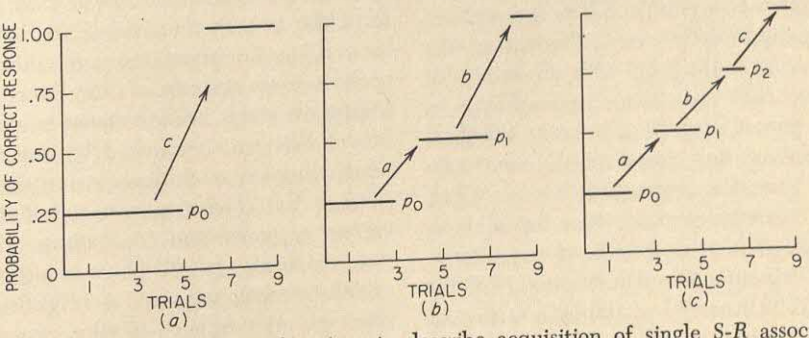


Fig. 5-9. Some examples of step functions to describe acquisition of single *S-R* associations. Figs. (*a*), (*b*), and (*c*) show, respectively, one, two, or three steps to the acquisition process. The probability of a one-trial transition to the next higher step is indicated beside the directed arrows. The number of trials during which performance is at a particular level before the next jump randomly varies across subjects.

ment to a new (replacement) item. Other things being equal, the results were inconsistent with an incremental theory and consistent with the assumption of one-trial learning, with the probability of learning being constant independent of the number of prior repetitions and failures to learn.

The question, of course, is whether "other things are equal" for the repetition and replacement groups. Specifically, there is the possibility that subjects in the replacement group are at a favorable advantage because the procedure differentially removes the difficult items, so they are likely to end up with an easier list of items. Rock recognized this possibility and performed several experiments attempting to reduce or eliminate selection artifacts (Rock, 1957; Rock and Heimer, 1959). Further experiments (Wogan and Waters, 1959; Clark, Lansford, and Dallenbach, 1960) yielded essentially the same conclusion as did Rock's first study. The question has been raised, however, whether these experimental controls on item selection have succeeded (Postman, 1962), and there have recently been reported a number of attempts to assess the amount of item selection induced by the replacement procedure.

The theme of these subsequent evaluations can be seen in an experiment by Williams (1961). Using Rock's materials and methods, Williams first replicated Rock's finding of nearly equal trials to criterion for control and replacement groups. Secondly, she asked whether the final items for a replacement subject were easier than the randomly selected items learned by control subjects. To assess this, she had a yoked control subject learn by repetition the final list arrived at by a replacement subject. Such yoked-list control subjects learned in significantly fewer trials than either of the other two groups. Thus, the replacement subjects apparently were eliminating the difficult items and ending up with an easier list on the average. The easy items appeared to be those with responses among the lower numbers (1-15), suggesting that response differentiation (Saltz, 1961) was the responsible factor. From these results, one may conclude that the replacement procedure induces selection of easy items. A subsequent experiment by Underwood, Rehula, and Keppel (1962) arrived at similar conclusions regarding item selection. Thus, the implicative force of Rock's initial results for incremental theory have been lessened. Still one may note that Rock's replacement subjects did learn in "one trial" in the descriptive sense that an item was replaced unless it showed a one-trial transition to 100 per cent correct responding. Some may also feel that somewhat implausible coincidences must be invoked to argue that the item selection (for replacement subjects) occurs in just the right amount to offset the benefit of repetition (for control subjects).

Related experiments have attempted to assess the benefits of repetition for individual items that have been missed a few times. The implication of the simple all-or-none model in Fig. 5-9*a* is that the conditional probability of a success following n consecutive failures to an item will be a constant, $c + (1 - c)p_0$. Experiments by Wollen, Battig, and Williams (reviewed earlier) showed that this prediction was false for their types of learning materials; in their studies, this conditional probability increased with the number of prior reinforcements (and failures). The results are consistent with the one-step model (Fig. 5-9*a*) which assumes that c increases over trials due to warm-up, learning to recall the responses, or whatnot. They are also consistent with a one-step model which assumes an asymptote slightly less than unity. Furthermore, they are also consistent with the two-step model in Fig. 5-9*b* or with the linear model. As typically happens with experiments on this issue, the simple model rejected by the data is reasonably clear; the alternative model confirmed by the data is not clear since no alternative is put to a stringent test.

A second series of experiments surrounding the incremental versus all-or-none issue was initiated by Estes (1960; 1961) and Estes, Hopkins, and Crothers (1960). Estes noted that in elementary form the two theories differ in their predictions concerning the repetition and alternation of responses over a series of unreinforced test trials. Suppose a single reinforcement is given to a number of *S-R* pairs, followed by two test trials on which there is no information to the subject concerning the correct response. Let 1 represent a correct response and 0 a failure. Then over the two test trials we will observe either 11, 10, 01, or 00 for each sequence (subject-item). Let p_{ij} represent the respective joint probabilities. One implication of the linear model is that p_{10} should equal p_{01}: that is, the number of alternations from success to failure should be balanced by those from failure to success. Estes (1961) has shown that the prediction follows even though it is assumed that (*a*) the effect of the reinforcement varies over sequences, and (*b*) the response occurring on the first test is increased in probability (by a linear transormation) by an amount varying over sequences. These are rather general conditions. The initial evidence presented on this issue by Estes was not in accord with these predictions; specifically, p_{10} was considerably larger than p_{01}. Results of a similar kind have been reported by others. The results were consistent with an all-or-none model which invoked some forgetting of correct responses between the two test trials. However, this evidence has been put in doubt recently due to some results by Postman (1963). In eight different PA conditions with the double-test design, Postman found the alternation probabilities, p_{10} and p_{01}, were about equal. Thus, research is needed to specify the causes for the discrepancy. One factor affecting the frequency of alternations is the time permitted the subject to get out a response on test trials (the exposure time). The writer has found the proportion of alternations $(p_{10} + p_{01})$ to be between four and five times larger with brief (1.25 seconds) than with long (10 seconds) exposure times on test trials. The influence of exposure time on the results is an ancillary performance effect easily incorporated into either model. Until new experiments clarify the facts, however, the RTT evidence is presently ambiguous on the theoretical issue.

Another methodological attack on the incremental versus all-or-none issue has been employed by Suppes and Ginsberg (1961) and Bower (1962*b*). The procedure consists of examining backwards learning curves to see whether they are horizontal. A backwards curve (Hayes, 1953) is obtained by averaging responses from each sequence 1, 2, 3, . . . trials back from the last error on each sequence. Alternatively, one may plot a forward "prelearning" curve starting from trial 1 by using on each trial only those sequences for which the last error occurred on a later trial. If the one-step process of Fig. 5-9*a* is a proper description of the data, then the prelearning curve will be horizontal, i.e., a constant level of response probability, p_o, over trials prior to the last error on each sequence. This result would be obtained even though the probability of learning (going from p_o to 1.00) was increasing with trials. Incremental theories would not predict flat backwards learning curves, regardless of differences in learning rates of subjects and/or items averaged together in the curve. A number of Monte Carlo runs (cf. Bush and Mosteller, 1955) with the linear model by the writer demonstrated this with several plausible distributions (over the subjects and items) of learning rates.

Initial tests of this prediction for verbal learning situations involving two response alternatives gave encouraging results for the all-or-none theory (Suppes and Ginsberg, 1961; Bower, 1962*b*). Figure 5-10*a* shows some results from two-response PAL and Fig. 5-10*b* shows results from a two-choice verbal discrimination learning task. Both exhibit the requisite stationarity of response probability prior to the last error. The smooth increasing curves at the top of Figs. 5-10*a* and 5-10*b* are the trial by trial probabilities of correct responses averaged over all sequences. The gradually increasing curves apparently result from averaging together sequences for which the jump in probability occurs at different trials. Bower and Trabasso (1964) have shown similar data for two-category concept identification experiments.

It should be pointed out immediately that such stationary prelearning curves are frequently not obtained under slightly more complicated experimental situations. This could result for several reasons. One possibility is that the one-step model is basically right, but that in some conditions the terminal probability of a correct response is less than unity (say .97). In this case, empirical backwards curves from the alleged point of the "last error" (before a list criterion) would not be expected to be stationary. A second possibility is that two or more performance stages are encountered during acquisition. The nonstationary results encountered so far are easily handled by the two-step model in Fig. 5-9*b*, which assumes an intermediate performance level. The strategy adopted by the writer is to determine the experimental conditions under which stationarity is obtained. Effectively, these conditions amount to a relatively small subset of the experimental arrangements customarily studied in PAL. That is, it is a relatively easy matter to complicate the situation so that stationarity is violated. The theoretical strategy has been to begin with the elementary all-or-none idea as a fundamental base, and to handle various complicating factors by compound-

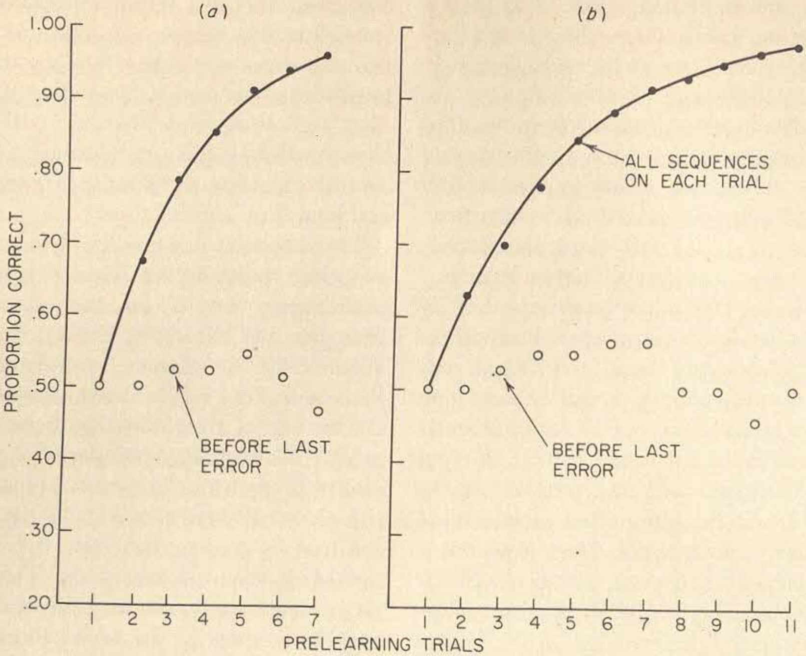


Fig. 5-10. Prelearning curves for paired associates (a) and verbal discrimination (b) learning. The chance probability of a correct response is .50. The lower points represent the proportion of correct responses over those sequences for which the last error has not yet occurred. The upper curves are the obtained and predicted proportions of correct responses averaged over all sequences whether or not their last error has occurred on some earlier trials. The theoretical function fit to these data is $p_n = 1 - .5(1-c)^{n-1}$, where $c = .344$ and .252 in Figs. (a) and (b), respectively.

ing or concatenating the basic theory. Restle (1964) was the first to suggest this approach; though early results are encouraging, it is too early to tell whether the strategy will be useful in the long run.

The sources of complication are essentially those having to do with stimulus discrimination and response learning (integration, differentiation). Let us review briefly some of the evidence concerning the effect of stimulus confusability on the probability of obtaining the simple all-or-none results. If stimulus confusability is minimized and response learning is circumvented, then the strict all-or-none pattern of results seems to be obtained. Using very distinctive pictures of objects as stimuli, high-frequency nouns as responses, and ten seconds to respond on test trials, Bower (1962*b*) found that 99.8 per cent of the subject-item sequences had no errors following the first correct response in the sequence. Such results are to be expected if response probabilities jump from zero to one in one trial.

A second experiment by Bower and Levine (reported in Restle, 1964) found that the extent to which the results deviated from a simple one-step description varied directly with the opportunity for stimulus confusions. The stimuli consisted of three Korean characters, and they were paired with common English nouns. With a twelve-item list, clusters of three items had common characters but differed from all other clusters. The clusters had either 0, 1, 2, or 3 common Korean characters; when there were three common characters, the stimuli within the cluster varied in the left-to-right order of the characters (i.e., *abc*, *bac*, *cab*). The principal results were that the percentage of sequences within a cluster having a reversal (i.e., at least one error following the first correct) increased almost linearly with the number of common characters in the cluster. In the extreme cases, with no common characters (high discriminability) the percentage reversals was only 1 per cent and the results were fit well by a simple one-step learning function; with three common characters, the percentage reversals was 34 per cent and the one-step model was rejected.

Restle's analysis of these data supposes that there are two stages to learning. The first stage consists of conditioning the responses to certain aspects of the stimuli. If that aspect differentiates the stimulus from other stimuli, then the learning is over (one step). If the aspect is not differential, then stimuli with common aspects will be confused and generalization errors will occur. The second stage Restle identifies with the subject discriminating between confusable stimuli, eventually selecting a differential aspect by trial and error. The probability that the "confusion" state is entered at all depends on the similarity of the stimuli, as does the probability of leaving the second stage (completing learning) if it is entered. Each stage proceeds in an all-or-nothing manner. However, the result of convoluting two all-or-nothing processes is a two-step individual learning curve. If there are four known responses, then the initial probability of a correct response will be near .25. If two stimuli in the list are confused with one another, then when the second stage is entered, response probability to these stimuli jumps to .50 for several trials, and thence to unity when the discrimination is solved. The instances in which the model predicts a one-step learning function arise when (*a*) the stimuli are dissimilar so that confusion errors are minimized, or (*b*) there are just two response alternatives. The latter happens because one obtains an average correct response probability of .50 whether the subject is just guessing (stage 1) or is confusing similar stimuli with different responses (stage 2).

The other complicating factor mentioned above was that of response learning, in the sense of integration and recall of the response terms. Satisfactory extensions of the multistage model are not available presently to account for the response learning factor. One perplexing problem is to identify what should be the appropriate unit of the response to which the theory is to be applied. Suppose the responses are paired consonants such as JW, FB, etc. One may think of the two letters of each response and their order becoming separately associated with the stimulus member. If the learning of each letter and their order each proceeds on an all-or-none basis, then the resultant learning sequence for the compound response JW will not appear to be all-or-none in form. These ideas, suggested by the work of Crothers (1962), have been worked out in detail elsewhere (Bower, 1961*c*). The writer has carried out several experiments to show the feasibility of this line of argument. In one study, the response compounds consisted of two components, A or B and 1 or 2, making four compounds A1, A2, B1, B2. These compound responses were assigned equally often in a twenty-item PA list which subjects learned. Separate analyses of the component

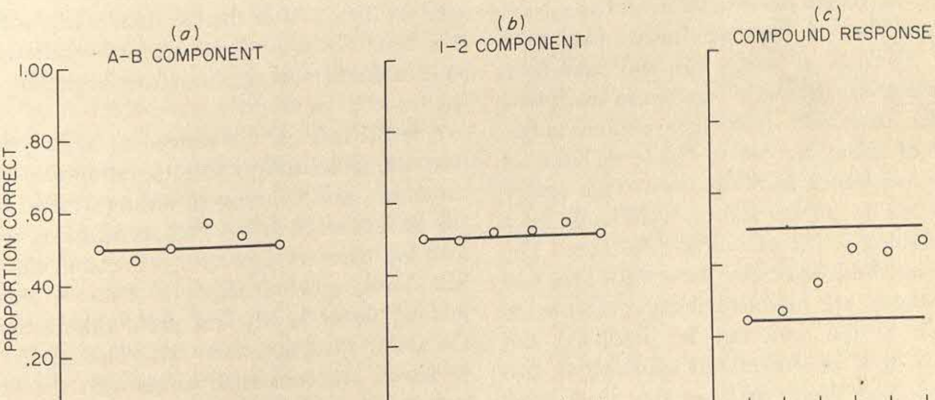


Fig. 5-11. Prelearning curves for the A-B component (11*a*), the 1-2 component (11*b*), and the entire compound response (11*c*). The component proportions fluctuate around .50. The proportion of correct compound responses increases from .25 towards .50 over successive trials before the last error on the response compound.

response sequence indicated that the learning of each component was an all-or-none process (cf. Figs. 5-11*a* and 5-11*b*). However, counting corrects only if both components were correct, the results for learning of the compound responses were not stationary (Fig. 5-11*c*). Probability of a correct compound response rises towards .50 over trials before the last error on the compound. This rise is evidently due to those cases in which one component is learned before the other; the rise in Fig. 5-11*c* is gradual because of averaging across sequences for which this jump from .25 to .50 occurs at different trials.

If only the results on the compound responses (Fig. 5-11*c*) were available, one would conclude that we are observing a gradual learning process. However, the component results in Fig. 5-11*a* and 5-11*b* suggest how the gradual compound curve arises from more basic all-or-none changes. Such data illustrate at an elementary level how compounds of molecular all-or-nothing learning processes can produce molar results which appear gradual in their major features. Similar results have been reported by Bower and Theios (1964) and by Kintsch (1962). The implication of these data is that the appropriate model to handle response learning will probably depend on the unit of analysis involved. The results cited above are, however, only programmatic, and much work remains in developing multistage models to handle response learning.

To summarize, then, this extended discussion concerning the course of acquisition, the initial promise of the simple all-or-none model as a complete description of learning failed, almost of necessity; a variety of results can and have been marshaled against it, and it appears to work well only under extremely restrictive circumstances. This state of affairs has led to the formulation of multistage models of learning, each stage associated with one or another source of difficulty for a subject learning a list of paired associates. The multistage models, especially those with only two or three stages, are mathematically tractable, so that details of the data can be predicted and quantitative tests of differential assumptions can be effected. The theory assumes that each stage is passed in an all-or-nothing fashion, and the passage (learning) parameters reflect how much difficulty subjects have with particular aspects of their total learning problem. Whether a two-stage model really differs much from an incremental model is a question best left to a practicing semanticist. The assumptions appear different and a number of its predictions differ from that of a continuous-growth model. Some of these are detailed in the papers by Bower and Theios (1964) and Restle (1964).

SERIAL LEARNING

One of the basic facts about behavior is that it is serially organized. The consequences of behavior typically depend not only upon which responses are made but also upon the order in which they occur. For conceptual analyses, a stream of behavior is broken into a chain of relatively discrete responses, with each response in the chain producing the cues for the next response. The importance of sequential ordering in behavior has motivated a substantial amount of research on serial learning. We will review some of the experiments on serial learning of verbal units.

Experiments on serial learning (SL) with verbal materials have come to have a reasonably standard methodology. A list of verbal items are displayed one at a time in a fixed order, frequently by a mechanical exposure device like a memory drum. In the classical anticipation method, the subject uses the item presented as a signal to anticipate the following item in the series. After a fixed time, the next item is exposed. Thus, each trial is successively a learning and a test trial. Also, each item serves in turn as a response (to the preceding item) and a stimulus (for the succeeding item). After the last item is exposed, there is a brief rest (intertrial interval) and then the subject starts over again at the beginning of the list.

The SL task can be viewed as having two distinct parts: learning and integrating the items as responses, and learning to arrange these items in the correct serial order. Factors affecting response learning have corresponding effects on the rate of SL. Thus, more meaningful lists of items are learned faster in SL and prefamiliarization with the items facilitates later SL. The studies to be reviewed are concerned more with the ordering task in SL and the factors that affect it. Under this label we will review work on remote associations, the serial position curve, and the effective stimulus for the responses in SL. These are interesting phenomena unique to the SL arrangement and have stimulated much research.

We begin with the phenomena of remote associations since they lead into the other topics. The phrase "remote associations" refers to list responses that occur in the wrong position—an example of labeling a phenomenon by the theoretical concept used to explain it. Such intrusion errors are classified as to their degree of remoteness. Bugelski (1950) reported that anticipatory errors of *n* degrees of remoteness decline exponentially with *n*. One can identify several variables that produce such errors of ordering, with intralist similarity and preestablished interitem associations being the more prominent ones. Following the analysis of Gibson (1940), if two items at separate list positions are similar, then intrusion errors will result because of confusing them when they act as stimuli and when they act as responses. Preestablished interitem associations are obviously important sources of some intrusion errors; thus, in the word series bread-chair-butter . . . , the preestablished association bread-butter will interfere with the first-order association bread-chair which is to be learned. But remote associations are also in evidence and follow certain laws even for list materials that have no formal similarity or preestablished interitem associations. Hence, additional factors inherent in the SL task must be identified as the causes of remote intrusion errors in such lists.

One of the interesting facts about SL is that the rate of learning an item depends upon its position in the serial list. Figure 5-12 shows a characteristic serial position error curve; items at the beginning and end of the list are learned fastest, while items just beyond the middle are the most difficult. The relative shape of such serial position error curves seems to be invariant over changes in certain variables that effect overall learning rate on the list, e.g., meaningfulness, presentation rate, intertrial interval (McCrary and Hunter, 1953). The absolute number of errors varies with such factors, but the relative percentage of all errors that are attributable to errors at each position yields pretty much the same curve. Because of the appealing simplicity of this invariance, a number of theorists have tried their hand at explaining it. Some of these efforts will be touched upon in the subsequent discussion.

THE EFFECTIVE STIMULUS IN SERIAL LEARNING Theoretical analyses of serial learning have proceeded on the intuitively natural assumption that the effective stimulus for the subject to emit, say, item 7 as a response is the stimulus complex consisting of the preceding item 6 and possibly traces from more remote items 5, 4, ... This has been called the "specificity" hypothesis. Because of the obvious face validity of the assumption, it has gone essentially unquestioned for many years. Consequently, the results of some recent experiments by Young (1959, 1961) came as a jar to these preconceptions. Young noted that the specificity hypothesis predicts high positive transfer from SL to a PA task in which pairs are adjacent items from the serial list. After learning the serial list ABCDE, subjects were trained on a PA list containing the pairs A–B, B–C, C–D, D–E presented in the random order characteristic of PAL. Young found very little of the expected high positive transfer. Some positive transfer was evident early in PA learning, but it disappeared so that experimental and control groups reached criterion on the PA task in about the same number of trials. A related experiment (Young, 1962) assessed transfer to a PA task in which the stimuli were the two preceding items of the prelearned serial list, i.e., AB–C, BC–D, etc. Again there was no evidence for positive transfer because of learning these items in the prior serial learning task. Thus, a simple prediction from the specificity hypothesis was disconfirmed.

An alternative view of SL identifies the position

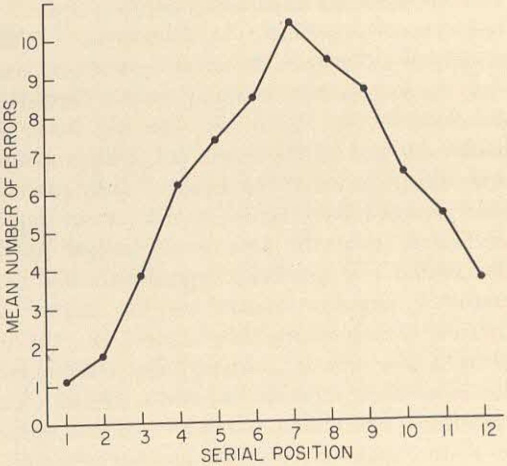


Fig. 5-12. Errors at each serial position in the learning of a twelve-item serial list of nonsense syllables to a criterion of mastery. The score is actually the average number of trials on which the item was not given correctly and includes failures to respond. (Hovland, 1938)

of an item in a temporal series as the effective cue for that item. Call this the "position" hypothesis. Serial position can be identified with some accuracy (Schulz, 1955); in fact, after SL, errors in identifying the correct position of an item show the typical bowed serial position curve. Initially, the discrimination may be rough, in terms of whether an item is near the beginning, middle, or end of the list. Later the discrimination may be sharpened by actually labeling the items by their ordinal position in the series.

Ebenholtz (1963*a*) has further evidence against the specificity hypothesis and favoring the position hypothesis. In one experiment, items occurred in a fixed sequence but not in fixed ordinal positions. A list can be viewed as a continuous cycle without beginning or end. Each trial was started at a different place in the cycle, and the subject went through the successive items of the cycle until he came back to the starting point of that trial. With this method of random starting points, subjects took about twice as long to learn as when item positions were fixed as in the conventional method of presentation. The random-starting-point procedure forces subjects to learn via sequential associations; fixed starting points permit the use of serial position cues as well. It is of interest to note that many of the subjects reported that they learned the cycled list by selecting a particular item as an "anchor point" or subjective beginning of the list, and then learned items around that anchor point.

A second experiment by Ebenholtz (1963*a*) investigated transfer between two successive serial lists, the second list containing several items from the first. It was found that the old items are readily learned in the second list if they retained their old positions in the series; if their positions were changed, then the old items were not learned any faster than the new items unique to the second list. The specificity hypothesis would have predicted negative transfer on the second list because of associative interference, i.e., the old items in the new list, having been initially connected to their adjacent old items, should create difficulties when new responses (of the second list) must be connected to these same stimuli.

In a third experiment, Ebenholtz (1963*b*) obtained high positive transfer between a temporal serial task and a PA task in which the stimuli were spatial positions. By the PA method, subjects learned to associate verbal responses with position cues (ten small windows in a vertical column), these cues occurring in a random temporal order. Following this, they learned a temporal list of these verbal items in which the temporal position of an item corresponded to its spatial position in the prior PA task. For example, the second item in the temporal series was the one associated with the second spatial location in the random-presentation PA task. Nearly perfect transfer was obtained between this PA task and its corresponding temporal serial task.

An interesting datum reported by Ebenholtz was the spatial position error curve for the PA task with spatial locations as cues. This position error curve was very similar in form to the bowed-shape position curve obtained in temporal serial learning. That is, fewest errors occurred on the cues at the ends of the spatial continuum and most errors occurred on cues near the middle of the continuum. This fact suggests that the error curve in temporal serial learning is but one variety of position effects obtained whenever the stimuli are arranged along an ordered dimension. This idea has been developed formally by Murdock (1960*b*) in a paper dealing with the distinctiveness of stimuli varying along a single stimulus dimension. He reported experiments on PAL with stimuli ordered along a variety of continua and found that the end stimuli were easiest to learn and the middle stimuli in the series were the most difficult.

The conclusion from these studies by Ebenholtz and Murdock is that ordinal position is an effective stimulus in SL and further that the serial position error curve results from natural differences in the cue distinctiveness of the various serial positions. According to this view, "remote association" intrusion errors result from confusions in the location of the unlearned items in the series. If a subject learns a general location for an item before he learns its specific location, then intrusion errors of several steps remoteness would not be as likely as those of one or two steps remoteness. Thus, the gradient of remote association errors reported by Bugelski (1950) would be explained. The position cue hypothesis may also be extended to handle other results of SL. For example, it is usually found that lengthening the rest period between trials increases the rate of learning in SL. A possible explanation is that the longer break between trials enables the subject to identify more clearly the beginning and end terms of the series, thus aiding position learning. In the extreme case, when there is no break between the last and first items, positions cannot be identified and it is a difficult task to learn (Eysenck, 1959).

A middle ground on the specificity versus position-cue issue will probably prevail among experimenters in verbal learning. The possible cues for the response in serial learning may be its serial position, the prior item, or a cluster of several prior items. The job for continuing research is to determine the conditions under which these cues take on different relative weights in the complex stimulus pattern that controls serial responses. It is highly probable that the relative weights of these various cues will depend on the nature of the verbal items composing the serial list. For example, if the items are very similar to one another, then cues from prior items are not distinctive and this may increase the relative weight of position cues.

An alternative theory of the serial position effect has been proposed recently, apparently independently, by Jensen (1962*a*) and by Feigenbaum and Simon (1962), and the resultant ordering of the data is attractive. Jensen's exposition will be outlined in the following. He assumes that the item learned first is usually the one to which the subject first attends, or in fact the first item in the list. This first learned item then serves as an "anchor point" for the remainder of the list. It is assumed further that the subject learns most readily by attaching new responses to previously learned responses. This implies that items are learned around the anchor point, both in a forward and backward direction. The order of learning is illustrated below with a series of nine items being folded around the anchor point (item 1).

Serial position:	6	7	8	9	1	2	3	4	5
Order of learning:	9	7	5	3	1	2	4	6	8

In terms of the order of learning, from first to last, the predicted order is items 1, 2, 9, 3, 8, 4, 7, 5, 6. The rule for generating the predicted series for any length list is to start with the first two items and then alternate successive terms in from the end of the series and away from the beginning of the series. This notion for predicting serial position difficulty appears to be quite valid; the average correlation between rank of predicted position difficulty and obtained errors was about .97 over some 70 serial position error curves that Jensen collated from the experimental literature. For example, for the Hovland data (1938) shown in Fig. 5-12, Jensen's rule predicts the rank of error scores with only one slight misordering (items 6 and 9 are reversed from predicted). The fit of the theory is about as high as the reliability of the serial position curves obtained in different studies.

Jensen takes this theory several steps further. Given that the items are going to be learned in a particular order, assume that once the subject arrives at the point where he is to learn the *n*th item, the time it takes him is constant. Suppose further that each item is learned in one trial in an all-or-none fashion. Then the proportion of the total errors contributed by a given position should be equal to the rank order in which that position is learned divided by $1 + 2 + 3 + \ldots + N$, where N is the number of items in the series. An outcome of this idea is that the shape of the relative percentage error curve (over serial positions) is invariant over changes in the overall learning rate on the list. This accords with the observation of McCrary and Hunter (1953). The predictions of the relative error percentages by this hypothesis corresponds closely to the data reported by McCrary and Hunter (cf. Feigenbaum and Simon, 1962).

There are several parts of this argument that require empirical confirmation: One is the assertion that an item is learned in a one-trial, all-or-none fashion; the other is the assertion about constant increments in error percentages. We defer the discussion of evidence for one-trial learning. Jensen includes several tests of the equal-increment prediction in his paper. Figure 5-13 reproduces one of these sets of results. The empirical order in which items are learned is used to construct the rank of scores on the abscissa. The straight line is an a priori prediction from the hypothesis and involves no arbitrary constants. The data are fit quite well by the straight-line relationship.

Jensen (1962*b*) has data to show that the straight-line relationship in Fig. 5-13 is relatively unaffected by certain variables that disturb the customary order in which items are learned. For example, a novel or unusual item placed in the series will be learned faster than neighboring items—a von Restorff effect. In Jensen's view, the novel item comes to serve as another anchor point in the list, and other items are learned around it. Although the novel item modifies the

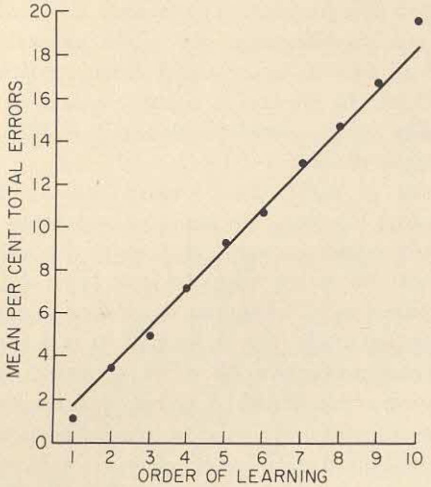


Fig. 5-13. Mean percentage of errors on each item plotted in the order in which each item in a ten-item serial list was learned to a criterion of one perfect trial on the list. The straight line is the prediction from the constant-increments assumption. (Jensen, 1962*a*)

order in which items are learned, there is little benefit to learning the entire list; certain items are simply interchanged from their customary order of learning. The relative percentage errors on an item remains a linear function of its rank order of learning. Other experiments (cf. McGeogh and Irion, 1952) have altered the customary serial position curve by instructing the subjects to attend and work first on various subsections of the list. The anchor point in the series can be readily modified by such instructions and, hence, the serial position curve is altered. Although such results are interpretable in Jensen's theory, Murdock's theory of position distinctiveness would seem not to allow for this alteration of the serial position curve by different instructions to the subjects.

A second assumption of Jensen's theory is that each serial item is learned in an all-or-nothing fashion. That is, the learning of an item is not a continuous, progressive process that grows over trials. No increment in associative strength to an item occurs until its neighboring items are learned. At the point where the neighboring items are learned, the subject begins work on learning the given item and then learns it in all-or-nothing fashion (cf. the one-element model in Fig. 5-9*a*). The evidence for this position is fragmentary at the moment. One supporting fact is presented in Fig. 5-14, taken from Jensen's paper (1962*a*). The curve shows, for the last item learned, the percentage correct responses over trials preceding the last error on that item. The striking feature of the estimates in Fig. 5-14 is the absence of any improvement in performance prior to the trial on which the last error occurred on the item. In the task yielding the data in Fig. 5-14, the responses were known in advance so that subjects could guess correctly with a certain low percentage (28 per cent in fact). Jensen plotted backward curves for each of the nine positions in his serial list, and each of the curves resembled the graph in Fig. 5-14. Also, the chance probability of a correct response prior to learning was about the same at the nine serial positions. Thus, there was no improvement in performance at a given position up until the trial of the last error at that position. The result probably would not be obtained with lists in which the items must be learned as integrated responses during the experiment. But such data are not particularly relevant to the ordering issue at hand.

The assumption of Jensen's theory is that the associative strength of certain items in the middle of the list is not built up over all trials, but only after certain other items (at the beginning and end) are learned. Bolles (1959) has reported some evidence confirming this assumption. About one-third of the way through the course of learning a nine-item list of nonsense syllables, the subjects had the fourth and sixth items interchanged in the list. If these items had acquired associative connections to their neighboring items, then at least four connections (involving items 4 and 6 as stimulus and response terms) would now be wrong and would interfere with the subsequent course of list acquisition. On the other hand, if these middle items had developed no connections over the early trials, then the interchange should result in no interference in subsequent learning. Bolles' result supported the later prediction. Trials to the criterion of list mastery were nearly identical for the interchange condition and for the control (noninterchange) condition. Also, the average errors made at the fourth and sixth positions (and their neighbors) were about the same for the two conditions. Bolles replicated this result in a second experiment and added a third condition in which the fourth and sixth syllables were replaced by new ones about one-third of the

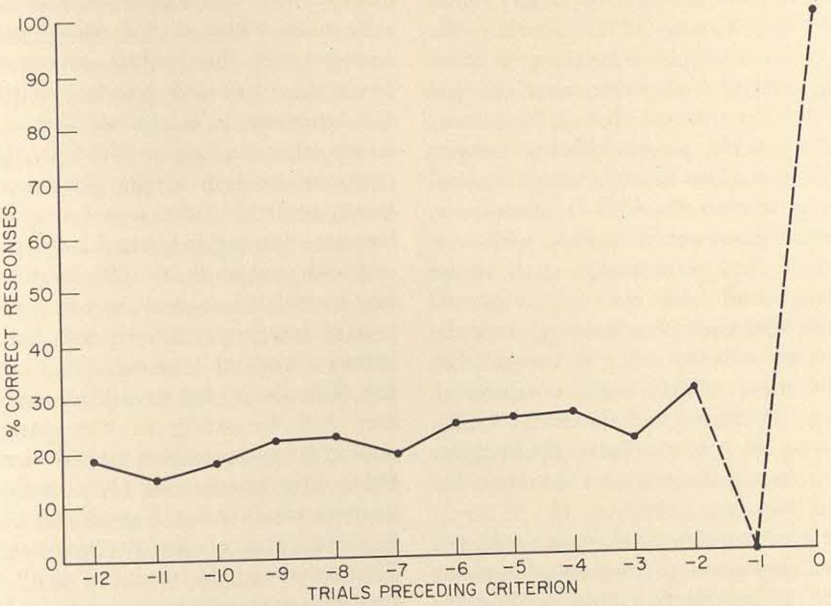


Fig. 5-14. A backwards learning curve for the last item learned, showing the percentage of correct responses among 200 subjects on each of the twelve-trials preceding the criterion of learning this item. The chance level of guessing correctly, estimated from other subjects with a changing list, was about 28 per cent. (Jensen, 1962*a*)

way through learning. The introduction of new items at these positions at that point in learning did not retard subsequent learning of the list. In fact, the lists with replaced or interchanged items were learned slightly faster than the control list. Jensen (1963) has replicated the essentials of these results. Thus, the results support the view that associative strength of the items in the middle of the list is not acquired steadily over all trials. These results by Jensen and Bolles challenge the traditional view that serial position learning is a gradual, continuous process. Alternatively, they encourage the development of simpler all-or-none models for serial learning.

LONG-TERM RETENTION AND FORGETTING

The common fact of forgetting is well known. In the laboratory setting, the amount forgotten is calculated as the difference in performance on the last trial of initial learning and on a later test for recall. An alternative method measures retention by how easily the material is relearned. If material initially required ten trials to be learned and later was relearned in four trials, then a 60 per cent savings on the second test was produced by the prior learning. The question in theoretical analyses of forgetting is, how are we to conceptualize what happens during the retention interval that produces the performance decrement? The answers to this question point to two kinds of factors: change over the retention interval in the background, contextual stimuli that supported the prior performance, and changes in the strengths of competing responses to the experimental stimuli resulting from interpolated learning or spontaneous recovery of old associations established prior to the learning of the new association.

There is sufficient positive evidence concerning the decremental effect on performance of changes in contextual cues. McGeogh and Irion (1952) have reviewed research showing the supportive role for recall of seemingly irrelevant background cues. The fluctuation theory of Estes (1955) supposes that uncontrolled changes constantly occur in the contextual cues, leading to performance decrements classified as forgetting. Irion (1948, 1949) proposed that the subject's performance "set" with respect to the task changed during a retention interval. The set includes attitudes, postural adjustment, and the direction of attention. An intriguing possibility is to identify Estes' idea of fluctuating background stimuli with Irion's notion of slowly changing performance sets. Evidence has accumulated (e.g., Irion, 1949; Irion and Wham, 1951; Thune, 1951) showing the beneficial effect on recall of reinstating by some "warming-up" activity the performance set lost during the retention interval. Irion's hypotheses also explain the typically poor correlation between first-test recall scores and savings scores derived from relearning. First-trial recall is often poor, but the material is relearned quickly with considerable savings. The performance set is absent before the first recall trial, but it is reinstated during the first few trials of relearning, once the subject "gets back into the swing of things." The research on warm-up effects is not unequivocal, however (e.g., Rockway and Duncan, 1952), and explanations for a few negative findings are lacking. The relevant literature on the topic has been reviewed by Adams (1961).

By far the most experimental work on forgetting has tested the notion of response competition (interference) as it affects retention. Postman (1962) has provided a good review of the evidence on this interference idea. McGeogh (1942) gave an early explicit statement of interference theory, and it is eminently simple and direct. If a subject has learned the stimulus-response connection A-B, then subsequent learning of the connection A-C will interfere with recall of B as a response to A. In McGeogh's version, the interpolated learning of A-C was supposed not to alter the strength of the prior connection A-B. Response B to A became less probable simply because a strong habit had been developed to a competing response, C. The forgetting of laboratory materials was presumed to result from interpolated learning occurring with similar materials between original learning (OL) and the recall test. This theory led to explicit study of competition and forgetting in the paradigms called retroactive and proactive interference experiments. Retroactive interference (RI) refers to the decrement in recall of list A produced by interpolated learning (IL) of list B. Proactive interference (PI) refers to a decrement in recall of a reference list because of prior learning of a similar list.

Not surprisingly, the major results from interference experiments are explained easily by the competition of response theory. The recall of A-B

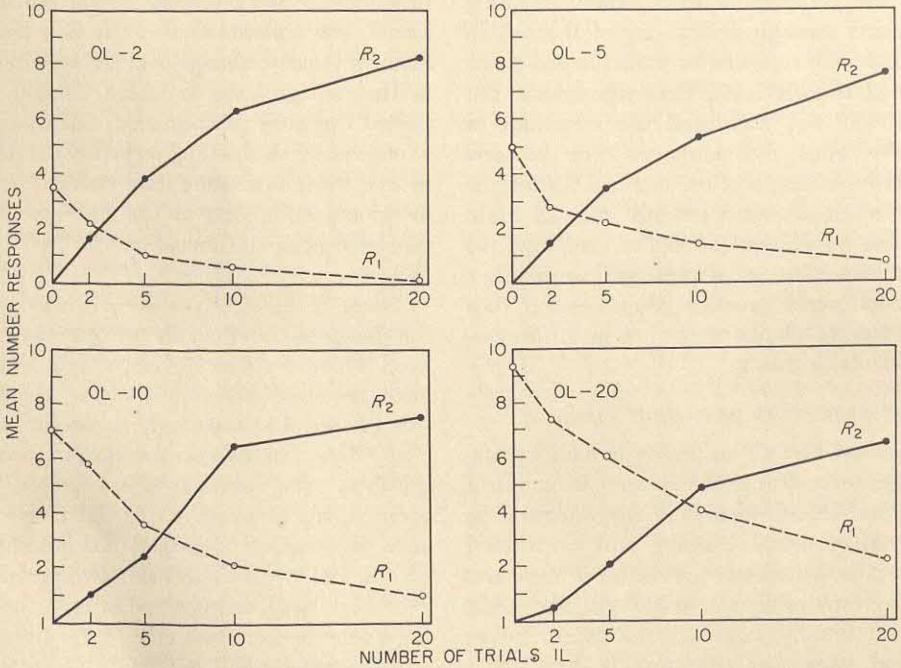


Fig. 5-15. Relative response frequencies of the originally learned response (R_1) and the newly learned response (R_2) during learning of the new response as measured by modified free recall. The four graphs come from four different groups of subjects given 2, 5, 10, or 20 trials of original learning. (Briggs, 1957)

increases with the amount of OL on A-B and decreases with amount of IL on A-C (cf. a review by Slamecka and Ceraso, 1960). Recall of an initial list is poorer the greater the number of similiar lists interpolated before recall of the first list (Underwood, 1945). Retroactive interference increases with the similarity of the stimuli in OL and IL, being greatest when the stimuli are identical. Changes in background contextual factors between OL and IL result in less RI when recall of the first list is carried out in the same context as initial learning (Bilodeau and Schlosberg, 1951; Greenspoon and Ranyard, 1957). These results are sensible in light of the response competition theory.

Briggs has conducted some experiments which permit assessment of the relative strengths of the old and the new habits during the course of interpolated learning. Using an A-R_1, A-R_2 paradigm, Briggs (1957) trained subjects for varying numbers of trials on OL followed immediately by IL with the same stimuli but new responses. At various points during IL, modified free recall tests were given with the stimuli. On such tests the subjects were instructed to give whatever response first came to mind, and there was no feedback from the experimenter to indicate which response was wanted. The curves in Fig. 5-15 give a graphic description of the frequency of the new (R_2) and old (R_1) responses after varying numbers of OL and IL trials. At the beginning of IL, the frequency of R_1 depended directly upon the number of OL trials. During the course of IL, R_1 decreased in frequency while R_2 increased. In an earlier study, Briggs (1954) investigated the change in R_1 and R_2 frequencies over various time intervals following IL. At the end of IL, the R_2 frequency was high and R_1 low, as in Fig. 5-15. However, with the passage of time, the relative frequency of R_2 declined and that for R_1 increased; after an interval of several days, R_1 and R_2 occurred with about equal frequencies on the modified free recall test.

One implication of the competition of response theory is that the amount of RI should covary directly with the number of intrusions of responses from the interpolated lists; that is, forgetting is the result of such intrusions. However, the correlation is frequently lacking. Melton and Irwin (1940) found, for example, that although RI increased with amount of IL, the number of intrusion errors increased then decreased with larger amounts of IL. At higher degrees of IL, the subjects were simply unable to give a response when trying to recall those from the first list. Melton and Irwin suggested that first-list responses are unlearned (extinguished) during the course of IL; thus, the greater unavailability of first-list responses with higher degrees of IL is explained. The unlearning was presumed to result from the nonreinforced intrusions of first-list responses during IL. Alternately, one might view it as counterconditioning; that is, acquisition of a new response automatically extinguishes an old response to that stimulus complex.

There is some direct evidence for this unlearning of first-list responses. The experiment by Briggs (Fig. 5-15) is not definitive since only one response was requested, and the decreasing frequency of R_1 during IL could result simply from more successful competition of R_2 responses. An experiment by Barnes and Underwood (1959) assessed the availability of both R_1 and R_2 during IL. After initial PA learning, subjects received IL with the same stimuli but different responses. On the modified recall test given immediately after the IL trials, the subject was asked to give both responses to each stimulus and to identify which response came from which list. Their subjects could frequently recall both responses and could identify accurately the list membership of

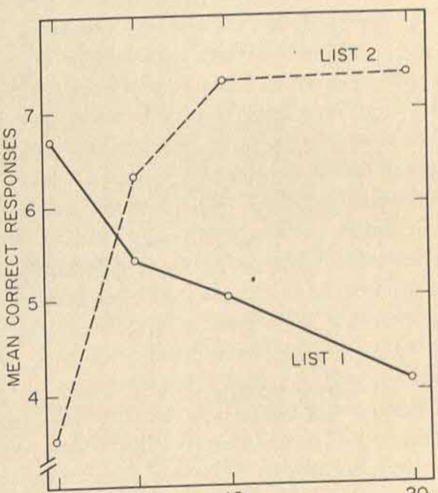


Fig. 5-16. Mean number of responses recalled and correctly identified with stimulus and list with the A-R_1, A-R_2 paradigm. Eight is the maximum possible score in each case. (Barnes and Underwood, 1959)

the recalled responses. Over a longer recall interval one would expect list identification of the responses to become less accurate. The results of primary interest are the recall curves of R_1 and R_2 over the course of IL. These results are shown in Fig. 5-16. As these results show, although the subject tries to recall both R_1 and R_2, the old R_1 responses become increasingly unavailable as IL progresses. The decline in R_1 is not simply due to the greater passage of time with more IL trials. A control group had OL to criterion, then rested for a time equivalent to that required for twenty IL trials; a recall test then yielded a mean recall of 7.75 R_1 responses, better than R_1 recall for the groups given any amount of IL. Thus, the R_1 responses seem to be unlearned or counterconditioned during the course of IL. Adams (1961) has replicated these findings. In addition, she measured the retention of both list responses after delays of two and seven days and found evidence for the spontaneous recovery of first-list responses over time following IL.

The pattern of findings in the Briggs, Barnes and Underwood, and Adams studies may be interpreted within the stimulus fluctuation theory which the writer favors. The fluctuation theory is easily extended to take into account the period of interpolated learning of incompatible responses; the form of the extension is given only schematically here (cf. Estes, 1959). The relevant information is shown in Fig. 5-17 which represents within a pictorial model the relevant stages of conditioning of a single stimulus item in a retroactive interference experiment.

The population of stimulus events identified with successive presentations of the experimental stimulus is represented pictorially by a large circle. The division at the middle divides the population into those stimulus elements that are temporarily available (S) for sampling and conditioning and those elements temporarily unavailable (S′). For simplicity of exposition, we suppose that each stimulus element is connected either to response 1 from the first list (open dots) or to response 2 from the second list (filled dots). Performance at any time is determined by the connections of the constituent elements in the available set, S. At the end of original learning which is carried to a stringent criterion, the elements are connected to R_1. During interpolated learning, the elements in

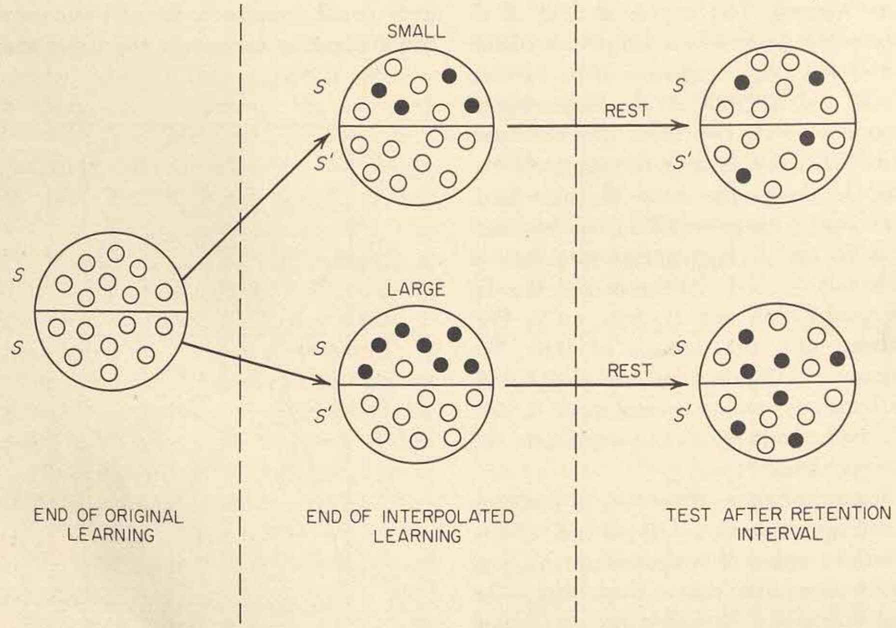


Fig. 5-17. Representation of the stimulus fluctuation model. Open dots represent stimulus elements connected to the first-list response; filled dots are elements connected to the second response learned during interpolated practice. The top figure represents the situation after a small amount of interpolated training; the bottom figure represents the situation after a larger amount of interpolated training.

the available set are counterconditioned, becoming connected to R_2. Figure 5-17 shows two cases in which learning on the interpolated list is carried out for few or for many trials, thus counterconditioning few or many elements of the temporarily available set of elements that were formerly connected to R_1. However, notice that elements that are unavailable during interpolated learning remain connected to R_1. If subjects are given modified free recall tests (Briggs, 1954) immediately after IL, then with a small degree of IL they will recall more R_1 than R_2 responses; with a high degree of IL, they will recall more R_2 than R_1 responses because the former R_1 elements in the available set have been counterconditioned to R_2.

Now imagine that a rest interval is imposed between the end of IL and a later test for modified free recall. During the rest interval, the process of stimulus fluctuation results in some interchange of elements between S and S′, the amount of change in the constituency of the available set, S, increasing exponentially over time. The change is such as to bring the proportions of R_1 elements in S and S′ towards equilibrium; that is, the proportion of R_1 elements in S tends over time to approach the overall proportion of R_1 elements in the entire population of elements. Thus, at the delayed free recall test, the probability of R_2 will have declined because of the loss from the available set of elements connected to R_2. Similarly, the probability of R_1 will have increased because of the influx into the available set of stimulus elements connected to R_1. These R_1 elements are those that were "saved" from counterconditioning because they were unavailable during interpolated learning. Briggs' results (1954) show this exponential decline in R_2 probability and the exponential rise in R_1 probability over the temporal course of a retention interval. The forgetting of R_2 and the spontaneous recovery of R_1 both follow from Eq. 1. If one is interested in R_1 probability, then we set $p_o = 0$ and $p_o' = 1$ in Eq. 1 and $p(t)$ describes the course of spontaneous recovery of R_1. Similarly, if we set $p_o = 1$ and $p_o' = 0$ in Eq. 1, then $p(t)$ describes the course of forgetting of R_2, the response acquired in interpolated learning. The model can be extended in a simple way to handle the Barnes and Underwood (1959) data, where the subject tries to give two responses to each test stimulus.

A simple corollary of the temporal changes in R_1 and R_2 probabilities is that the relative amounts of RI and PI should change systematically over time since the end of IL. Immediately following IL, RI should be strongest and PI negligible. However, as time passes since the end of IL, RI should decline and PI increase, so that eventually there is little difference in their magnitudes. Underwood (1948) has reported results supporting this corollary.

FORGETTING WITHOUT EXPLICIT INTERPOLATED LEARNING Early estimates of the amount forgotten without explicit IL were rather large, ranging around 60 to 75 per cent forgetting of an experimental list over one day. Such results came from experimental designs which used each subject repeatedly in a variety of learning-forgetting conditions. This massive forgetting put an interference theorist in an uncomfortable position. In some way, he had to claim, a good deal of interfering learning had gone on over the twenty-four hours to account for the drastic drop in retention. The argument lacked conviction since it was difficult to see what a person learns in everyday life that could interfere with the retention of nonsense material.

This uncomfortable perplexity began to dissolve, however, when investigators began using each of their naive subjects in only a single list-forgetting condition. It is now clear that a naive subject will retain about 75 to 80 per cent of a list (of words or nonsense) over a 24-hour interval. These facts were collated by Underwood (1957), who showed that the massive forgetting resulted from the procedure of using a subject repeatedly in a variety of list-forgetting conditions. Such conditions lead to an accumulation of proactive interference. Collating the studies in the literature, Underwood showed that the percentage of material retained after twenty-four hours was a strict decreasing function of the number of prior lists learned by the subjects. The average percentage retention of a single PA list was about 75 per cent one day following learning to a criterion of one perfect recitation. Allowing for the normal post-criterion drop (to 90 per cent, say), then a retention loss of only about 15 per cent has to be accounted for. The quantity seems more within the range that could be handled by the notions of interference from casual interpolated learning and fluctuations in contextual stimulation (warm-up) between training and test.

Underwood and Postman (1960) have attempted to identify and study some of the extraexperimental sources of interference that promote forgetting of materials subjects learn in the laboratory. They appeal to preestablished language habits or associations which are partially unlearned during the course of learning new experimental material. Practice and/or spontaneous recovery of these old habits during the retention interval is assumed to produce the retention loss. They identify two sources of extraexperimental habits that may enter in this way. One of these is letter-sequence habits. Thus, the consonant syllable JQB violates previously established letter-sequence habits since Q never follows J in natural language but all the vowels do, and most vowels would spell a word. Thus, during the learning of JQB as a response unit, certain previously learned letter-sequence habits would have to be extinguished. From this analysis, one would expect high-frequency trigrams to be better remembered (less interfered with) than low-frequency trigrams because learning of the latter units requires breaking common letter-sequence habits. Their results bear out this prediction. A second factor Underwood and Postman identify is unit-sequence habits, represented by interassociations between word units. To use their example, if the word *over* has a preestablished connection to *there,* then the connection would have to be unlearned in a serial list involving *over* and *there* as nonconsecutive items. If unlearning of the connection occurs during initial training, spontaneous recovery of the association will result in errors (forgetting) at recall. These considerations lead one to expect that a serial list of high-frequency words, having many associations, will be forgotten faster than a list of high-frequency trigrams that are not words (having few associations). Differences in this direction were obtained from relearning scores though not on the first-recall trial. These preliminary results provide an opening wedge for investigations of forgetting using materials with various relationships to the linguistic habits of the subjects.

A clear demonstration of sequential interference because of language habits has been provided by Coleman (1962). Coleman had his subjects attempt to recall a sequence of twenty-five letters they had studied briefly. The letters were scrambled initially; after studying them for a brief time, the subject was handed twenty-five cards bearing the twenty-five letters and asked to arrange them in the original serial order he had studied. The order reproduced by the first subject was recorded and given to a second subject to study and then recall. The sequence recalled by the second subject was given to a third subject, and so on, through eleven successive subjects. As the original sequence passed through each subject, it was rearranged little by little. The misorderings uniformly drifted towards the occurrence of letter sequences having higher frequencies in English than did the original random sequence. In a second experiment word-sequence interference was demonstrated by this successive reproduction method. An English sentence of twenty-four words was taken from a book, the word order scrambled, then given to the first subject to study. He then attempted to reconstruct the sequence of the words he had just studied. Coleman filtered such reproductions through sixteen successive subjects. The results leave little doubt that successive reproductions tended towards sensible English sentences. The example below shows the contrast between an original sequence and its sixteenth reproduction.

Original	*16th Recall*
about was good-looking way and treating made of that a him the quiet youngster nice he manners a them girls wild go with	he was a youngster nice quiet with manners good-looking and a way of treating them that made the girls go wild about him

(From Coleman, 1962)

One could not ask for a more direct demonstration of extraexperimental interference by language habits with the novel habits that one attempts to set up in the laboratory.

REINFORCEMENT IN VERBAL LEARNING

In human verbal learning, the primary effect promoting learning is knowledge of results or information about the correct response. When the subject is set to learn, those of his responses followed by the experimenter's announcement "Right" tend to be repeated; similarly, informing the subject of the correct response tends to increase the probability of that response. Some of the experimental material relating to these reinforcement operations is discussed in this section. It will be assumed throughout that the subject is motivated to learn the relevant materials. By this is meant that certain manipulations have been performed, usually via instructions, which orient the subject to the task, which initiate rehearsal of the material with intent to learn, which sustain performance throughout training, and which define the adequacy of his performance. This has been called an "intentional learning set." Another class of studies of incidental learning are concerned with the effects of deleting or altering important parts of the motivating instructions. Reviews of the material on incidental learning may be found in other sources (e.g., Postman and Sassenrath, 1961; Adams, 1957; McGeogh and Irion, 1952; Postman, 1947).

Reinforcement variables typically refer to those acting at the time an *S-R* item receives one trial of training. Considering the anticipation method of PAL, one can identify two classes of information events that are used following the subject's response to a given stimulus. One class of events are indicative only of the congruence between the subject's response and the correct one, e.g., right or wrong. The other class of operations evokes the correct response, e.g., presenting the unit which the subject reads aloud. The first method of providing information is generally applicable only when the response alternatives have been restricted in advance through instructions or background conditioning. An interesting counterexample to this uniform methodology is a PA experiment by Underwood and Schulz (1960), in which the subject was reinforced for any verbal response he made to a given stimulus.

A number of studies have investigated the effects of right (R) and wrong (W) given following a subject's responses when these are selected from a known set of r alternatives (e.g., the first r integers). One is interested in the probability that the subject will repeat the response to a given stimulus on the next trial. The repetition probability of connections called R or W is compared with an empirical baseline repetition probability obtained when the connection is followed by neither R nor W (i.e., nothing, N). This latter baseline is required since frequently the repetition probability following N is greater than the chance level of $1/r$. As would be expected, the probability of repeating a response to a stimulus which was followed by R is increased above the baseline repetition probability. However, evidence can be amassed (e.g., Thorndike, 1932; Buchwald, 1962) to indicate that in the usual PA situation W typically has only a small, often negligible, effect on repetition probability. The size of this effect is larger the less the number of response alternatives. Buchwald (1963) has provided a useful model which brings orderliness into an otherwise puzzling array of facts regarding the effects of R, W, and N. His model relies upon a neat distinction between the subject recalling his response (given last trial to a stimulus) and recalling the information (R, W, or N) given following his response to that stimulus. The two recall processes are assumed to be independent. Responses are learned by their contiguity to the stimulus. The main effect of information events (R, W, N), insofar as they also are recalled when the stimulus recurs, is to modify performance probabilities (of the response recalled, if any). By developing a model along these lines, Buchwald was able to predict numerically a variety of results with good accuracy.

TEMPORAL PARAMETERS IN PAIRED-ASSOCIATE LEARNING All theories of learning emphasize the necessity of temporal contiguity between the stimulus and response terms to be associated. Several experimental variables that affect learning rate in PAL can be understood in terms of the probability that one or another procedure promotes the necessary coincidence between the *S* and the *R* terms. Theories differ in whether contiguity is considered to be a sufficient basis for learning; for example, Thorndike (1932) and Hull (1943) supposed that a satisfier or positive reinforcer was required in addition to the basic *S-R* contiguity. Such differences are immaterial in the discussion which follows; that is, we examine some variables in PAL which theoretically affect only the likelihood that *S-R* contiguity occurs, and this latter event is assumed uniformly to be a necessary condition for learning.

If one considers the events that occur on a single trial with a PA item, several important time intervals can be identified. The first interval of some importance is that between stimulus presentation and the occasion for the subject's response. Suppose that the stimulus term is presented briefly, then removed, and the subject is prevented from rehearsing it by some immediate interfering activity, such as counting backwards. After a brief period, he is stopped and asked to give the appropriate response that goes with the stimulus shown a few seconds before. The procedure resembles the "delayed-response" method used for the study of immediate memory with animals. Although the study has not been done, there is little doubt that the results would be comparable to that found with animals; the longer the delay before the response is permitted, the poorer presumably will be the performance.

The presumed decrement in performance by this procedure would be ascribed to the subject forgetting the stimulus before he could respond. Since the interval affects the probability that the stimulus is available at the time for response, this interval would be considered as a factor affecting performance rather than learning. However, by a simple variant on the procedure, one may convert it into a variable affecting learning. Suppose training on the PA list is carried out by alternate study trials and no-information test trials with the stimuli alone. On study trials one may vary the time between the brief exposure of the stimulus and the presentation of the response term to be associated with that preceding stimulus. This variable is like the CS-US interval in Pavlovian studies of trace conditioning. Assume that a gain in associative strength occurs whenever the subject recalls the stimulus term at the time the response term is presented. The longer the delay interval, the less likely it is that a reinforcement (*S-R* coincidence) occurs. Presumably, then, learning will be slower the longer the interval separating presentation of the stimulus and the correct response term. This simple relation is yet to be tested, but it seems clear that it should be found.

A second time interval which may be examined by the anticipation method is the time between the subject's response and the presentation of information by the experimenter. Again, assume that the subject is engaged in some interpolated activity during the delay interval. The information presented by the experimenter may be of two types: one type aids rehearsal of the correct response (e.g., presenting the stimulus and correct response terms); the other does not reinstate the response (e.g., saying "right" or "wrong"). The effect of the response-to-information interval should depend on which procedure is adopted. By the latter method (saying *R* or *W*), the subject may well forget the response he gave, or the stimulus presented, or both, and in neither case will the information, *R* or *W*, be of any help to establishing the correct association. Saltzman (1951) introduced a 6-second delay by this method and found that it retarded the rate of verbal learning. Landsman and Turkewitz (1962) have reported similar results. This situation approximates closely conditions of delayed reward in animal studies. By contrast, if the stimulus and correct response term are presented together at a delay following the subject's response, there is little reason to expect that the delay would produce slower learning. The procedure ensures that each trial with a given item terminates with the stimulus and response terms in temporal coincidence; the delay between the subject's initial guess and the subsequent coincidence should be of no consequence.

A third time interval of importance in verbal learning is the duration of the simultaneous exposure of the stimulus and response terms, viz., the study time per pair. Taking an analog to animal studies, the variable may be analogous to the duration of consummatory activity used to reinforce the animal. Alternatively, it may be analogous to the number of contiguous *S-R* pairings per trial unit. In human learning, this study-time variable has an obvious effect on overall learning rates. Figure 5-18 below shows some results obtained by the writer, relating total errors before learning to the study time per item. Subjects learned sixteen syllable-number pairs by alternate study and test trials. On study trials, each *S-R*

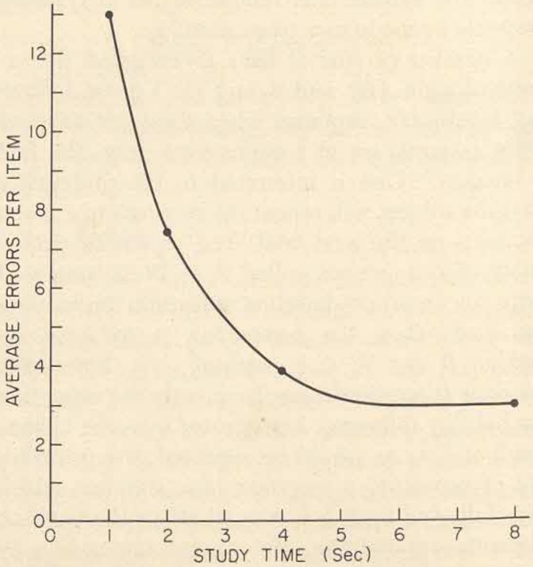


Fig. 5-18. Average errors per item related to the study time on the item. Unpublished results of Bower, 1960.

pair was presented for 1, 2, 4, or 8 seconds for different subjects; on nonreinforced test trials, each stimulus was shown for four seconds and the subject tried to supply the appropriate response. The measure plotted in Fig. 5-18 is the average errors per item per subject before a criterion of list mastery was attained. The results in Fig. 5-18 can be accounted for theoretically. Assume that the effect of increasing the study time is essentially to provide more "subtrials" of rehearsal for a given S-R pair. If each rehearsal has the same strengthening effect on the association, then the net learning rate for a study time of length t will be an exponentially increasing function of t (by Eqs. 2 and 3). Since total errors per item is proportional to the reciprocal of the net learning rate, the mean errors will be the reciprocal of an exponential increasing function. The relation describes the data in Fig. 5-18 reasonably well.

VARIATIONS IN DEGREE OF REINFORCEMENT If we identify reinforcement with information, then the amount of reinforcement will vary with the specificity of the information (given to the subject) about what is the correct response. The more specific the information, the more likely it is that the necessary stimulus-response connection will be rehearsed by the subject. Trowbridge and Cason (1932) varied the specificity of feedback information given to blindfolded subjects who were trying to draw 3-inch lines. After each response, some subjects were told nothing, some were told "right" or "wrong," and others were told the exact amount and direction of their error. The first group showed no improvement, the second group slight improvement, and the third group considerable improvement over trials in their ability to produce a 3-inch line.

Bower (1962*a*) has adapted a similar procedure to paired-associate learning. Suppose that the responses are drawn from a small set of known alternatives, such as the integers from 1 to 10, and to each stimulus the subject is to associate a particular number. Assume that when the subject says the correct number, he is told "right." Several training conditions can be devised to vary the probability that the correct response will be reinforced following trials on which the subject makes an error. The possible forms of information following an error might be as follows: (*a*) complete correction—"2 is correct," (*b*) noncorrection—"wrong," or (*c*) varying degrees of partial correction—"4 or 2 or 7 is correct." In the latter case of partial correction, the subject is instructed that only one of the numbers given is correct and the other two numbers are distractors (chosen at random), but there are no cues to distinguish the one correct answer. Partial correction can be in various degrees depending on the number of "possibly correct" responses told to the subject. These conditions form an obvious continuum, and they can be shown to produce graded changes in net learning rates. Learning is fastest under complete correction, slowest under noncorrection, and partial correction conditions are intermediate in learning rate. In an experiment comparing these conditions, Bower (1962*a*) showed that the results were predicted with quantitative accuracy by assuming, first, that the effect of saying R following a correct response was the same in all conditions, viz., the probability of the correct response following R increases by the equation

$$p_{n+1} = (1 - \theta)p_n + \theta. \tag{4}$$

The conditions differ in the operator applied to p_n following an error. With complete correction, the same operator as in Eq. 4 is applied to increase response probability. With partial correction, where the subject is told k possibly correct alternatives, the θ in Eq. 4 is replaced by θ/k to obtain the new response probability. With noncorrection, when "wrong" follows an error, the identity operator is applied, i.e., $p_{n+1} = p_n$. Using these assumptions, the results from the three training conditions were predicted accurately using a single common estimate of the basic learning rate, θ.

The reader may note that the variables discussed under this subheading of amount of reinforcement have not included variations in the amount of symbolic reward (e.g., money) provided the subject. Money or tokens symbolic of value correspond to what most people would mean by different amounts of reward. The obvious question is whether learning progresses faster the greater is the monetary payoff for correct responses and the monetary loss for errors. In the verbal learning context, the question has been investigated frequently and with almost uniformly discouraging results (e.g., Thorndike and Forlano, 1933; Rock, 1935; Eisenson, 1935). Typically, giving material incentives in addition to information (R, W) results in little, if any, increase in learning rate; in those cases in which a small effect of incentive conditions is obtained, the size of the effect does not increase with the size of the monetary payoff. With adult subjects, the motivation induced by the intentional-learning instructions apparently yields a rate of learning with simple information which is so high that the addition of special incentives has little influence.

One can arrange special experiments in which monetary payoffs have differential effects because they convey different amounts of information. A study by Keller, Cole, Burke, and Estes (1965) is exceptionally clear on this point. Their subjects learned a two-choice paired-associate task involving twenty-five different stimulus items. For each stimulus, there were two possible responses: one response was worth x points when chosen, the other y points, and $x > y$. The values for x and y were the possible combinations of 1, 2, 3, 4, or 5 points over different items in the series. The points were convertible into cash at the end of the experiment. Subjects were instructed to learn to choose that response to each stimulus which had the larger value. Two conditions were run: in the first, after the subject's response, the worth of both responses was shown; in the second, the subject was informed only of the points associated with the response he chose for that stimulus. The effect of payoff on the results was different for the two groups. With complete information, the rate of learning (to choose the larger-valued response) was not affected by the values of x or $x - y$ for an item; with incomplete information, learning rate on an item increased directly with the number of points, x, for the larger alternative and with the difference in points, $x - y$. This difference in outcomes may be interpreted in terms of the probability that the two conditions lead to identification and rehearsal of the "correct" response by the subject. This identification requires some comparison between x and y. In the complete information condition x and y are shown, so identification and rehearsal of the higher alternative occurs on each trial after the subject's choice. In contrast, the comparison is difficult when the subject is informed only of the points associated with the chosen alternative. Either he must remember the points associated with the other alternative or he must make some judgment of the likelihood that the chosen alternative is the higher valued of the two. The probability of a favorable judgment (and hence reinforcement) when the correct response occurs will increase with x simply because an overall high value is more likely to be the local higher value in a set of two alternatives. The same reasoning carries through in explaining the influence of x-y upon the results in the noncorrection condition.

This information analysis of the experimenter's "reinforcement operations" leads us to consider how the subject interprets the various actions the experimenter takes conditional upon his (the subject's) responses. The effect of the subject's interpretation is most readily seen when one or more of the experimenter's actions are ambiguous (Estes and Johns, 1958). One factor affecting how the subject interprets an ambiguous experimenter action is the context of alternative experimenter actions in which this ambiguous one is embedded. Buchwald (1959*a*, 1959*b*, 1960) has shown that the nothing event, N, following responses can be varied in its effect on learning depending on whether N is alternated with R alone, with W alone, or with both R and W. The reinforcing value induced onto N (via the subject's interpretation) is opposite in sign to R or W, whichever occurs alternately with N. He obtained similar results with the ambiguous "mmhm" uttered by the experimenter following the subject's response.

The basic notion in these studies is that of changing the reinforcement value of a given event via its contrast with other reinforcing events occurring in the same context. There is supportive evidence for this notion from experiments on differential rewards with animals (Bower, 1961*b*; Crespi, 1942; Black, Adamson, and Bevan, 1961; Reynolds, 1962). Bevan and Adamson (1960) have stated the hypothesis concisely and have related it to Helson's theory of adaptation level which explains certain phenomena of psychophysical judgments (Helson, 1959). This theory supposes that a subject judges the apparent sensory magnitude of a stimulus by referring it to an internal norm or standard (the adaptation level or AL). Furthermore, the internal norm is derived, via an averaging process, from the background and the stimulus events that have occurred over the immediate past of the subject. Thus, the apparent or judged loudness of a 70-db tone will be lower when it occurs within a series of 90-db. tones than within a series of 50-db. tones. Applying this notion to a continuum of reinforcing stimuli (e.g., electric shocks), the apparent magnitude and, hence, reinforcing effect of a given intensity reinforcing stimulus will vary according to the alternative reinforcing stimuli occurring in the same context (which determine the AL).

Bevan and Adamson (1960) have conducted some preliminary experimental tests of this idea. In one of their experiments, the task for the subject was to learn to move a hand stylus through a 28-unit bolthead maze. At each of the twenty-eight choice points, there were two boltheads, one left and one right, and the subject made his choice by touching one of these with the stylus. If he touched the wrong one, he was given a brief shock of 2.3 ma, then corrected himself and went on to the next unit of the maze. The shock of 2.3 ma is the negative reinforcing stimulus in the study. Bevan and Adamson attempted to manipulate its value as a reinforcer by giving a prior series of 30 shocks of either 1.3, 2.3, or 3.3 ma to different groups of subjects. The prior shock series is presumed to establish an appropriate adaptation level, and hence should affect the apparent intensity of the 2.3 ma shock given during the subsequent maze training. For subjects adapted with 1.3 ma shocks, the maze shock of 2.3 ma would appear to be very strong, and hence they should quickly learn to eliminate errors. Conversely, subjects adapted with 3.3 ma shocks would perceive the 2.3 ma shock as relatively weak and should learn more slowly. Each subject was run five trials on the 28-unit maze, and the rate of reduction in errors over trials confirmed the predictions. Subjects adapted with 1.3 ma shocks learned fastest and those adapted with 3.3 ma shocks learned slowest.

This AL conception of magnitude of reinforcement seems a fruitful one and efforts should be made to extend it to other kinds of reinforcers with human subjects. Before the idea can be fairly tested with other reinforcement continua, it first would be necessary to show that AL-type effects occur when the subject makes psychophysical (category) judgments along the prospective continuum. The size of these contextual effects will probably depend on the type of continuum, whether physical (shock) or symbolic (approval), and the extent to which category judgments along the continuum have been overlearned in the subject's social environment.

REFERENCES

ADAMS, J. A. The second facet of forgetting: A review of warm-up decrement. *Psychol. Bull.*, 1961, **58**, 257-273.

ADAMS, J. K. Laboratory studies of behavior without awareness. *Psychol. Bull.*, 1957, **54**, 383-405.

ADAMS, S. Temporal changes in the strength of competing verbal associates. Unpublished doctoral dissertation. University of California, 1961.

ADELSON, M., MUCKLER, F. A., AND WILLIAMS, A. C., JR. Verbal learning and message variables related to amount of information. In H. Quastler (ed.), *Information Theory in Psychology*. Glencoe, Ill.: Free Press, 1955.

ASCH, S. E., CERASO, J., AND HEIMER, W. Perceptual conditions of association. *Psychol. Monogr.*, 1960, **74**, No. 490.

BARNES, J. M., AND UNDERWOOD, B. J. Fate of first-list associations in transfer theory. *J. exp. Psychol.*, 1959, **58**, 97-105.

BATTIG, W. F. Paired-associate learning under simultaneous repetition and nonrepetition conditions. *J. exp. Psychol.*, 1962, **64**, 87-93.

BEVAN, W., AND ADAMSON, R. Reinforcers and reinforcement: Their relation to maze performance. *J. exp. Psychol.*, 1960, **59**, 226-232.

BILODEAU, I. M., AND SCHLOSBERG, H. Similarity in stimulating conditions as a variable in retroactive inhibition. *J. exp. Psychol.*, 1951, **41**, 199-203.

BLACK, R., ADAMSON, R., AND BEVAN, W. Runway behavior as a function of apparent intensity of shock. *J. comp. physiol. Psychol.*, 1961, **54**, 270-274.

BOLLES, R. C. The effect of altering the middle of the list during serial learning. *Amer. J. Psychol.*, 1959, **72**, 577-580.

BORING, E. G. *A History of Experimental Psychology*. New York: Appleton-Century-Crofts, Inc., 1950.

BOUSFIELD, W. A., AND BARCLAY, W. D. The relation between order and frequency of occurrence of restricted associative responses. *J. exp. Psychol.*, 1950, **40**, 643-647.

BOWER, G. H. Application of a model to paired-associate learning. *Psychometrika*, 1961*a*, **26**, 255-280.

———. A contrast effect in differential conditioning. *J. exp. Psychol.*, 1961*b*, **62**, 196-199.

———. Application of the all-or-none conditioning model to the learning of compound responses. Tech. report No. 37, Psychology Series, Institute for mathematical studies in the social sciences. Stanford, Calif.: Stanford University, 1961*c*.

———. An association model for response and training variables in paired-associate learning. *Psychol. Rev.*, 1962*a*, **69**, 34-53.

———. Some experiments related to a learning model. Paper at Western Psychol. Assoc. meetings. March, 1962*b*.

———, AND THEIOS, J. A learning model for discrete performance levels. In R. C. Atkinson (ed.): *Studies in Mathematical Psychology.* Stanford: Stanford University Press, 1964.

———, AND TRABASSO, T. Concept identification. In Atkinson, R. C. (ed.): *Studies in Mathematical Psychology.* Stanford: Stanford University Press, 1964.

BRIGGS, G. E. Acquisition, extinction and recovery functions in retroactive inhibition. *J. exp. Psychol.,* 1954, **47**, 285-293.

———. Retroactive inhibition as a function of original and interpolated learning. *J. exp. Psychol.,* 1957, **53**, 60-67.

BROWN, J. Some tests of the decay theory of immediate memory. *Quart. J. exp. Psychol.,* 1958, **10**, 12-21.

BUCHWALD, A. M. Extinction after acquisition under different verbal reinforcement combinations. *J. exp. Psychol.,* 1959*a,* **57**, 43-48.

———. Experimental alterations in the effectiveness of verbal reinforcement combinations. *J. exp. Psychol.,* 1959*b,* **57**, 351-361.

———. Supplementary report: Alteration in the reinforcement value of a positive reinforcer. *J. exp. Psychol.,* 1960, **60**, 416-418.

———. Variations in the apparent effects of "Right" and "Wrong" on subsequent behavior. *J. verbal learning & verbal behavior,* 1962, **1**, 71-78.

———. A family of mathematical models for the effectiveness of *Right* and *Wrong* in altering behavior. Tech. report No. 2, Indiana University, USPHS grant, Aug. 1963.

BUGELSKI, B. R. A remote association explanation of the relative difficulty of learning nonsense syllables in a serial list. *J. exp. Psychol.,* 1950, **40**, 336-348.

BUSH, R. R., AND MOSTELLER, F. A mathematical model for simple learning. *Psychol. Rev.,* 1951, **58**, 313-323.

———, AND ———. *Stochastic Models for Learning.* New York: John Wiley & Sons, Inc., 1955.

BUSCHKE, H. Retention in immediate memory estimated without retrieval. *Science,* 1963, **140**, 56-57.

CLARK, L. L., LANSFORD, T. G., AND DALLENBACH, K. M. Repetition and associative learning. *Amer. J. Psychol.,* 1960, **73**, 22-40.

COLEMAN, E. B. Sequential interference demonstrated by serial reconstruction. *J. exp. Psychol.,* 1962, **64**, 46-51.

———. Approximations to English: some comments on the method. *Amer. J. Psychol.,* 1963, **76**, 239-247.

CONRAD, R. Errors in immediate memory. *Brit. J. Psychol.,* 1959, **50**, 349-359.

COTTEN, J. W. On making predictions from Hull's theory. *Psychol. Rev.,* 1955, **62**, 303-314.

CRESPI, L. P. Quantitative variation of incentive and performance in the white rat. *Amer. J. Psychol.,* 1942, **55**, 467-517.

CROTHERS, E. J. Paired-associate learning with compound responses. *J. verbal Learning & verbal Behavior,* 1962, **1**, 66-70.

DAVIS, R., SUTHERLAND, N. S., AND JUDD, B. R. Information content in recognition and recall. *J. exp. Psychol.,* 1961, **61**, 422-429.

DEESE, J., AND KAUFMAN, R. A. Serial effects in recall of unorganized and sequentially organized verbal material. *J. exp. Psychol.,* 1957, 54, 180-187.

DEESE, J. Influence of inter-item associative strength upon immediate free recall. *Psychol. Rep.,* 1959*a,* **5**, 305-312.

———. On the prediction of occurrence of particular verbal intrusions in immediate recall. *J. exp. Psychol.,* 1959*b,* **58**, 17-22.

EBBINGHAUS, H. *Über das Gedächtnis: Untersuchungen zur Experimentellen Psychologie.* Leipzig: Duncker and Humklot, 1885. (Translated as *Memory: A Contribution to Experimental Psychology* by H. A. Ruger and C. E. Bussenius. New York: Teachers College, Columbia University, 1913.)

EBENHOLTZ, S. The relative role of position learning and sequential associations in the serial learning process. Unpublished doctoral dissertation. New School for Social Research, 1961.

EBENHOLTZ, S. M. Position mediated transfer between serial learning and a spatial discrimination task. *J. exp. Psychol.,* 1963*a,* **65**, 603-608.

———. Serial learning: Position learning and sequential association. *J. exp. Psychol.,* 1963*b,* **66**, 353-362.

EGAN, J. P. Recognition memory and the operating characteristic. *Tech. Report,* Contract No. AF 19(604)-1962. Indiana University, June, 1958.

EISENSON, J. Confirmation and information in reward and punishment. *Arch. Psychol., N.Y.,* 1935, **27**, No. 181.

EPSTEIN, W., ROCK, I., AND ZUCKERMAN, C. B. Meaning and familiarity in associative learning. *Psychol. Monogr.,* 1960, **74**, No. 491.

ERICKSON, R. L. Relational isolation as a means of producing the von Restorff effect in paired-associate learning. *J. exp. Psychol.,* 1963, **66**, 111-119.

ESTES, W. K. Toward a statistical theory of learning. *Psychol. Rev.,* 1950, **57**, 94-107.

———. Statistical theory of spontaneous recovery and regression. *Psychol. Rev.*, 1955, **62**, 145-154.

———. The statistical approach to learning theory. In Koch, S. (ed.), *Psychology: A study of a Science.* Vol. II. New York: McGraw-Hill Book Company, Inc., 1959*a*.

———. Component and pattern models with Markovian interpretations. In Bush, R. R., and Estes, W. K. (eds.): *Studies in Mathematical Learning Theory.* Stanford, Calif.: Stanford University Press, 1959*b*.

———. Learning theory and the new "mental chemistry." *Psychol. Rev.*, 1960, **67**, 207-223.

———. New developments in statistical behavior theory: differential tests of axioms for associative learning. *Psychometrika,* 1961, **26**, 73-84.

———, and Burke, C. J. A theory of stimulus variability in learning. *Psychol. Rev.*, 1953, **60**, 276-286.

———, Hopkins, B. L., and Crothers, E. J. All-or-none and conservation effects in the learning and retention of paired associates. *J. exp. Psychol.*, 1960, **60**, 329-339.

———, and Johns, M. D. Probability learning with ambiguity in the reinforcing stimulus. *Amer. J. Psychol.*, 1958, **71**, 219-228.

Eysenck, H. W. Serial position effects in nonsense syllable learning as a function of interlist rest pauses. *Brit. J. Psychol.*, 1959, **50**, 360-362.

Feigenbaum, E. A. The simulation of verbal learning behavior. *Proc. West. Joint Computer Conf.*, 1961, **19**, 121-132.

———, and Simon, H. A. A theory of the serial position effect. *Brit. J. Psychol.*, 1962, **53**, 307-320.

Garner, W. R. *Uncertainty and Structure as Psychological Concepts.* New York: John Wiley & Sons, Inc., 1962.

Gibson, E. J. A systematic application of the concepts of generalization and differentiation to verbal learning. *Psychol. Rev.*, 1940, **47**, 196-229.

———. Intralist generalization as a factor in verbal learning. *J. exp. Psychol.*, 1942, **30**, 185-200.

Gibson, J. J., and Gibson, E. J. Perceptual learning: differentiation or enrichment? *Psychol. Rev.*, 1955, **62**, 32-41.

Goss, A. E. Transfer as a function of type and amount of preliminary experience with task stimuli. *J. exp. Psychol.*, 1953, **46**, 419-428.

Greenspoon, J., and Raynard, R. Stimulus conditions and retroactive inhibition. *J. exp. Psychol.*, 1957, **53**, 55-59.

Haber, R. N. The effects of encoding on perceptual memory. *Amer. Psychologist,* 1963, **18**, 472 (abstract).

Hake, H. W., and Erickson, C. W. Role of response variable in recognition and identification of complex visual forms. *J. exp. Psychol.*, 1956, **52**, 235-243.

Harris, C. S., and Haber, R. N. Selective attention and coding in visual perception. *J. exp. Psychol.*, 1963, **65**, 328-333.

Hayes, K. J. The backward curve: a method for the study of learning. *Psychol. Rev.*, 1953, **60**, 269-275.

Hellyer, S. Supplementary report: Frequency of stimulus presentation and short-term decrement in recall. *J. exp. Psychol.*, 1962, **64**, 650.

Helson, H. Adaptation-level theory. In S. Koch (ed.) *Psychology: A Study of a Science.* Vol. I. New York: McGraw-Hill Book Company, Inc., 1959, pp. 565-621.

Horowitz, L. M. Free recall and ordering of trigrams. *J. exp. Psychol.*, 1961, **62**, 51-57.

———. Associative matching and intralist similarity. *Psychol. Rep.*, 1962, **10**, 751-757.

Hovland, C. I. Experimental studies in rote-learning theory; III. Distribution of practice with varying speeds of syllable presentation. *J. exp. Psychol.*, 1938, **23**, 172-190.

Hull, C. L. *Principles of Behavior.* New York: Appleton-Crofts, 1943.

Irion, A. L. The relation of "set" to retention. *Psychol. Rev.*, 1948, **55**, 336-341.

———. Retention and warming-up effects in paired associate learning. *J. exp. Psychol.*, 1949, **39**, 669-675.

———. Rote learning. In Koch, S. (ed.) *Psychology: A Study of a Science.* Vol. 2. New York: McGraw-Hill Book Company, Inc., 1959.

———, and Wham, D. S. Recovery from retention loss as a function of amount of pre-recall warming-up. *J. exp. Psychol.*, 1951, **41**, 242-246.

Jensen, A. R. An empirical theory of the serial-position effect. *J. Psychol.*, 1962*a*, **53**, 127-142.

———. The von Restorff isolation effect with minimal response learning. *J. exp. Psychol.*, 1962*b*, **64**, 123-125.

———. Serial rote learning: incremental or all-or-none? *Q. J. exp. Psychol.*, 1963, **15**, 27-35.

Johnson, R. C. Reanalysis of "Meaningfulness and verbal learning." *Psychol. Rev.*, 1962, **69**, 233-238.

Kaswan, J. Association of nonsense figures as a function of fittingness and intention to learn. *Amer. J. Psychol.*, 1957, **70**, 447-450.

Keller, L., Cole, M., Burke, C. J., and Estes, W. K. Reward and information values of trial outcomes in paired associate learning. *Psychol. Monogr.*, 1965, **79**, No. 12, whole No. 605.

Kendler, H. H., and D'Amato, M. F. A comparison of reversal shifts and nonreversal shifts in human concept formation behavior. *J. exp. Psychol.*, 1955, **49**, 165-174.

Kintsch, W. All-or-none learning and the role of repetition in paired-associate learning. *Science*, 1963, **140**, 310-312.

Kohler, W. *Gestalt Psychology.* New York: Livermore, 1929.

———. On the nature of association. *Proc. Amer. Philos. Soc.*, 1941, **84**, 489-502.

Kurtz, K. H. Discrimination of complex stimuli: The relationships of training and test stimuli in transfer of discrimination. *J. exp. Psychol.*, 1955, **50**, 283-292.

Landsman, H. J., and Turkewitz, M. Delay of knowledge of results and performance on a cognitive task. *Psychol. Rep.*, 1962, **11**, 66.

Lawrence, D. H. The nature of a stimulus. In Koch, S. (ed.) *Psychology: A Study of a Science.* Vol. 5. New York: McGraw-Hill Book Company, Inc., 1963.

Mackworth, J. F. The visual image and the memory trace. *Canad. J. Psychol.*, 1962, **16**, 55-59.

McCrary, J. W., and Hunter, W. S. Serial position curves in verbal learning. *Science,* 1953, **117**, 131-134.

McGeogh, J. A. *The Psychology of Human Learning.* New York: Longmans, Green & Co., Inc., 1942.

———, and Irion, A. L. *The Psychology of Human Learning.* New York: David McKay Company, Inc., 2d ed., 1952.

McGuire, W. J. A multi-process model for paired-associate learning. *J. exp. Psychol.*, 1961, **62**, 335-347.

Melton, A. Implications of short-term memory for a general theory of memory. *J. verb. Learning verb. Behav.*, 1963, 2, 1-21.

———, and Irwin, J. M. The influence of degree of interpolated learning on retroactive inhibition and the overt transfer of specific responses. *Amer. J. Psychol.*, 1940, **53**, 173-203.

Miller, G. A., and Selfridge, J. A. Verbal context and the recall of meaningful material. *Amer. J. Psychol.*, 1950, **63**, 176-185.

Miller, G. A. The magical number seven; plus or minus two: some limits on our capacity for processing information. *Psychol. Rev.*, 1956, **63**, 81-97.

———. Free recall of redundant strings of letters. *J. exp. Psychol.*, 1958, **56**, 485-491.

Murdock, B. B., Jr. Backward learning in paired-associates. *J. exp. Psychol.*, 1956, **51**, 213-215.

———. The immediate retention of unrelated words. *J. exp. Psychol.*, 1960*a*, **60**, 222-234.

———. The distinctiveness of stimuli. *Psychol. Rev.*, 1960*b*, **67**, 16-31.

———. Short-term retention of single paired-associates. *Psychol. Rep.*, 1961*a*, **8**, 280.

———. The retention of individual items. *J. exp. Psychol.*, 1961*b*, **62**, 618-625.

Nachmias, J., Gleitman, H., and McKenna, V. The effect of isolation of stimuli and responses in paired associates. *Amer. J. Psychol.*, 1961, **74**, 452-456.

———, and Sternberg, S. An analysis of the recognition process. Paper read at Psychonomic Meetings, Philadelphia, August, 1963.

Newman, S. E. A mediation model for paired-associate learning. Tech. Report No. 1, ONR Contract NONR 486 (08). North Carolina State College; Raleigh, North Carolina, 1961.

———, and Saltz, E. Isolation effects: stimulus and response generalization as explanatory concepts. *J. exp. Psychol.*, 1958, **55**, 467-472.

Noble, C. E. An analysis of meaning. *Psychol. Rev.*. 1952, **59**, 421-430.

———. The meaning-familiarity relationship. *Psychol. Rev.*, 1953, **60**, 89-98.

Parker, G. V. C., and Noble, C. E. Effects of experimentally-produced meaningfulness (*m*) on paired-associate learning. *Amer. Psychologist,* 1960, **15**, 451 (abstract).

Peterson, L. R. Immediate memory: data and theory. In Cofer, C. N., and Musgrave, B. S. *Verbal Behavior and Learning.* New York: McGraw-Hill Book Company, Inc., 1963.

———, and Peterson, M. J. Short-term retention of individual verbal items. *J. exp. Psychol.*, 1959, **58**, 193-198.

———, and ———. The effect of spacing repetitions on short-term retention. *Amer. Psychologist,* 1960, **15**, 450 (abstract).

———, and ———. Minimal paired-associate learning. *J. exp. Psychol.*, 1962, **63**, 521-527.

———, ———, and Miller, A. Short-term retention and meaningfulness. *Canad. J. Psychol.*, 1961, **15**, 143-147.

———, Saltzman, D., Hillner, K., and Land, V. Recency and frequency in paired-associate learning. *J. exp. Psychol.*, 1962, **63**, 396-403.

Postman, L. The history and status of the law of effect. *Psychol. Bull.*, 1947, **44**, 489-563.

———. The generalization gradient in recognition memory. *J. exp. Psychol.*, 1951, **42**, 231-235.

———. The present status of interference theory. In Cofer, C. N. *Verbal Learning and Verbal Behavior.* New York: McGraw-Hill Book Company, Inc., 1961.

———. Repetition and paired-associate learning. *Amer. J. Psychol.*, 1962, **75**, 372-389.

———. One-trial learning. In Cofer, C. N., and

Musgrave, B. S. *Verbal Behavior and Learning.* New York: McGraw-Hill Book Company, Inc., 1963.

———, and Riley, D. A. A critique of Kohler's theory of association. *Psychol. Rev.*, 1957, **64**, 61-72.

———, and Sassenrath, J. The automatic action of verbal rewards and punishments. *J. gen. Psychol.*, 1961, **65**, 109-136.

Prentice, W. C. H., and Asch, S. E. Paired-associations with related and unrelated pairs of nonsense figures. *Amer. J. Psychol.*, 1958, **71**, 247-254.

Restle, F. Sources of difficulty in paired-associate learning. In Atkinson, R. C. (ed.) *Studies in Mathematical Psychology.* Stanford, Calif.: Stanford University Press, 1964.

Reynolds, G. S. Behavioral contrast. *J. exp. anal. Behav.*, 1961, **4**, 57-71.

Rock, I. The role of repetition in associative learning. *Amer. J. Psychol.*, 1957, **70**, 186-193.

———, and Heimer, W. Further evidence of one-trial associative learning. *Amer. J. Psychol.*, 1959, **72**, 1-16.

Rock, R. T., Jr. The influence upon learning of the quantitative variation in after-effects. Teach. Coll. Contr. Educ., 1935, No. 650.

Rockway, M. R., and Duncan, C. P. Pre-recall warming-up in verbal retention. *J. exp. Psychol.*, 1952, **43**, 305-312.

Rothkopf, E. Z., and Coke, E. U. The prediction of free recall from word association measures. *J. exp. Psychol.*, 1961, **62**, 433-438.

Runquist, W. B. Stimulus generalization in paired-associate learning. Paper at Western Psychol. Assoc. meetings, April, 1962.

Saltz, E. Response pretraining: differentiation or availability. *J. exp. Psychol.*, 1961, **62**, 583-587.

Saltzman, I. J. Delay of reward and human verbal learning. *J. exp. Psychol.*, 1951, **41**, 437-439.

Schulz, R. W. Generalization of serial position in rote serial learning. *J. exp. Psychol.*, 1955, **49**, 267-272.

Shannon, C. E. A mathematical theory of communication. *Bell Syst. Tech. J.*, 1948, **27**, 379-423, 623-656.

Shepard, R. N. Stimulus and response generalization: tests of a model relating generalization to distance in psychological space. *J. exp. Psychol.*, 1958, **55**, 509-523.

———, and Teghtsoonian, M. Retention of information under conditions approaching a steady state. *J. exp. Psychol.*, 1961, **62**, 302-309.

———. Application of a trace model to the retention of information in a recognition task. *Psychometrika*, 1961, **26**, 185-203.

———. The analysis of proximities: multidimensional scaling with an unknown distance function. I. *Psychometrika*, 1962, **27**, 125-140.

Slamecka, N. J., and Ceraso, J. Retroactive and proactive inhibition of verbal learning. *Psychol. Bull.*, 1960, **57**, 449-475.

Sperling, G. The information available in brief visual presentations. *Psychol. Monogr.*, 1960, **74**, *11*, whole No. 498.

Suppes, P., and Ginsberg, R. A fundamental property of all-or-none models. *Psychol. Rev.*, 1963, **70**, 139-161.

Swets, J. A., Tanner, W. P., Jr., and Birdsall, T. G. Decision processes in perception. *Psychol. Rev.*, 1961, **68**, 301-340.

Thorndike, E. L. *The Fundamentals of Learning.* New York: Teacher's College, 1932.

———, and Forlano, G. The influence of increase and decrease of the amount of reward upon the rate of learning. *J. educ. Psychol.*, 1933, **24**, 401-411.

Thune, L. E. Warm-up effect as a function of level of practice in verbal learning. *J. exp. Psychol.*, 1951, **42**, 250-256.

Thurstone, L. L. A law of comparative judgment. *Psychol. Rev.*, 1927, **34**, 273-286.

Underwood, B. J. The effect of successive interpolation on retroactive and proactive inhibition. *Psychol. Monogr.*, 1945, **59**, *3*, whole No. 273.

———. Retroactive and proactive inhibition after five and forty-eight hours. *J. exp. Psychol.*, 1948, **38**, 29-38.

———. Studies in distributed practice: VII. Learning and retention of serial nonsense lists as a function of intralist similarity. *J. exp. Psychol.*, 1952, **44**, 80-87.

———. Interference and forgetting. *Psychol. Rev.*, 1957, **64**, 49-60.

———, and Postman, L. Extra-experimental sources of interference in forgetting. *Psychol. Rev.*, 1960, **67**, 73-95.

———, Rehula, R., and Keppel, G. Item selection in paired-associate learning. *Amer. J. Psychol.*, 1962, **75**, 353-371.

———, Runquist, W. N., and Schulz, R. W. Response learning in paired-associate lists as a function of intralist similarity. *J. exp. Psychol.*, 1959, **58**, 70-78.

———, and Schulz, R. W. *Meaningfulness and Verbal Learning.* Philadelphia: J. B. Lippincott Company, 1960.

Waugh, N. C. Free versus serial recall. *J. exp. Psychol.*, 1961, **62**, 496-502.

———. Length of series and the learning curve. *Amer. J. Psychol.*, 1962, **75**, 177-192.

Williams, J. P. Supplementary report: A selection artifact in Rock's study of the role of repetition. *J. exp. Psychol.*, 1961, **62**, 627-628.

———. A test of the all-or-none hypothesis for verbal learning. *J. exp. Psychol.*, 1962, **64**, 158-165.

WOGAN, M., AND WATERS, R. H. The role of repetition in learning. *Amer. J. Psychol.*, 1959, **72**, 612-613.

WOLLEN, K. One-trial versus incremental paired-associate learning. *J. verb. Learning and verb. Behav.*, 1962, **1**, 14-21.

YOUNG, R. K. A comparison of two methods of learning serial associations. *Amer. J. Psychol.*, 1959, **72**, 554-559.

———. The stimulus in serial verbal learning. *Amer. J. Psychol.*, 1961, **74**, 517-528.

———. Tests of three hypotheses about the effective stimulus in serial learning. *J. exp. Psychol.*, 1962, **63**, 307-313.

CHAPTER 6

Motivation and Affectivity

JOHN F. HALL
The Pennsylvania State University

INTRODUCTION

Although Vinacke (1960) has speculated that the coming era in the development of academic experimental psychology will become known as the "age of motivation," it must be readily acknowledged that at the present time it is in a somewhat confused as well as disordered state. The area is abundantly supplied with such concepts as desire, drive, incentive, interest, need, and purpose, to mention only a few, and although many would accept the position that all of these terms are "motivational" in character, the properties or functions which distinguish one from another or which differentiate motivational concepts from nonmotivational ones are only infrequently spelled out. We find that, at least with regard to a definition of drive or motive, we are no better off now than we were over thirty years ago when Granich (1932) was concerned with the fact that the current literature indicated six definitions for the concept of drive and twice that many for motive. In spite of such definitional difficulties, a number of distinct, but yet related approaches to the area can be discerned.

One approach has been to examine a motive's functional characteristics. Here, a motive is conceived of as an intervening variable which operates or functions to change behavior in specific or characteristic ways. Stimuli, or conditions which produce such changes, would be classified as motivational.

One such function has been specified as an energizing or activating function. Beginning with the work of Richter (1922) and continuing with Dashiell (1928), Cannon (1932), and more recently Hull (1943), and Hebb (1955), it has been assumed that the presence of a motive energizes or elicits behavior in the organism. A need state, for example, is presumed to increase an organism's activity, and thus it becomes a motivational determinant by virtue of its energizing function. As we shall note, this function plays a major role in the contemporary motivational positions of Hull and Spence. On the other hand, such a function has been vigorously attacked on both experimental as well as theoretical grounds, and it has led Cofer (1959) to speculate that ". . . motivation as a distinctive concept, coordinate to other psychological concepts, may well disappear."

A second function has been denoted as a selective or, perhaps better stated, reinforcing function. Such a function posits that certain stimuli which follow the making of a response may operate to either increase or decrease the probability of that response being made again. Such a statement refers to the actions of what are typically regarded as rewards and punishments. The role that these constructs play in changing behavior has been frequently subsumed under the topic of motivation since the reinforcing or selective function has been generally included as one function of a motive. On the other hand, some confusion has been introduced into this area by some writers who acknowledge such a function, but who nonetheless conceive of reward as a construct which is separate and distinct from motivation. Thus, the phrase motivation *and* reward is one which is frequently encountered.

A third function that from time to time has been posited is persistence. As Peak (1955) has written, "Behavior persists to a goal and it is this *persistence,* despite changes in stimulation, that makes it necessary to postulate the operation of intervening variables whose properties must be inferred from boundary conditions. We may willingly dispense with the somewhat anthropomorphic notion of a push or drive to action that is often implicit in discussions of energy, but we cannot forget the fact of persisting activity. . . ." Feather (1962) has also treated persistence as a motivational phenomenon. One problem, however, has been that persistence of behavior may be partially subsumed under the energizing function, since it is assumed that activity will continue as long as the motivational determinants are present. A second problem is that one of the most pervasive measures of persistence, i.e., resistance to extinction, has been generally regarded as a measure from which learning or performance may be inferred and thus reflects a motive's selective function. One may speculate that when the low correlations which have been found between acquisition measures and resistance to extinction are generally acknowledged, and there is a concomitant disavowal of extinction as a learning measure, there may be much to commend in the position that extinction is a measure of persistence.

Other motivational functions have been posited, but in general there has been little enthusiasm for their acceptance. The directive or cue function as postulated by Melton (1950) is one in which it is stated that motivational stimuli provide cues in directing an organism's behavior. There is no doubt that motivational determinants may produce stimuli that have directive properties, but such a function cannot help us in our primary mission of differentiating motivational constructs from nonmotivational ones. Finally, some writers working in the area of perception have posited a sensitizing as well as a defense function, both of which have presumably arisen from the operation of motivational constructs. Unfortunately, the findings of many of these studies which have demonstrated such functions can be explained by reference to the operation of other variables.

A second approach to the area has been the attempt to enumerate a class of conditions which, on a number of dimensions, are distinct from other classes, and which should be specified as motivational. These variables should, of course, reflect the motivational functions which we have just discussed. Many contemporary treatments of motivation have taken this approach—an early example is found in Dashiell (1928) who posited that it was the biological requirements or primary needs of an organism which provided the source from which all motives originate. A primary *function* of a need was to produce activity, or as Dashiell stated, "to get a man into motion."

We find then, that a motive can be defined in terms of its function, or by reference to a specific class of environmental events. A large number of investigations have been conducted in an effort to examine the characteristics of these environmental events which give rise to the operation of motive. Following another presentation (Hall, 1961) we shall describe these events as motivational antecedents and devote one part of our presentation to an examination of this work. A second area of motivational interest has been the determination of how the manipulation of motivational antecedents is reflected in a variety of behaviors or behavior consequents. Such behavior can be roughly classified as: (1) spontaneous, (2) consummatory, or (3) instrumental. The second part of our presentation will summarize a portion of the experimental work found in this area.

MOTIVATIONAL ANTECEDENTS

PRIMARY NEEDS It is not surprising when viewed in its appropriate historical context that the primary needs of an organism, typically produced by deprivation operations, were considered to be appropriate antecedents for the establishment of a motive. The experimental work of Richter (1922) and Moss (1924), as well as the introductory texts of Carr (1925) and Dashiell (1928), are landmarks in such a history. Working at about the time of the demise of instinct, Richter (1922) became interested in the determination of the origin of an organism's activity—

what it is that drives it, so to speak, about in its environment. Measuring activity in triangular tambour-mounted cages, he found that rats which were deprived of food increased their activity. As a result, he concluded that the origins of activity were found within the organism, and, in this particular study, in the contractions of the stomach. A few years later, Dashiell (1925) found a similar deprivation-activity relationship when the activity of hungry and satiated rats was measured in a checkerboard maze. Wang (1923) discovered that activity was related to the rat's estrous cycle, with estrum being correlated with maximum activity, thus supporting and extending the biological approach to the study of motivation. Such experimental evidence led Dashiell (1928) in his introductory text to write that a basic function of a motive was an arousal or energizing function. Of the many individuals who have continued to support and elaborate upon this function, Cannon's work has been most influential. Cannon (1932) considered need states to be automatic mechanisms which drove the organism to action and thus attempted to maintain a state of equilibrium or homeostasis as he chose to call it. Cannon's homeostatic model since this time has been considered by many to be a basic framework for contemporary motivational systems.

Physiological Factors in Primary Need States. A number of psychologists have joined the physiologists in being interested in the physiological basis of some of the primary needs, particularly those of hunger and thirst; as a result we should like to summarize some of the basic work in this area. The reader is referred to Miner (1955) or Wolf (1958) for a more extended presentation.

An examination of the physiological factors underlying hunger reveals that hunger has been related to: (1) gastric contractions and stomach distention as well as oropharyngeal stimulation, (2) blood conditions, and (3) central nervous system factors. The importance of stomach contractions for the sensation of hunger has been notably associated with the work of Cannon (1911-12), Cannon and Washburn (1912), and Carlson (1916). Briefly, it has been posited that hunger is caused by contractions of the stomach. Such a position was supported by Cannon and Washburn's findings that sensations of hunger, as reported by adult humans, were correlated with gastric contractions measured by pressure changes in a partially inflated balloon which had been swallowed by the subject. These conclusions, however, have been recently challenged by a number of investigators who have pointed out that: (1) it was the balloon itself which was responsible for the contractions (Davis, Garafolo, and Kveim, 1959; Davis, Garafolo, and Gault, 1957), and (2) neither eating nor food-seeking behavior is changed when the nerve pathways leading from the stomach to the brain are severed, or when the stomach itself is removed (Wangensteen and Carlson, 1931; Tsang, 1938; Bash, 1939; Morgan and Morgan, 1940).

Both caloric intake and stomach distention have also been examined as possible sources for the sensation of hunger, but as with stomach contractions, these too have been rejected as total explanations for hunger.

Stimulated by Carlson's (1916) early finding of an inverse relationship between an organism's blood sugar level and hunger contractions, a second area of interest has been to examine the contribution of blood sugar level, commonly referred to as a glucostatic factor. In enunciating such a position, Mayer (1955) has stated ". . . the mechanism postulated for the short-term regulation of energy intake rests on the concept that somewhere, possibly in the hypothalamic centers shown to be implicated in the regulation of food intake, perhaps peripherally as well, there are glucoreceptors sensitive to blood glucose in the measure that they can utilize."

A whole host of studies have been conducted in this area, utilizing physiological, behavioral, and experiential response measures, with the results being most controversial. Bulatao and Carlson (1924), and Stunkard and Wolff (1954, 1956), have presented findings in which they reported stomach contractions can be inhibited within a relatively short time as a result of an intravenous infusion of glucose solution. On the other hand, Quigley and Hallaran (1932) making seventy-one observations on twelve dogs, and varying the quantity of glucose injected from 1 to 25 g, reported that the hyperglycemia produced by the glucose had no immediate effect on spontaneous gastric motility.

Mayer and Bates (1952) have reported that rats which were given two daily injections of glucose, fructose, or adrenaline which produced hyperglycemia significantly decreased their food intake even when the caloric equivalent of the injected substance was taken into account. Smith and Duffy (1957) have also reported that injections of isotonic glucose depressed eating as compared with isotonic saline, but they were puzzled by the fact that similar injections of sorbitol and sucrose similarly depressed eating, even though sucrose could not have been converted to glucose and sorbitol only very slightly. As they have pointed out, if the reduced eating was due to the activation of glucostatic receptors, they must have been very nonspecific in response.

In sharp contrast to these findings are the results of a series of studies by Janowitz and Grossman (1948, 1949, 1951) and Bernstein and Grossman (1956) using rats, dogs, and humans as experimental subjects. These authors have found that a glucose injection was not accompanied by any greater depression of food intake than when a saline solution was injected into the organism.

One interesting development in this area has been the rather recent discovery of a relationship between glucagon, a quick-acting hormone capable of stimulating liver glycogenolysis and hence of raising the blood sugar concentration, and hunger. Studies by Stunkard, Van Itallie, and Reis (1955); Sporn and Necheles (1956); Morrison, Ju Lin, Eckel, Van Itallie, and Mayer (1958); and Schulman, Carleton, Whitney, and Whitehorn (1958) have all demonstrated the abolition of hunger contractions with glucagon administration.

In 1940 Hetherington and Ransom discovered that obesity could be produced by lesions confined to the hypothalamus, while a few years later, Brobeck, Tepperman, and Long (1943) found that such obesity was a function of hyperphagia or overeating. Since these early studies, a number of investigators have been interested, not only in describing the characteristic ingestion pattern of hyperphagic animals, but also in determining how such eating behavior is related to the animal's diet.

Hyperphagia typically consists of two phases—a dynamic phase in which the animal consumes great quantities of food and shows a rapid gain of weight, as well as a static phase in which the organism's weight levels off at a relatively high level, accompanied by some decrease in food intake. The influence of diet variations has been examined by a number of investigators. Teitelbaum (1955) for example, has discovered that grinding of the rat's normal diet of pellets to powder results in a decrease or actual cessation of eating for animals in the static phase of hyperphagia, although this variation in diet does not change the food consumption of either normal rats or those in the dynamic phase of hyperphagia.

In 1951, Anand and Brobeck (1951) working with rats found that injury to the hypothalamus could also produce aphagia or undereating. Teitelbaum and Stellar (1954), and Morrison and Mayer (1957) have found that after a period of aphagia, there may be a spontaneous recovering of eating, although such recovery is aided by tube feeding for varying lengths of time following the operation. In a recent review, Teitelbaum and Epstein (1962) have indicated that the recovery phenomenon, at least for the rat, can be divided into a number of stages. During Stage I, animals refuse all food, wet or dry, and they will not drink; during Stage II, the animals will eat wet and palatable food but refuse dry pellets and water. The animals do not eat well enough, however, to maintain body weight. Stage III consists of the animals regulating their caloric intake of the wet and palatable foods but still refusing to drink water. Stage IV is the eventual recovery in which the animals begin to drink water, although sensitive tests reveal that a severe deficit in water regulation still exists.

As would be expected, work in the area of thirst has paralleled that found with hunger. In general, investigators have been concerned with thirst as related to: (1) dryness found in the mouth and throat, (2) conditions of the blood, and (3) central nervous system factors.

Dryness in the mouth and throat as the primary contributor to thirst found its primary champion in the work of Cannon (1918), who stated that when the salivary glands "fail to provide sufficient fluid to moisten the mouth and throat, the local discomfort and unpleasantness which result constitute a feeling of thirst." Although some studies (i.e., Pack 1923; Holmes and Gregersen 1947, 1950, 1950*a*) have provided support for Cannon's position, a good deal of evidence has accumulated to dispute the position that thirst is solely dependent upon a dry throat. Montgomery (1931), for example, removed the salivary glands of dogs and measured their water intake for more than a year. Results revealed no increase in their average daily water intake, thus making it quite unlikely that the salivary glands played an important role in the thirst sensation. Similarly, Steggerda (1939) has reported that a college student's daily fluid intake of water was normal even though he did not have salivary glands.

That stomach factors are involved in thirst is revealed in the "sham" drinking studies of Bellows (1939) and Towbin (1949). In one series of studies, Bellows placed dogs on water deprivation and estimated their water deficit. This amount of water was then placed directly into the stomach. Bellows noted that if the animal was permitted to drink from 0 to 9 minutes following such water administration, the animal drank an amount equivalent to its body deficit. If the animal's drinking was delayed for 10 to 15 minutes, the animal drank only about 20 per cent of its deficit, while if the animal was delayed for 16 to 30 minutes following such water administration, the dog refused to drink. These experimental findings led Bellows to point out that thirst involved the presence of at least two factors: (1) one which is related to buccal and pharyngeal factors, and (2) one which acts after a delay and is related to the stomach, or other organs and conditions within the body. Towbin's (1949) work supplemented Bellows' in indicating that stomach distention was clearly one variable related to the amount of water consumed.

With regard to blood and tissue conditions, cellular dehydration and electrolytic concentration also appear to play a role in thirst. Gilman (1937) administered a 20 per cent solution of salt to some dogs, and a 40 per cent solution of urea to others. Although both solutions produce the same rise in osmotic pressure in the blood, they do so at different rates. The diffusion of urea into the cells, in contrast to sodium chloride, is such as to permit cellular equilibrium without the loss of water. When dogs were permitted to drink for fifteen minutes following the administration of one of these experimental liquids, animals which were injected with salt drank large quantities of water, while those injected with urea did not drink until after more than an hour.

Recently, Miller (1961a) has discussed results obtained in his laboratory concerning the role of electrolytes. His experimental technique has been to implant into a rat's brain two electrodes and measure the resistance between them. He has assumed that the electrolyte concentration of various body fluids is presumably the same so that specific electrode placement does not have crucial significance. A number of findings have demonstrated that thirst is partially dependent upon the concentration of electrolytes. He has reported that electrolyte concentration, as measured by changes in conductivity, increases with hours of water deprivation; and that the feeding of dry food to a hungry animal, which also makes the animal thirsty, also increases electrolyte concentration. That thirst is under the control of other factors as well, however, is revealed by the fact that an injection of hypertonic saline produces a greater increase in electrolytic conductivity than does a period of water deprivation, but water deprivation elicits greater amounts of drinking.

Since it has been discovered that lesions in the hypothalamus can result in either over- or undereating, it seems reasonable to assume that drinking behavior can be similarly influenced. In an early study in the area, Andersson (1953) found that hypertonic solutions of saline injected into the hypothalamic region of goats resulted in the animal's drinking almost immediately following injection. Two years later, Andersson and McCann (1955) found that electrical stimulation in the hypothalamic region could also produce drinking behavior and do so much more reliably than chemical injection. Miller (1961*a*) was interested in determining whether Andersson's early findings with goats could be repeated in cats, and also whether a *reduction* in drinking could be produced by an injection of pure water—a hypotonic solution. Results not only confirmed Andersson's findings, but moreover, it was found that the injection of distilled water reliably decreased water consumption.

ACQUIRED NEEDS It has been generally recognized that the behavior of many organisms and particularly the human cannot always be related to the operation of primary needs. As a result, it has been necessary to posit other mechanisms, or antecedents, in order to account successfully for such behavior.

One early attempt at such an accounting was made by Allport (1937) who, although accepting the position that biological needs could represent a starting point for any theory of motivation, believed that they were inadequate to account for the motivational structure found in the adult human. Somehow, he pointed out, the crude organic tensions of the child were transformed into adult motivational systems which were distinctive and different in aim and character from those found in infancy. The transformation principle, he posited, was that of functional autonomy. Such a principle involved the notion that adult motives were infinitely varied, self-sustaining, contemporary systems, which grew out of antecedent systems but were functionally independent of them.

Although arousing early interest, the concept of functional autonomy has been severely criticized, (Bertocci, 1940; McClelland, 1942; Rethlingshafer, 1943), and the difficulty of putting this loosely formulated principle to an experimental test has resulted in little enthusiasm for its acceptance as an answer to the question of how needs are acquired.

A second approach to this problem and one which has been taken much more frequently has been one which has emphasized learning principles. Dashiell (1937) proposed that when an external stimulus occurred together with a need state, the external stimulus could become a substitute for the need state. A few years later, Anderson (1941) speculated that through learning, drives could become externalized. More specifically Anderson posited that if an internally aroused drive was satisfied over a long period of time in a relatively constant external situation, the drive mechanism would become aroused by this external stimulation. It was further assumed that the process of externalization spread from one external stimulus constellation to another so that in time, almost any stimulus object that was associated in any way with the drive could come to arouse that drive and lead to the type of persistent behavior which reflected the operation of a motive.

Some years later, Hull (1951) incorporated into his system a corollary similar to the principle which Anderson proposed. "When neutral stimuli are repeatedly and consistently associated with the evocation of a primary or secondary drive and this drive stimulus undergoes an abrupt diminution, the hitherto neutral stimuli acquire the capacity to bring about the drive stimuli which thereby become the condition of a secondary drive or motivation."

More recently, Brown (1961) has spelled out implications of the acquired drive position in considerable detail by pointing out that secondary drive phenomena involve both learning and motivation. More specifically, acquired drives are learned responses having many of the general properties of all responses, but which, in addition, have the capacity to affect other responses in the same way in which these other responses are affected by motivational variables. Couched in somewhat different terms, the acquired drive problem demands answers to the question of how, and to what extent, the variables that govern the learning of one response function as though they were motivational variables when that response is interacting with other responses.

An examination of the evidence related to the acquired need hypothesis reveals that this area can be divided into those studies in which experimenters have attempted to set up acquired needs based upon: (1) appetitional states, as well as (2) noxious or aversive stimuli.

When appetitional states are used, both negative and positive findings have been reported; however, the findings of those investigators who have reported positive results, frequently can be interpreted by recourse to the operation of other variables. Moreover, when such studies have been replicated, negative findings have been the rule.

For example, Calvin, Bicknell, and Sperling (1953) placed two groups of rats in a black and white striped, triangular shaped box, for 30 minutes a day for 24 days. One group received this daily experience under 22 hours of food deprivation, while the other group was only one hour deprived. The subjects were not fed during this time. Following this, both groups were placed under 11½ hours of deprivation and the amount of food consumed by both groups during four 15-minute eating periods was measured. The result, which indicated that the 22-hour deprived group consumed significantly more food than the one-hour group, presumably supported an acquired need position. A number of other interpretations, however, are equally admissible. It is possible that the 22-hour deprived group, as a result of being fed only a short time each day, learned to eat rapidly, so that the short testing period reflected nothing more than faster eating on the part of these animals. Or, it is possible that the one-hour deprived subjects had learned other responses during their daily experience in the striped box, and that these responses interfered with the eating response when they were placed in the test situation. In any event, control groups were not employed to rule out these possibilities. It should be further noted that Siegel and MacDonnell's (1954) replication of this study failed to provide confirming evidence.

Other studies fall into a similar pattern. Siegel's (1946*a*) replication of Anderson's early study which has been used to support an externalization of drive position yielded negative findings, while Myers and Miller (1954), Greenberg (1954), Denny and Behan (1956), Andersson and Larsson (1956), Novin and Miller (1962), and Howard and Young (1962) are representative of those studies in which investigators have been unable to find experimental support for an acquired need based upon the need for food or water. It should be noted that Andersson and Larsson (1956) used goats, while Howard and Young (1962) employed monkeys as experimental subjects in contrast to the use of rats by all of the other investigators.

The Greenberg (1954), Andersson and Larsson (1956), and Novin and Miller (1962) studies are interesting since in these investigations there was an attempt to use techniques which would produce needs which would arise in a rather abrupt fashion, in contrast to the gradualness with which such needs are normally experienced. By injecting rats with a saline solution, Greenberg (1954) was able to produce an almost immediate need for water. A blinking light was then paired with the presence of the need state, but subsequent tests of the drinking response made in the presence of light but without prior injection revealed no tendency for experimental animals to consume more water than control animals who had not been presented with the blinking-light–need-for-water contingency.

In the Novin and Miller (1962) study, a strong and rapid increase in thirst was induced by feeding the animals dry food. Four groups of rats were utilized in this study with the experimental treatment as follows: Group 1 was a nonthirsty control group which was placed in a distinctive cage (9.75″ × 10″ × 8″ painted black with beaverboard floor) for 30 minutes; Group 2 had thirst aroused in a distinctive cage by being fed dry food for 30 minutes, the last three of which they were provided 3 cc of water. Group 3 was similar to Group 2 except that water was not provided during the last three minutes of the experimental session. Finally, Group 4 was placed in the distinctive cage when already thirsty, but it was given the opportunity to drink during the last three minutes of the 30-minute session. All animals were given one trial a day for 10 days and tested on Day 11. Following this, the animals were placed on ad lib schedule for three days, and then given 10 additional days training followed by a test day. Testing consisted of first satiating the animals for food and water in their home cage, and then placing them in the distinctive cage for 30 minutes, the last 15 minutes of which they had access to water. Although the technique of feeding the rats dry food promoted a strong and rapid increase in the thirst drive, differences among groups as measured by the amount of water consumed was not significant, nor was any trend in evidence. Finally, Andersson and Larsson (1956) elicited drinking by electrical stimulation in the hypothalamus. In an attempt to condition the drinking response, they paired a light or tone with such stimulation, but results indicated that the response could not be conditioned although a large number of CS-UCS pairings were provided.

A second class of acquired drives are those based upon noxious or aversive states; most generally pain, with its associated response of fear.

Miller's (1948) study is much too familiar to warrant a very specific detailing of his experimental procedure. Suffice to say, he trained rats to escape shock by running from a white to a black compartment. Following such training, when a door was placed so as to block the escape response, he found that some of his animals could learn to manipulate a wheel or press a bar in order to escape even though shock was no longer employed in the experimental situation. According to Miller (1948) fear which accompanied pain produced by the shock was conditioned to the white box. The response of fear had stimulus characteristics which also functioned as a motive for the instrumental response of bar pressing or wheel turning. Fear was thus conceptualized as a learned drive—learned since it was a response to a previous neutral cue (white box), and a drive since it was a stimulus which could motivate performance in the same way as primary need states. Fear reduction which accompanied escape into the black compartment served as reinforcement, not only for the animals learning to fear the white compartment, but also for the instrumental response which permitted the animal access to the black compartment.

A year later, Brown and Jacobs (1949) suggested that Miller's findings might be interpreted by positing frustration rather than fear. More specifically, they reasoned that the blocking of the escape response might have resulted in frustration which could serve to motivate the learning of those instrumental responses which permitted escape into the black compartment. Two studies were conducted in an effort to control for this possible frustration factor, and although both studies yielded somewhat similar findings, the second study provided the most unequivocal results. Here, experimental animals were placed in one section of an oblong box which had been divided into two sections by a partition, and given twenty-two trials with each trial consisting of the presentation of a light-tone combination (CS) with shock (UCS). The CS was presented for nine seconds, with the shock being presented during the last six seconds of the period. Escape responses in the form of jumping from one side of the box to the other could not be made. Control animals were presented with the CS but not with the UCS. Forty test trials (ten on each of four successive days) were then provided in which the partition separating the two sections was removed and replaced by a 2-in barrier, and the light-tone combination presented. Results indicated that although both experimental and control subjects crossed the barrier with about the same latencies for the first five trials, the experimental animals exhibited decreasing times on subsequent trials, while the control group revealed no tendency to improve their level of performance. As a result, the authors believed that frustration as a possible explanation for Miller's findings could be effectively ruled out.

Since the publication of these early studies demonstrating that fear can function as an acquired drive, a number of other studies have been conducted examining variables which would be expected to influence its strength. The most general conclusion that can be reached is that the strength of fear is influenced by the same variables that determine the strength of other conditioned responses. For an extended discussion of this topic, the reader is referred to Miller (1951) and Brown (1961).

As we find with other topics in psychology, the findings in this area are not completely unequivocal and questions can be raised about some of the results. Miller (1948), for example, found in his study that twelve of his twenty-five experimental subjects did not learn the new instrumental response which would permit them to leave the white compartment, while in a somewhat later experiment, Myers and Miller (1954) found that rats will learn to press a bar in order to escape from the white box into the black even though they are satiated for food and water, and shock or fear is not present. Studies of Carlson, Black, Walters, and Kirman (1957); Baron (1959); Smith, McFarland, and Taylor (1961); and Grossberg (1962) have also pointed to difficulties in the "fear as an acquired drive" position. In general, these studies have emphasized the fact that many of the reference experiments in this area have not utilized appropriate pseudoconditioning control groups. Smith, McFarland, and Taylor (1961), for example, found that rats failed to learn to turn a wheel in order to avoid shock when light was used as the CS, but that when a buzzer was substituted for the light, the avoidance response was acquired very rapidly. However, when a pseudoconditioning control group which also received the buzzer but which was never paired with shock was employed, it was found that this group learned the avoidance response as well as the experimental animals. In fact, at low shock levels the pseudoconditioned group performed significantly superior to the normally conditioned group. Grossberg's (1962) replication of the Brown and Jacobs (1949) study with pseudoconditioning controls also provided findings which have supported the Smith, McFarland, and Taylor (1961) study.

THE ROLE OF REWARDS The second major motivational determinant to be considered is that of reward or reinforcement. As we indicated in our introduction, there has been some ambiguity in this area, since a number of the writers have considered reward or reinforcement as distinct from motivation, although at the same time, acknowledging the reinforcing function as a major motivational function. Thus, it is not unusual to find current texts treating reward as a construct distinct from motivation. Although separate treatment of reward as contrasted to other motivational variables might be dictated by organizational considerations, reward should be recognized primarily as a motivational determinant.

Any examination of the concept of reward must first begin with a definition. In the area of learning where there has been major concern with the term, there are, as Kimble (1961) has pointed out, empirical definitions as well as theoretical. One empirical definition is found in the empirical law of effect (McGeogh and Irion, 1951) which simply states that acts are fixated and eliminated as functions of their effects. Theoretical definitions, on the other hand, are concerned with the nature of these effects or the mechanisms which underlie them. We cannot at this time undertake an extended discussion of the theoretical aspects of reward, but it should be pointed out that it was Hull's (1943) Law of Primary Reinforcement which for a number of years represented the most widely accepted theoretical definition of the construct. In his Principles, Hull (1943) defined primary reinforcement in terms of the reduction of a primary need; later, it was revised so that a reduction in drive rather than need became critical for reinforcement to take place although the distinction between need and drive was never made with any degree of preciseness.

Beginning in the early fifties and continuing to the present time, a whole host of studies have been published which have cast doubt on the position that reinforcement must be defined in terms of either need or drive reduction. Some investigators have been merely content to demonstrate that certain stimulus conditions unrelated to need or drive reduction could increase the probability of the occurrence of a response, while others have attempted to place their findings within some given theoretical framework. Some of the empirical findings have been as follows.

Sheffield and Roby (1950) found that saccharin could serve as a reward for a rat learning a position habit in a T-maze, while Hendry and Rasche (1961) have demonstrated that the licking at a stream of cool air (air drinking) could serve as reinforcement for thirsty rats; neither stimulus, however, has nutritive value for the animal. Sheffield, Wulff, and Backer (1951) have shown that naive male rats with no previous history of ejaculation could learn to run and climb hurdles with the only reward being the opportunity to copulate with a receptive female, although need reduction in the form of ejaculation was not permitted.

The studies of Marx, Henderson, and Roberts (1955); Kish (1955); Kling, Horowitz, and Delhagen (1956); Butler (1957); Hurwitz and De (1958); Roberts, Marx, and Collier (1958); Clayton (1958); Davis (1958); and Robinson (1961) are only a few of a large number which have shown that light increment or decrement has reinforcing properties, while studies by Butler (1957*a*) working with monkeys, and more recently Barnes and Kish (1961) using mice, have demonstrated that the onset of sound stimulation also may act as a reward. Campbell and Pickelman (1961) have found that objects to which an organism has been imprinted may serve as a reinforcing stimulus in an instrumental learning situation. Finally, a number of studies have appeared which explore the question of whether a unique environment will take on positive reinforcing value if animals are raised in it from infancy and for relatively long periods of time (Hunt and Quay 1961, Warren and Pfaffmann 1958).

Some investigators have demonstrated that reinforcement may be defined in terms of an organism's response. Here, Montgomery (1954), and Montgomery and Segall (1955) have found that rats can learn T-maze problems if the correct response is followed by permitting the animal to explore a Dashiell checkerboard maze, while Kagan and Berkun (1954) have shown that if rats are given the opportunity to run in an activity wheel, such running appears to reinforce a bar pressing response.

Any theoretical framework which has been adopted to explain the loci or mechanism of rewards will involve a consideration of the characteristics of either: (*a*) the stimuli which are presented following the instrumental response; (*b*) the response that is made to the stimulus; or (*c*) the physiological or internal states related to the action of either the stimulus or the response. The reader can supplement our abbreviated discussion by referring to Kimble's (1961) excellent treatment of this topic.

With regard to those mechanisms of reward which emphasize the stimulus, we have already called attention to a number of studies which have demonstrated that an increment in the intensity of a light or sound can be reinforcing. Some investigators have been concerned not with the absolute properties of a reinforcing stimulus but rather with stimulus change. Moon and Lodahl (1956), for example, placed rhesus monkeys in a plywood box for 60 seconds during which time a lever-pulling response would result in either a decrease (60-w lamp changed to 15 w), or an increase (15-w lamp changed to 60 w) in the illumination of the box. A control group was used in which the depression of the lever produced no illumination change. Results indicated a significant difference between the experimental groups and the controls, but a separate examination of the two experimental groups did not reveal any significant difference between them. The authors have interpreted these findings to indicate that the effective reinforcing dimension was *change* per se rather than the direction of such change. Forgays and Levin (1958) have also hypothesized that a change of sensory stimulation may have reinforcing properties.

In contrast to "explaining" the nature of the reinforcement via stimulus mechanisms, others have placed emphasis upon the response. In the Sheffield and Roby (1950) study to which we have previously referred, the authors in discussing their findings suggested that the ". . . performance of a consummatory response appears to be more important to instrumental learning—in a primary, not acquired way—than the drive satisfaction which the consummatory response normally achieves." The importance of the consummatory response was further demonstrated by the Sheffield, Wulff, and Backer (1951) study previously reported, which demonstrated that copulation without ejaculation could reinforce a response. Sheffield, Roby, and Campbell (1954) trained rats to run a straightaway in which food reinforcement varied in sweetness as well as in nourishment value. The findings indicated that when runway performance was plotted as a function of the rate of ingestion, an almost linear relationship was found between the two measures, the results again supporting a consummatory response interpretation of reinforcement. These authors have pointed out that reinforcement may play a dual role in having both an arousal as well as a selective function. They have proposed that ". . . the reward stimulus is at the outset of training an 'unconditioned' stimulus for the consummatory response in the sense that it will regularly elicit the response when presented. It would be expected that this terminal consummatory response would become conditioned to immediate neutral cues, especially those which just precede the onset of the reward stimulus. On successive experiences after the first these cues will arouse the consummatory response *ahead* of the reward. Moreover, this arousal of the incomplete consummatory response will work its way from the goal backward over the instrumental sequence since only the cues in this sequence invariably precede reward. Thus performing the correct responses—and only the correct ones—becomes a cue-producing situation that arouses the incomplete consummatory response."

More recent studies investigating the relationship between an instrumental running response and the consummatory response have not, however, supported the consummatory response hypothesis of reinforcement. Studies by Goodrich (1960); Snyder and Hulse (1961); and Collier, Knarr, and Marx (1961); to mention a few have all yielded negative findings. In the Snyder and Hulse (1961) study, these investigators examined the influence of three levels of water reinforcement (2.65, 1.50, and .75 cc), and three levels of total licks, (295, 450, and 675) upon: (*a*) running speed in an L-shaped runway, and (*b*) various aspects of the consummatory response. Thirty-four acquisition trials were provided. Results did not reveal any consistent relationship between running speed and either total volume consumed, total licks, volume per lick, or rate of licking. In the Collier, Knarr, and Marx (1961) study, rats were given fifty-two acquisition trials on a runway in which either a 4 or 32 per cent sucrose solution was used as reinforcement. Time permitted to drink was held constant at 20 seconds for twelve consecutive trials, 40 seconds for the next eight trials and one minute for the remaining trials. Results indicated that rats receiving the 32 per cent solution had faster running speeds than the 4 per cent group; and an analysis of the consummatory response in terms of total frequency of licks or total amount consumed revealed that after a large number of trials both measures differed significantly as a function of the intensity of the solution. However, it was found that differences in both starting time and running speed evinced themselves very early, long before these differences in lick rate or amount consumed appeared. The authors have concluded that neither variations in the amount consumed, nor the number of licks in one minute of reinforcement are necessary conditions for the production of differences in the instrumental response.

Interest in physiological factors underlying reinforcement has stemmed from Hull's early emphasis upon need reduction as its primary mechanism, as well as from the more recent discovery by Olds and Milner (1954) that electrical stimulation in certain sections of the brain may reinforce behavior. In an early exploratory study, Hull, Livingston, Rouse, and Barker (1951) produced two esophageal fistulas in a small dog. A tubular link was also made which could be placed into the fistula as taking the place of the interrupted section of the esophagus and with it in place, a dog could eat in a normal manner; without it, eating was a sham. Performance in a T-maze revealed that eating had much more reinforcing power than sham feeding, but that sham feeding was more reinforcing than no feeding at all.

Studies by Miller and his associates have confirmed as well as extended these general conclusions. Miller and Kessen (1952) had small plastic fistulas sewn into rats' stomachs and then provided their subjects with two training trials per day on a T-maze, with the first trial being a free choice trial, while the second trial was one which forced the animal to the side opposite to that chosen on trial one. A minimum of twenty-five days of training was provided. Three experimental groups were run: (1) stomach injection animals received an injection of 14 cc of milk via the fistula when they went to the correct side and 14 cc of isotonic saline when they went to the incorrect side, (2) no delay animals received dishes containing 14 cc of milk or saline in the correct or incorrect goal boxes respectively, (3) delay animals received their dishes of 14 cc of milk for a correct choice or saline for an incorrect one after a delay of 7 minutes and 35 seconds—the time normally taken to complete the injection. Results, indicated by Fig. 6-1, reveals the efficiency of receiving reinforcement via mouth, but it also demonstrates that learning is possible via milk placed directly into the stomach.

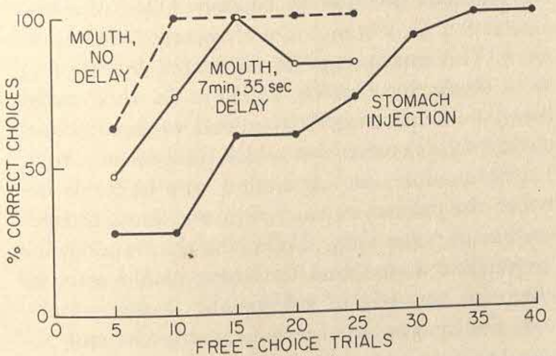


Fig. 6-1. Percentage of choices to the correct (milk) side on free choice trials. (Adapted from Miller and Kessen, 1952)

Support for these findings was obtained by Kohn (1951) who placed rats under food deprivation and then taught them to push a panel in order to secure milk. Following this, the animals underwent surgery in which a plastic fistula was sewn into their stomach. Following retraining, the animals were tested with three types of reinforcement: (1) 14 cc of enriched milk via the mouth, (2) 14 cc of enriched milk placed directly into the stomach, (3) 14 cc of saline placed directly in the stomach. Results, similar to those obtained by Miller and Kessen (1952), revealed that milk via mouth is superior in maintaining the panel pushing response to milk which is placed directly into the stomach. This condition is superior, however, to saline placed in the stomach. Berkun, Kessen, and Miller (1952) replicated Kohn's experimental procedure, but they measured the consummatory response of drinking. Similar findings were obtained.

In an effort to place the locus of reinforcing effects in the blood, Coppock and Chambers (1954) deprived rats for three days, following which each animal was restrained in a snugly fitting cage. Under anesthetic, a hypodermic needle was inserted in the caudal vein at the base of the tail. The animal's head was placed between vertical beams of an infrared light so that horizontal flexion of the head to either side actuated the photo-relay on that side. Following a 10-minute period of nonreinforcement in which an operant level of head turning was obtained, there followed a 90-minute reinforcement period. During this period, experimental subjects received a 10 per cent glucose solution as long as they kept their head in a given position, while control animals were reinforced with saline. Results indicated that the 10 per cent glucose solution was more effective than saline in increasing the relative duration of, or preference for, a given right or left head position.

In a second study, Chambers (1956) was unable to obtain a similar finding with dogs, but he reported positive results when rabbits were the experimental subjects.

As we have indicated earlier, a second area of physiological interest has been the examination of electrical stimulation in the brain as a source of reinforcement. The reference experiment here is a study by Olds and Milner (1954) who reported that rats which had electrodes implanted in the septal region of their rhinencephalon, when placed in Skinner boxes in which stimulation was correlated with the pressing of the bar, would continue to make the bar-pressing response again and again. These findings have been interpreted in terms of the brain having a reward or reinforcing section.

About the same time as Olds and Milner

(1954) reported their experimental findings, Delgado, Roberts, and Miller (1954) demonstrated that it was possible to motivate escape or avoidance behavior utilizing brain stimulation techniques. In this study, cats were trained to turn a paddle wheel in order to terminate a peripheral shock. Electrical stimulation was then provided in the tectal area, and after a number of trials the animal learned to rotate the wheel in order to terminate and thus escape from such central stimulation. In a second study, the same procedure was used except that a tone was used as a CS and paired with central stimulation. If the wheel was turned within a period of time, central stimulation could be avoided. Although the tone did not elicit any response at first, after a number of avoidance conditioning trials, the animals learned to avoid central stimulation. A few years later, Cohen, Brown, and Brown (1957) also demonstrated the feasibility of setting up conditioned avoidance responses via central stimulation.

Some investigators have shown that electrical stimulation at the same location may result in both rewarding as well as punishing effects. Roberts (1958) found that electrical stimulation would motivate cats to learn to respond correctly in a T-maze in order to terminate stimulation in the posterior hypothalamus, but it would not motivate them to learn to leave the starting box in order to avoid such stimulation. Roberts hypothesized that the onset of stimulation in this particular part of the brain was rewarding, but that continued stimulation resulted in pain. In a second study, (1958*a*) this hypothesis was confirmed, when it was demonstrated that cats would not only learn to press a bar in order to receive stimulation but also learn to perform an escape response in order to terminate stimulation of the same voltage. In this second study, a Y-maze was used in which the animal's entry into one arm turned stimulation on, entry into a second arm turned it off, while the third arm resulted in no change and thus permitted the animal to make errors. Results indicated that the animals learned to oscillate back and forth between the off and on arms. Comparable areas in the brain of the rat in which stimulation can lead to both approach and avoidance learning have been found by Bower and Miller (1958).

Perhaps the most intriguing finding in this area has been made by Brown and Cohen (1959). In this study, electrodes were implanted in the hypothalamus of the cat. When stimulation was presented, only those subjects which exhibited classical "hypothalamic rage" and random locomotion were selected as subjects for their study. Each subject was then required to learn an approach task, which consisted of running down a straightaway, as well as an avoidance task, involving running from one side to the other in a shuttle box. Here, the animals were required to run from one side of the box to the other during a 15-second presentation of a CS in order to avoid the UCS. The interesting aspect of this study was that .3 seconds of the hypothalamic stimulation served as: (1) reward for the animals following their running in the straightaway; and (2) the UCS which was to be avoided in the shuttle box.

SECONDARY REINFORCEMENT Since Hull (1943) postulated that need reduction was effective for only a short time in the strengthening of a stimulus-response relationship, there was an obvious difficulty in accounting for the learning of responses which were not followed immediately with reward. As a result, the principle of secondary reinforcement was introduced in order to help solve this problem. Briefly, this law stated that the power of reinforcement could be transmitted to any stimulus situation by consistent and repeated association of a stimulus with primary reinforcement. The concept can be illustrated by referring to a study by Cowles (1937). In this study, hungry chimps were first trained to insert poker chips into a slot machine which paid off in raisins. In this manner, an association was built up between the primary reward of raisins and a secondary one of poker chips. Following the training, the investigator found that the chips would serve as adequate rewards in subsequent learning tasks; thus the association of a neutral stimulus and primary reinforcement resulted in the neutral stimuli acquiring reinforcing properties.

Although most investigators have established secondary reinforcers by pairing neutral stimuli with some type of primary reinforcement (food, water, etc.), Smith and Buchanan (1954) have demonstrated that stimuli associated with reduction in shock would also acquire secondary reinforcing properties, while more recently, Roberts, Lebow, and Yoder (1961) have found it possible to obtain secondary reinforcement based only upon the primary reinforcing effects of stimulus change.

It is interesting to note that although the law of primary reinforcement has few supporters, the concept of secondary reinforcement has continued to have a history of extensive experimental investigation, roughly divided into two areas. The first has to do with its parameters; that is, how does the strength of a secondary reinforcer vary as a function of the number of primary reinforcements, amount of primary reinforcement, etc. The second area has to do with considerations concerning the nature of the principles which underlie its operation.

THE PARAMETERS OF SECONDARY REINFORCEMENT Since a learning procedure is involved in the neutral stimulus acquiring secondary reinforcing properties, the major variables which influence learning also appear to influence the strength of secondary reinforcers.

Amount of Primary Reinforcement. An examination of this variable has led some investigators to utilize what Lawson (1957) has termed the *differential* in contrast to an *absolute* experimental procedure. The differential method is one in which each subject serves as its own control; thus, it learns to associate one amount of reinforcement with one stimulus, and a different amount with another. A test situation is then presented in which the subject must make a choice between the two stimuli.

Using the differential method, D'Amato (1955) was able to demonstrate that the strength of a secondary reinforcing stimulus varied as a function of the amount of reinforcement with which it had been paired. Rats first learned to associate five pellets of food with a goal box of one color and just one pellet with a goal box of another color. The test situation consisted of utilizing a T-maze with the five-pellet box on one side and the one-pellet box on the other and giving the animals fifteen trials. Primary reinforcement, of course, was never provided in the test situation. Results indicated that the mean number of responses to the five-pellet box was significantly greater than chance; eighteen of the twenty rats used in the study made eight or more responses out of fifteen to the goal box previously associated with five pellets of food. Lawson's (1957) experiment has confirmed D'Amato's findings.

The absolute method involves a situation in which each group of experimental subjects has the opportunity to associate only one magnitude of reward with the primary reinforcing stimulus. The experimental findings, in contrast to those obtained with the differential method, are somewhat controversial. The early studies of Lawson (1953) and Hopkins (1955) provided negative findings, but more recent experiments of Butter and Thomas (1958), and Stebbins (1959) have provided positive results. In the Butter and Thomas (1958) study, rats received forty-eight trials in which the click of a reinforcement mechanism was associated with the animal obtaining either an 8 or 24 per cent sucrose solution. Following such training, a bar was introduced into the apparatus with all bar depressions being reinforced by the click which served as the secondary reinforcing stimulus. Experimental findings revealed the 24 per cent solution group made a significantly greater number of bar depressions than the 8 per cent group.

Frequency of Primary Reinforcement. A number of investigators (Hall, 1951; Bersh, 1951; Miles, 1956) have demonstrated that the strength of a secondary reinforcing stimulus is related to the frequency with which it has been paired with primary reinforcement. As an example of this general finding, Bersh (1951) provided five groups of rats with 10, 20, 40, 80, or 120 reinforcements for bar pressing. Each response produced a three-second light with reinforcement taking place after the light had been present for one second. A control group was given 120 reinforcements without the accompanying light. Following this, all groups were then extinguished, but the extinction situation did not include presentation of the light at any time. Presumably, this procedure reduced the bar-pressing response to the same level for all groups. Following this, a

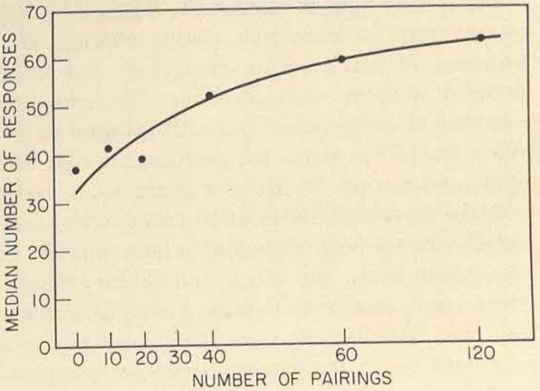


Fig. 6-2. Median number of bar-pressing responses. (Adapted from Bersh, 1951)

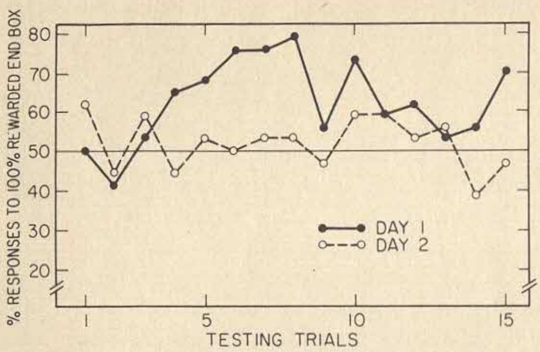


Fig. 6-3. Performance on secondary reinforcement test trials. (Adapted from D'Amato, Lachman, and Kivy, 1958)

final testing session was provided in which each bar depression resulted in the light being presented for one second. Results, presented in Fig. 6-2, reveal the relationship between the number of times that light was paired with food, and the median number of bar-pressing responses for each of the groups.

Partial Reinforcement. Partial reinforcement has been found to be an influential variable in learning studies; as a result, there has been interest in examining its role in determining the strength of secondary reinforcing stimuli. Two different types of training procedures have been used in these investigations. One has been to have subjects act as their own control in that they experience both continuous and partial reinforcement, with each reinforcement contingency being associated with different secondary reinforcing stimuli. The test situation is one in which differential responses are associated with the receiving of these different secondary reinforcing stimuli.

Using this type of procedure, Mason (1957) trained rats to learn two discriminations. One consisted of black versus grey, with the black stimulus positive, while the other discrimination consisted of white versus grey with white positive. With one of the tasks, the positive stimulus was reinforced on only 50 per cent of the trials, while with the other task, the positive stimulus was reinforced 100 per cent. Following a large number of acquisition trials, the black and white stimulus cards were paired thus forming a new discrimination task. The animals were then given ten nonreinforced trials. Results indicated that seventeen of the twenty animals made six or more choices to the stimulus which had been associated with continuous reinforcement during the original training.

A study by D'Amato, Lachman, and Kivy (1958) utilizing a slightly different training procedure has confirmed Mason's findings. These investigators provided runway training in which rats received partial reinforcement in a black (or white) goal box, and continuous reinforcement in white (or black). Following eighty-eight training trials, thirty test trials (fifteen per day) with a T-maze were provided. The black and white goal boxes used in the runway procedure were placed on opposite sides of the T, but primary reinforcement was not, of course, provided. As Fig. 6-3 reveals, entries into the goal box associated with continuous reinforcement were significantly greater than entries into the box associated with partial reinforcement. It can be noted, then, that there is general agreement with the position that a continuous reinforcement procedure is superior to a partial reinforcement schedule in establishing the strength of a secondary reinforcing stimulus *when each subject is used as its own control.*

The second procedure used to investigate the role of partial reinforcement on the strength of secondary reinforcing stimuli is to employ separate groups. During training, each group has a secondary reinforcing stimulus associated with either a continuous or a partial reinforcement contingency. The test situation involves a learning task in which the efficiency of the secondary reinforcing stimulus in promoting learning is contrasted with a neutral stimulus.

Using the separate group design, D'Amato, Lachman, and Kivy (1958) provided one group of rats with training on a runway in which reinforcement was obtained in a black goal box on

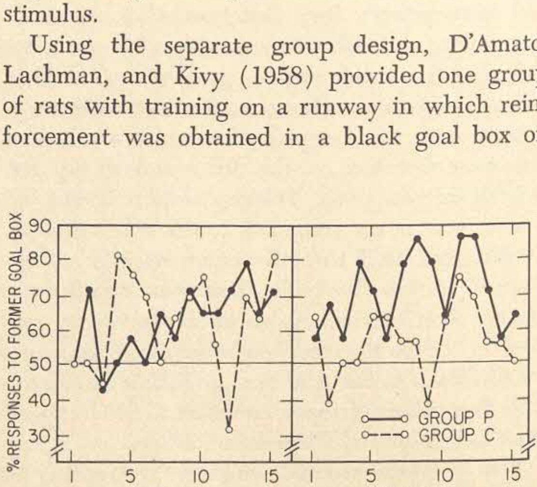


Fig. 6-4. Performance on secondary reinforcement test trials. (Adapted from D'Amato, Lachman, and Kivy, 1958)

every trial, while a second group was reinforced in the same black box on only 50 per cent of the trials. Following such training, both groups received thirty test trials (fifteen per day) on a T-maze which contained the previously reinforced goal box as well as a neutral goal box. Although no difference between groups was found for the first fifteen trials, an examination of the second fifteen trials revealed that the partial reinforcement group was superior to the continuous group. Figure 6-4 presents this finding.

Confirmation of this general finding has been obtained by Klein (1959). In this study, six groups of rats were trained to run a straightaway for reinforcement. Each group of animals was given 120 training trials with one of the following reinforcement percentages: 20, 40, 60, 80, 90, or 100. The goal box served as the secondary reinforcing stimulus. A T-maze, containing the previously reinforced goal box on one side and a neutral goal box on the other, was then employed for the test trials. The median number of entries over twenty test trials into the previously reinforced goal box for each of the varying groups can be noted in Table 6-1. Inspection of this data reveals that the strength of a secondary reinforcing stimulus increased as the percentage of reinforced trials during acquisition decreased. Finally, Armus and Garlich (1961) have extended these runway and maze findings to the Skinner box situation. In this study, rats were first trained to press a bar in order to receive food. For the continuous reinforced group, each bar press was always accompanied by food as well as by secondary reinforcement (sound of the pellet dispenser and a two-second flash of light). The partial reinforced group was placed on a fixed ratio schedule in which primary reinforcement (as well as secondary) was provided after every fifth response. The animals were then tested in a *two*-lever situation in which depression of one bar provided the secondary reinforcing stimuli, while the second bar did not. An examination of 150 test trials revealed that the fixed ratio group, as measured by per cent correct responses, exhibited a very rapid improvement in performance, while the continuous reinforcement group showed only a rather slow increase.

TABLE 6-1

Number of Entries into Training Box

	Reinforcement Groups					
	100%	90%	80%	60%	40%	20%
Median No. Responses	11.5	12.5	14.5	16.0	18.0	18.5

In conclusion, the separate groups procedure, as supported by D'Amato, Lachman, and Kivy (1958); Klein (1959); and Armus and Garlich (1961) studies, reveals the superiority of a partial reinforcement technique. This is in sharp contrast to those previous studies which we have reported in which the experimental procedure has been one of subjects serving as their own control, and which has provided experimental support for the continuous reinforcement procedure.

Temporal Relationships. Both Jenkins (1950), and Bersh (1951) have demonstrated secondary reinforcement to be a function of the interval of time which separates the presentation of the neutral stimulus and the occurrence of primary reinforcement. Using a Skinner box with the bar removed, Bersh (1951) first paired light with the presentation of food but varied the interstimulus interval. Thus, light and food was presented simultaneously to one group, while for five other groups, either .5, 1.0, 2.0, 4.0, or 10.0 seconds intervened between the onset of light and the receipt of the reinforcement. Following training trials, the bar was introduced into the situation, and conditions so arranged that a bar depression resulted in the presentation of light. The number of responses taking place during two 45-minute test sessions are shown in Fig. 6-5.

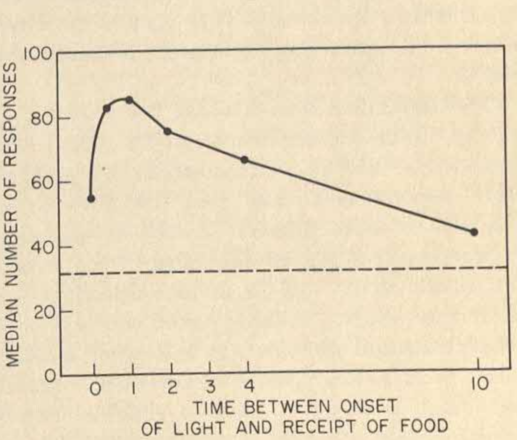


Fig. 6-5. Median number of bar-pressing responses made during two 45-minute test sessions combined. (Adapted from Bersh, 1951)

General and Theoretical Issues. One general consideration in this area has to do with whether reinforcing stimuli obtain their strength by actually reducing, at least in part, the drive state of the organism. Studies by both Simon, Wickens, Brown, and Pennock (1951), as well as by Miles and Wickens (1953) have been unequivocal in demonstrating that such is not the case. In this latter study, two groups of rats were conditioned in a Skinner box with food as reinforcement and a click (secondary reinforcing stimulus) associated with the delivery of food. Following such training, extinction trials were provided with one group receiving the click, while the other group did not. Immediately following the extinction trials food consumption was measured. Although the group which received the click during extinction trials pressed the bar significantly more times than the nonclick group, thus indicating that the click had served as an effective secondary reinforcing stimulus, there was no difference between the groups with regard to amount of food consumed.

The host of parametric studies investigating secondary reinforcement have provided overwhelming evidence that secondary reinforcing stimuli can increase the probability of a response being made, but there has been a nagging doubt in the minds of many as to whether or not secondary reinforcing stimuli are capable of accounting for the protracted behavior sequences which are so typical of man's behavior. As Hall (1961) has written, there must be the experimental demonstration of secondary reinforcement acting over a long period of time and without opportunity to be even occasionally associated with primary reinforcement, if the concept is to assume the heavy burden of accounting for much of human behavior.

The durability issue has not been solved, although studies by Zimmerman (1957, 1959) have provided at least a partial answer to this problem. This experimenter has used an intermittent schedule of both primary as well as secondary reinforcement in the establishment of the secondary reinforcer as well as in its subsequent use. More specifically, a training period was utilized in which a neutral stimulus was first paired continuously with primary reinforcement. As training continued, however, a fixed or variable interval reinforcement schedule was substituted for the continuous schedule. Such a procedure involved the frequent presentation of the neutral stimulus although the number of times it was paired with primary reinforcement grew progressively smaller. Finally, the neutral stimulus was tested for its secondary reinforcing properties by presenting it following the bar-pressing response, but the procedure was one which involved intermittent secondary reinforcement. The animal was required to make a large number of bar presses, but the secondary reinforcing stimulus occurred only a small percentage of the time. Using such a procedure, Zimmerman has pointed out that a highly stable response can be maintained through the action of only a secondary reinforcing stimulus.

The procedure utilized by Zimmerman involves not only a partial reinforcement schedule during the training period but also during the testing period; thus, a kind of two-stage procedure is employed, in which the intermittent schedule is used in both stages. An important question is whether or not it is necessary to provide partial reinforcement during both stages in order to obtain the long-lasting effects obtained by Zimmerman. Fox and King (1961) have attempted to answer the question.

The experimental design which they utilized was as follows: During Stage I (i.e., the establishment of a secondary reinforcer), a continuous reinforcement group received 100 presentations of a buzzer which was always paired with the presentation of water. A partial reinforcement group received 200 presentations of the buzzer, but only 100 reinforcements were provided. The ratio of buzzer/water dipper presentations was not fixed but took place as follows over five training sessions: 50:50; 50:30; 50:15; 50:5. Thus, the final ratio of buzzer to dipper presentations was 10:1. Stage II consisted of a test for the strength of the secondary reinforcer. A bar was introduced into the apparatus, and its depression could be followed by the sound of a buzzer. Subjects from both the continuous and partial reinforcement groups were randomly assigned to one of three Stage II groups: (1) those on a continuous schedule received the buzzer as a consequence of each bar depression; (2) those on a partial reinforcement schedule received the buzzer on a fixed-interval, one-minute schedule; (3) a control group never received the buzzer as a consequence of bar depression. Two one-hour experimental sessions comprised Stage II. The mean number of bar presses for the first hour for each of the varying groups is indicated in Table 6-2, and it reveals

TABLE 6-2

Mean Number of Responses for Session I during Stage II

Stage I Condition	Stage II Condition		
	Partial	Continuous	Control
Partial	70.6	23.6	17.7
Continuous	21.0	31.5	11.5

the clear superiority of using a partial reinforcement schedule during both stages. Statistical analysis revealed that all experimental groups gave significantly more responses than their appropriate control groups, but more important, the partial-partial group provided significantly more responses than any of the other three experimental groups. The authors point out that such a finding indicates that partial reinforcement conditions must be present during the establishment of the secondary reinforcer as well as the test situation in order to provide secondary reinforcement with any degree of durability.

One theoretical question which has been raised concerns the discriminability of the secondary reinforcing stimulus. In an early study in the area by Schoenfeld, Antonitis, and Bersh (1950), two groups of hungry rats were provided food pellets for bar-pressing responses. For the experimental group, a light of one-second duration went on at the beginning of eating, rather than being paired with the bar-pressing response, which is typically the case in secondary reinforcing studies. The light stimulus was not presented to the control group. Secondary reinforcement was measured by noting the number of bar presses during extinction, when only the light was presented following the making of a response. When no differences were obtained between the groups, the authors concluded that the secondary reinforcing stimulus must precede the primary reinforcer if it is to achieve reinforcing power, and they further proposed that discrimination training was required in order to obtain secondary reinforcing effects. In effect, neutral stimuli, if they are to become secondary reinforcers, must be discriminative stimuli as well. (A discriminative stimulus is one which, if present, serves as a cue for a response, and if it is absent, is a cue for not responding).

Since this early study, a number of other investigators (Webb and Nolan, 1953; Dinsmoor, 1950; and McGuigan and Crockett, 1958) have supported the position that some type of discrimination training is necessary if a neutral stimulus is to acquire secondary reinforcing properties.

Wyckoff, Sidowski, and Chambliss (1958) have argued that investigators should make a distinction between the cue function and the reinforcing function of a secondary reinforcer, and that most studies in this area have failed to do so. That is, they have pointed out that if a response is learned as a function of the action of a secondary reinforcer, one should demonstrate that such an effect cannot be attributed to the cue function. The experiment which they have reported is one which attempted to differentiate the reinforcing function from the cue function, and it consisted of training rats to approach and lick a water dipper in response to a buzzer. Such training was followed by a test period during which time the lever pressing produced a buzz but no water. Control subjects were given identical training, but during the test period, each control animal was "yoked" to an experimental subject so that whenever the experimental animal pressed the bar and received the buzz, the control animal also received the buzz. The authors argued that this type of control was necessary in order to control for the cue action of the buzzer, and that it would result in the subject running to the water dipper. Such activity would bring the animal into the vicinity of the bar, and the resultant activity would increase the probability of a lever-pressing response. Experimental findings revealed no differences in bar pressing between groups, and as a result, the secondary reinforcing data gathered in Skinner box studies have been deemed questionable by the authors.

A series of studies by Crowder and his associates (Crowder, Morris, and McDaniel, 1959; Crowder, Gill, Hodge, and Nash, 1959; Crowder, Gay, Bright, and Lee, 1959; and Crowder, Gay, Fleming, and Hurst, 1959) unfortunately have been unable to verify the Wykoff et al. findings. Crowder et al. found that their use of the "yoked" control group continued to provide evidence for secondary reinforcing effects. Nonetheless, the use of a "yoked" control in addition to the normal control group would appear to be an experimental precaution that investigators working in the area should consider.

OTHER MOTIVATIONAL DETERMINANTS—As we have noted in our previous discussion, beginning in the early fifties, primary needs and need reduction as the basic motivational determinants came under sharp attack. A number of investigators began to consider sources of motivation other than these, and in particular to examine the role that external stimulation played in the determining of motivational states. An examination of many of these studies reveals that two basic procedures have been employed. One has been to enrich the environment for the organism by providing new or novel stimulation; while the other has been to impoverish it.

ENVIRONMENTAL ENRICHMENT *Novel Stimuli —The Role of Curiosity and Exploration.* Although both Dashiell (1925) and Nissen (1930) had written of curiosity drives, their experimental findings were not in keeping with the *Zeitgeist,* and interest in this area remained dormant until 1950, when Berlyne proposed that novel stimuli give rise to the motivational state of curiosity, with its functioning based on two postulates and three corollaries. Briefly, Berlyne posited that: (1) when a novel stimulus impinged upon an organism's receptors, there occurred a drive stimulus producing responses which he termed curiosity; and (2) as the curiosity-arousing stimulus continued to be present, curiosity diminished. The corollaries indicated that (*a*) behavior which increases such stimulation will be reinforced, (*b*) after a time curiosity behavior will become habituated and stop, but (*c*) after a period of time there will be some recovery of the initial reactivity.

About the same time that Berlyne (1950) posited the existence of a curiosity drive, Montgomery (1951) published the first of a series of experimental studies supporting the presence of what he termed an exploratory drive, the operation of which was almost identical with that of Berlyne's curiosity motive. The exploratory drive, Montgomery proposed, was aroused by novel stimulation which elicited exploratory behavior. Such behavior decreased with the time that the organism was exposed to the stimulus, but it recovered during a period of nonexposure.

We can note then, that the prominent features of this kind of behavior which are hypothesized to stem from the drive states of curiosity or exploration are: (1) heightened interest in novel stimuli, (2) habituation of interest with continued exposure, and (3) recovery of responsiveness during unstimulated periods.

A large number of investigators employing a variety of organisms have confirmed the reliability of this general pattern of behavior. Some individuals (i.e., Menzel, Davenport, and Rogers, 1961) have found, however, that if very young organisms are used, novel stimuli will result in a type of fear response which, only after a time, elicit exploration.

Since Montgomery considered the exploratory drive to be a primary source of motivation, he was interested in pointing out its independence of physiological needs, and also in demonstrating that exploratory behavior could not be considered as merely an expression of the general activity of the subjects. Both Montgomery (1953), and Zimbardo and Montgomery (1957) found that exploratory behavior was reduced by food and water deprivation. In another study (Montgomery 1953*a*), it was found that moderate amounts of activity deprivation over an eight-day period did not influence the amount of exploratory behavior. Although other investigators (Glickman and Jensen, 1961; Hall, Low, and Hanford, 1960) have been unable to confirm the finding that exploratory behavior is depressed by deprivation states, there has been the frequent finding of no significant relationship between deprivation and exploratory behavior. In any event, Montgomery's position of exploratory behavior being independent of deprivation is probably correct.

Manipulative Stimuli. In an early study in this area, Harlow, Harlow, and Meyer (1950) provided rhesus monkeys with twelve days experience in manipulating an assembled mechanical puzzle. The animals were not placed on any deprivation schedule; neither were any rewards available to them following each day's session. Four other monkeys had disassembled puzzles placed in their cages for the same length of time. On days thirteen and fourteen, the performance of the two groups was compared by examining the responses the monkeys made to the assembled puzzles for five-minute periods. Results indicated that those monkeys which had assembled puzzles placed in their cages were significantly more efficient than their controls when measured by total number of solutions, solutions obtained in sixty seconds, and ratio of correct to incorrect responses.

In accounting for the behavior of these monkeys, the authors have written, "It is the opinion of the experimenters that a manipulation drive can best account for the behavior obtained in this investigation. The stimuli to the drive are external and, in conjunction with the animal's capacities, set the pattern of behavior. The manipulation is conceived of as having reinforcing properties that account for the precision and speed that subjects acquire in carrying out the solution, and the persistence they show in repeated performance."

Although some individuals have quarreled with this explanation for the behavior, a large number of subsequent investigations coming from Harlow's laboratory have testified to the reliability of the findings.

IMPOVERISHMENT OF THE ENVIRONMENT *Sensory Deprivation.* One general framework which incorporates the experimental findings just reviewed is to assume that marked deviations in an organism's external environment can result in evoking some motivational state which in turn elicits some kind of adjusting response. In the studies which we have examined, the deviation or change has come by enriching the environment with new or novel stimulation.

It would follow from such a position that deviations in the environment can also be provided by placing the organism in a restricted environment —one in which normal or unusual stimulation was absent.

In an early study, Butler (1953) placed a monkey in a wire cage, visually isolated it by covering the structure with an opaque material, and then presented a visual discrimination problem to the animal. A trial was begun by raising a screen and exposing the test stimuli to the animal. If the monkey pushed against one stimulus, identified by one color, the door would open and the animal was permitted to view its surroundings for thirty seconds. If the incorrect stimulus was chosen, the door would not open, but rather an opaque screen was immediately lowered. Three rhesus monkeys, following preliminary training, were given twenty trials a day for a total of twenty days. At the end of this period, an examination of performance, as revealed by Fig. 6-6, indicated that they had learned the problem. A subsequent study by Butler and Harlow (1954) has revealed that such behavior, reinforced only by visual exploration, extinguishes very slowly. One animal in this study, for example, worked for almost twenty hours before finally refusing to respond.

Butler has considered his work essentially a continuance of the earlier work coming from Harlow's laboratory, which has emphasized the role of external stimulation. More specifically, he has hypothesized a visual exploration motive, in order to account for the behavior of his animals, recognizing, however, that such a motive represents another facet of externally elicited motivational states. An important consideration here, however, is the fact that the animal's environment has been effectively impoverished by the experimental apparatus; thus, there is a kind of sensory deprivation which provides the appropriate environmental conditions for the eliciting of the instrumental behavior. That such behavior is related to deprivation effects is revealed in a study by Butler (1957), who kept his subjects in their experimental boxes for varying lengths of time, (0, 2, 4, or 8 hours), and then tested them. Reinforcement consisted of a 12-second view of the monkey colony outside the test cage. Results indicated that the number of responses to the visual incentive during a one-hour testing session increased up until the four-hour deprivation period and then leveled off.

The work on sensory deprivation with humans appears to be a logical continuation of the work with primates, but in reality it did not come about in this way. As Hebb (1961) has pointed out, this work began with the problem of brainwashing, with the chief impetus being the dismay at the kind of "confessions" being produced at the Communist trials. In an early study reported by Bexton, Heron, and Scott (1954) these investigators studied the effects of depriving male college students of visual, auditory, and many kinesthetic stimuli. The students were paid to lie twenty-four

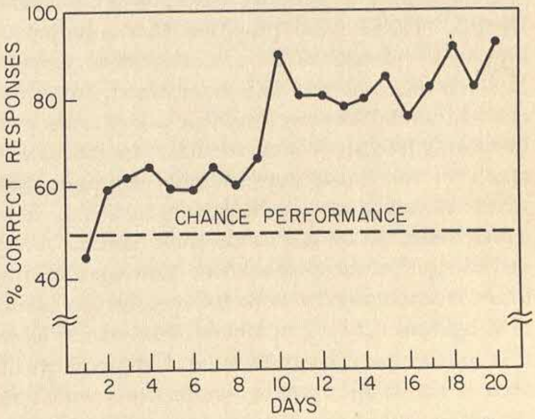


Fig. 6-6. Discrimination learning with visual exploration motivation. (Adapted from Butler, 1953)

hours a day in a comfortable bed in a lighted, semisoundproof cubicle which had an observatory window. Throughout the experiment, translucent goggles were used which admitted diffuse light but prevented pattern vision. Except when eating or at the toilet, cotton gloves and cardboard cuffs which extended from below the elbows to beyond the fingertips were worn, thus limiting tactual perception. A U-shaped foam rubber pillow, the walls of the cubicle and the masking noise of the thermostatically regulated air conditioner as well as other equipment severely limited auditory perception. Subjective reports indicated that in general the subjects first spent some time in reviewing their work, making attempts to solve personal problems, and in general engaging in organized thought patterns. As time wore on this type of activity became harder because of the difficulty of concentrating, and the subjects preferred "just to let their minds drift." Finally, blank periods during which the subjects could think of nothing would frequently occur. During isolation there were frequent reports of confusion, hallucinations, as well as an inability to concentrate.

Since the early Canadian studies in this area, a large number of investigators have been interested in this problem. As the Solomon, Kubzansky, et al. volume (1961) reveals, research has been concerned chiefly with two major areas; the first involves the relationship of parameters of sensory input to cognitive and behavioral efficiency; while the second has dealt with sensory deprivation as a special technique for assessing dynamic patterns of individual adaptation to a novel situation.

One interesting experimental variation in this general area is a study by Jones, Wilkinson, and Braden (1961), who proposed that information deprivation functions as a motivational variable in much the same way as hunger, thirst, etc. Thus, the information-deprived subject will execute with increasing frequency instrumental responses which result in increasing the amount of information which is introduced into the environment. Two experiments were run with their second study providing the most conclusive findings. In this latter study, subjects were isolated for ten hours in a lightproof room, in which noise was reduced to a minimum. The instrumental response available to the subject was a button press which resulted in a series of twenty-four light flashes, either red or green, which appeared on a panel in the ceiling of the experimental room. Varying degrees of information in the light series were used; the relative information values being .00, .33, .67, and 1.00. Thus, an information value of .00 provided no information, the twenty-four flashes being a single, fixed color. Information value of 1.00 was represented by a random determination of color for each of the twenty-four flashes making up the series, while the intervening values of .33 and .67 were associated with one third and two thirds of the twenty-four flashes being determined randomly with regard to color. A "cafeteria" selection of the light series was made possible by a dial which could be manipulated by the subjects in the dark. One group of subjects was allowed access to the dial after a delay of one hour, while a second group was given access to the dial after five hours. Results are indicated in Figs. 6-7 and 6-8. We note that Fig. 6-7 reveals that responses are a highly significant function of the information value of the light series, while Fig. 6-8 indicates the five-hour deprived group to be superior to the one-hour group in mean number of responses.

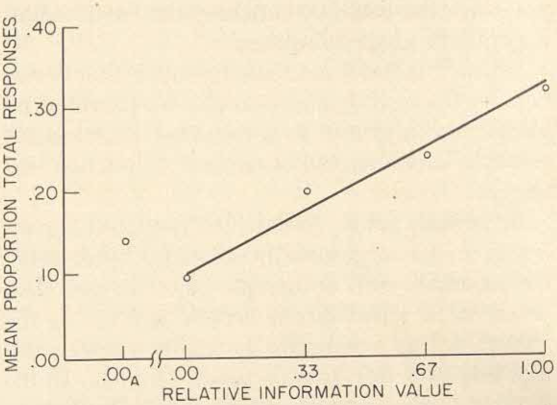


Fig. 6-7. Mean proportions of responses associated with the five light series. (Adapted from Jones, Wilkinson, and Braden, 1961)

One important conclusion from this study, aside from the experimental findings, is the proposal of the authors that the crucial aspect found in those studies presumably investigating curiosity, exploration, or manipulation, may be the reduction of uncertainty, and this information theory concept may be of considerable help in achieving some unification of these diverse points of view.

Berlyne's Theory of Arousal. Berlyne has used many of the experimental findings which we have

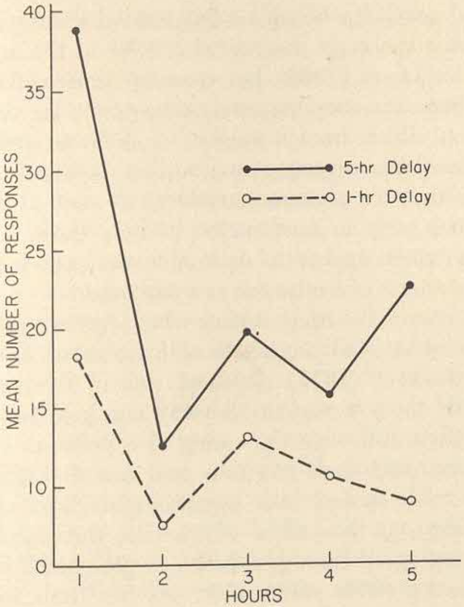


Fig. 6-8. Mean number of responses during the first five hours of access to the incentive. (Adapted from Jones, Wilkinson, and Braden, 1961)

presented in this last section in developing a theoretical system based on the construct of arousal (Berlyne, 1960). The importance of this volume merits our consideration, but unfortunately we can do little more than provide a brief outline of his position. First, he has posited that exploratory behavior can be divided into three categories according to the nature of the responses which comprise it. If the response involves a change in posture, or in the orientation of the sense organs, it is classified as an orienting response, while if such a response results primarily in locomotion, it is called locomotor exploration. The response measure in the experimental studies of both Berlyne (1950) and Montgomery (1951) would fall into this category. Finally, if the behavior produces changes in external objects, i.e., manipulation, such a response would be classified as an investigating response; included here would be Harlow's work involving the manipulation of puzzles by monkeys. In making such a classification, Berlyne has recognized response problems in this area, and he has effectively grouped within a single category a number of diverse behavior patterns.

The major construct of his theoretical system is arousal—an intervening variable which accounts for the behavior patterns which we have just enumerated. The determinants of arousal are grouped into three categories: (*a*) intensive variables, (*b*) affective variables, and (*c*) collative variables. Intensive variables are primarily concerned with the physical characteristics of the stimulus; intensity, size, etc., while affective variables comprise both pleasant and unpleasant forms of emotion. The last group of variables, collative ones, consist of novelty, uncertainty, complexity, and conflict—variables which most individuals relate to curiosity. Berlyne's basic postulate is ". . . that human beings and higher animals will normally strive to maintain an intermediate amount of arousal potential." Thus, if the level of arousal is too high, it becomes aversive and the individual will strive to reduce it; on the other hand, if it is too low, there will be an attempt to raise it.

Response Deprivation. In the studies which we have just considered and viewed within an *S-R* framework, emphasis has been placed upon deprivation on the stimulus side, although it could be pointed out that there has been a concomitant restriction of the organism's responses as well. Some investigators, however, have been interested in the response side of this relationship. More specifically, they have been interested in determining whether or not the restriction of activity on the part of the organism would result in subsequent increases in activity and thus perhaps merit the postulation of an activity drive.

Behaviorally, evidence for such a position would be the obtaining of a positive correlation between the amount of inactivity imposed upon an organism and the organism's subsequent activity level. The early studies of Shirley (1928); Siegel (1946); and Siegel and Alexander (1948) examining this problem provided only ambiguous findings. More recently, however, Hill (1956) has demonstrated that activity wheel behavior following 0, 5, 24, or 46½ hours of confinement in a small cage resulted in activity increases taking place as a function of the length of the confinement period. The complexity of the problem is revealed when longer periods of confinement are examined. With periods of confinement ranging from thirty-six to forty-seven days, activity when measured in a stabilimeter or tilt cage immediately increased following release from the confined quarters, but then decreased for several days thereafter. Activity when measured in the revolving drum, on the other hand, immediately decreased following such a confinement period. (Hill 1958, 1958*a*)

General Considerations. The major problem with many of the drive positions which have been reviewed in this section is that they do nothing more than describe the behavior which they purport to explain. A manipulation or curiosity drive, for example, simply states that animals engage in this type of behavior. Dissatisfaction with such a state of affairs has been voiced by a number of writers. Estes (1958), for example, has stated, "In a few well-studied experimental situations, involving for the most part food deprivation, water deprivation, or electric shock as antecedent conditions, all of the ingredients of the operational definitions are present and 'drive' can at least be used without ambiguity as a descriptive or summarizing concept. Its usefulness in this role breaks down, of course, when enthusiastic proponents extend usage of the term to situations in which only one of the defining relationships can be identified, thereby generating such ill-endowed mutants as exploratory drive."

Frustration. Although the Dollard, Doob, Miller, Mowrer, and Sears' text on Frustration and Aggression and the experimental work of Rohrer (1949), and Sheffield (1949, 1950), represented important contributions in this area, the work of Brown and Farber (1951), and Amsel and his associates has been largely responsible for frustration as a motivational determinant being considered within a theoretical framework. Brown and Farber (1951) assumed that frustration, regarded as a hypothetical construct, was produced either by some type of inhibitory condition or by a competitive excitatory tendency which is aroused simultaneously with an already ongoing excitatory tendency. The strength of this inhibitory tendency varies as a function of response blocking, amount of work, or nonreward. The primary consequents of frustration the authors point out were two: (1) it adds to the general drive level and thus functions like any other drive state, while (2) it provides internal stimuli to which appropriate responses may be attached.

Amsel (1958) has taken a somewhat similar position, although he has been primarily interested in nonreward as the basic contributor to frustration. In brief, he maintains that after a response has been rewarded a sufficient number of times to permit the development of anticipatory goal responses, nonreward will elicit a primary motivational condition which he has termed frustration.

Since the early theoretical articles in the area, and as Amsel (1962) has recently written, there has been considerable interest in examining frustration effects from a number of different points of view. Our organizational outline dictates however, that we concern ourselves, at least in this section, with an examination of only those variables which appear to determine the occurrence and strength of frustration as a motivator.

In one of the early studies which demonstrated the motivational properties of frustration, Amsel and Roussel (1952) deprived rats of food and trained them to run an alleyway into goal box 1, and then, following the eating of a pellet or two of food, had them run into goal box 2. Eighty-four trials spaced over twenty-eight days were provided, at the end of which time the animal's running speed from goal box 1 to goal box 2 had reached a stable value. Thirty-six test trials were then provided in which, on eighteen trials, the reward in goal box 1 was absent, while on the remaining trials it was present. The omission of reward, which the authors' hypothesized to be frustrating, should result in faster running. As Fig. 6-9 reveals, a comparison between the rewarded and frustration trials supported the posited motivational effects of frustration. This difference

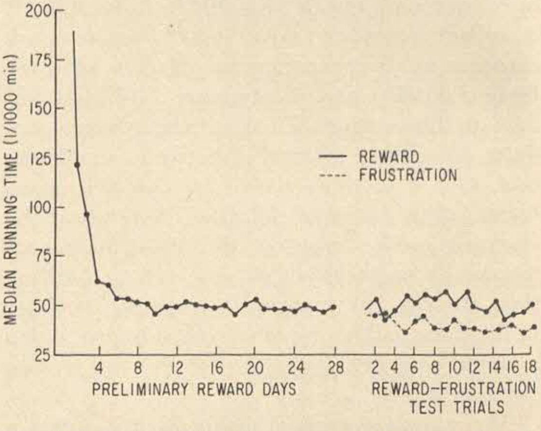


Fig. 6-9. Test trial performance under reward and frustration conditions. (Adapted from Amsel and Roussel, 1952)

between the vigor of performance following reward as contrasted with nonreward has been termed by Amsel as the frustration effect. It should be noted that delaying the animals for varying periods (5, 10, or 30 seconds) in the empty goal box 1, did not produce differential effects, and as a result, the authors' hypothesis that strength of frustration should vary as a function of the amount of time in the frustration situation was not confirmed.

An interesting extension of Amsel's position is found in a study by Penney (1960), who used kindergarten children and a specially designed apparatus which would permit the making of two discrete hand responses to two stimulus lights. During training, and following the making of the first response, the subject received a marble. This was followed by a second stimulus light, which was then followed by response II and a second marble. (Marbles were later exchanged for candy.) On test trials the reward which followed response I was omitted, and the author found it possible to obtain a frustration effect which Amsel and his colleagues have found with rats.

Bower (1962) was interested in determining whether the frustration effect varied as a function of the amount of the reduction of reward. In one study using the familiar double runway apparatus, animals were given eighty-four training trials in which four pellets of food were placed in goal box 1, and another four pellets in goal box 2. A test series was then provided in which the number of pellets found in goal box 1 was varied from the four found in the training series to three, two, one, or zero. Goal box 2 continued to have four pellets. Results as indicated in Fig. 6-10 reveal that the frustration effect, as measured by running speed on runway 2 was a function of the size of the reward found in goal box 1; thus, a reduction from four to three pellets was not as frustrating and did not result in as fast a running speed as a reduction of four to two, one, or zero pellets.

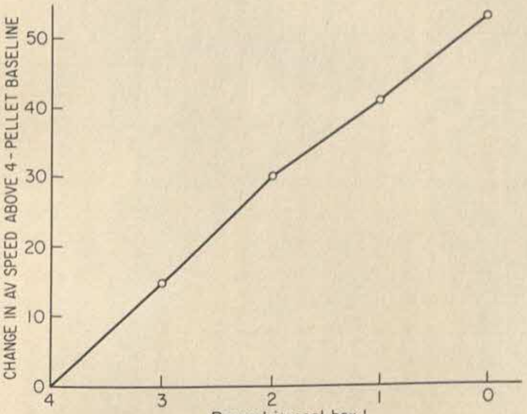


Fig. 6-10. Average change in running speed to goal box 2 following test rewards of 3, 2, 1, or 0 pellets in goal box 1. (Adapted from Bower, 1962)

One theoretical concern in this area is related to whether frustration is truly a nonassociative or motivational state, or whether the experimental findings can be interpreted in terms of learning or associative factors. With regard to this latter interpretation, it has been posited that the organism has simply learned to make a more vigorous response when frustrated. Marx (1956) in making such an interpretation has written: "This hypothesis is that the animal learns in previous situations to make a more vigorous response when frustrated. When the animal is frustrated in the experimental situation, he transfers, via frustration-produced cues, this earlier learning to the new situation, and we observe increased vigor in responding."

Although a pilot study by Marx (1956) tentatively supported the associative explanation, a more recent one by Amsel and Penick (1962) has not. In this latter study, a number of experimental variables presumed to be related to an associative interpretation were manipulated. The first was the number of rat pups present in a litter; here the authors reasoned that pups in a large litter (twelve per litter) would be more likely to be frustrated when hungry than pups in a small litter (three per litter); as a result, large-litter animals should learn to engage in more vigorous activity. A second variable concerned the conditioning of activity or inactivity to the frustrative presentation of food. Thus, following weaning, some animals were trained to vigorous activity in the presence of inaccessible food, while others were given food only after a period of relative inactivity. Again the authors reasoned that animals trained to be *inactive* in the presence of inaccessible food would exhibit a frustration effect at a later testing of a smaller magnitude than those subjects trained to be *active* in the presence of inaccessible food.

Following those experimental procedures which provided these varying associative experiences, regular double-runway training and testing similar to that found in the Amsel and Roussel (1952) study was provided. One primary difference, however, was that on half of the nonrewarded trials presented during the test period, food was present but inaccessible. (For the other half of the nonrewarded trials, food was absent.) Such a situation, the authors argued was ". . . more similar to the frustrative feeding experiences of subjects in the previous training stages, and it might be expected that there would be greater transfer of learned vigor or quiescence to this frustrative situation than to the food absent situation." Results revealed that the training procedure did not transfer to the test situation, and thus the frustration effect was not influenced by either the size of the litter or the conditioning of activity or inactivity to the frustrative presentation of food. These findings, the authors have stated, although not conclusive, do argue against an associative interpretation of frustration.

Affective States. There has been a reluctance on the part of many individuals to consider affective states (and conscious content) as an appropriate area of investigation, for in general, investigators have been more comfortable in dealing with those motivational antecedents which can be manipulated and measured more easily. Rewards defined in terms of per cent sucrose solution, or punishment as measured by the amount of electric shock, have been considered to be more suitable motivational antecedents than the affective state which has been presumably brought about by the operation of these variables. Moreover, there has been the problem of obtaining objective indices from which affective processes can be inferred.

A few investigators have been interested in this area. Certainly the general theoretical positions of Young (1955, 1959, 1961) and McClelland, et al. (1953) must be acknowledged. From time to time, others have also called attention to the role of affective states as a basic motivational determinant. Sargent (1948) has criticized those accounts of frustration which neglect the affect component, and he has hypothesized that emotion is the core to any frustration response; while Lazarus and Baker (1957) have considered affective arousal to be the fundamental component in their definition of stress. Hall (1961) has postulated that the affective states of the organism represent the common denominator for all those constructs which have been classified under the heading of ego involvement.

Experimental work in the area of fear, although

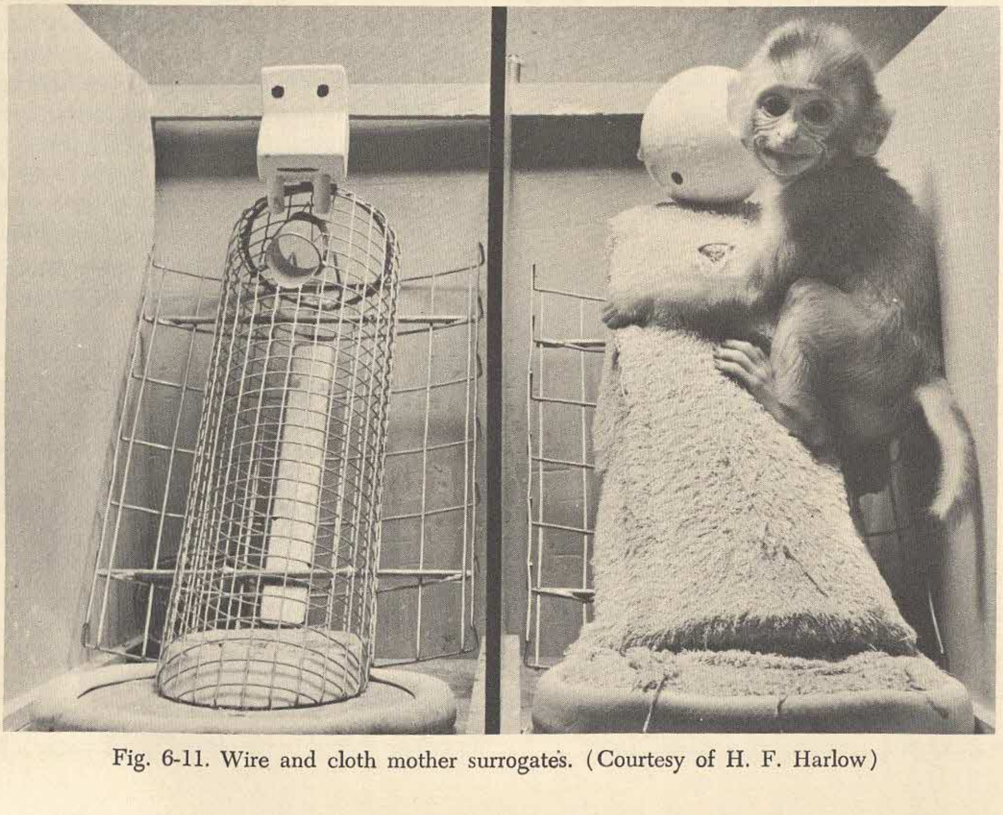


Fig. 6-11. Wire and cloth mother surrogates. (Courtesy of H. F. Harlow)

not placed in an affective state context is, nonetheless, relevant, and as we have already noted, quite extensive. The recent and exciting work of Harlow (1958, 1959) and his associates on the nature of love has aroused considerable interest in the area. The observation that baby monkeys which were separated from their mothers at birth had a strong attachment to the cloth pads which were used to cover the hardware cloth floors of their cages led Harlow (1958) to examine the role of contact comfort and the development of affectional responses in the infant monkey.

The procedure was to build two different types of artificial or surrogate mothers. One, designed to maximize comfort, was constructed with sponge rubber and covered with terry cloth. The other was made of wire mesh which was adequate to provide postural support for the infant but devoid of contact comfort. Both were designed so that the infant could nurse from a nipple which extruded from the body. Figure 6-11 illustrates the wire and cloth mother surrogates.

In one of the early studies designed to examine the relative importance of contact comfort in contrast to the securing of food, the cloth and wire mothers were placed in different cubicles attached to the infant's living cage. For four subjects, the cloth mother lactated, and the wire mother did not, while for four other subjects, this condition was reversed. As Fig. 6-12 indicates, the results reveal that contact comfort was a variable of extreme importance in the development of affectional responses, whereas lactation was of only negligible significance. The persistence of the effect is revealed throughout the 165 consecutive days of testing.

Subsequent studies in this series have investigated the role of these surrogate mothers in allaying fear responses and providing security for these infants. Findings revealed that the clinging and clutching responses of the infant monkeys were made significantly more frequently to the cloth than to the wire mother in the presence of fear-provoking objects. It has been noted that the retention of affectional responses persists over a period of years, even though the frequency of contact between the infant and the surrogate is reduced as the infant grows older. It has been demonstrated that when infants are separated from their mothers at 165 to 170 days of age and tested with a variety of affectional retention tests at 30-day intervals, the findings reveal that the attachment to the cloth mothers formed during the first six months of life is quite enduring. As Harlow and Zimmerman (1959) have written, " . . . once an affectional bond is formed it is maintained for a very considerable length of time with little reinforcement of the contact-comfort variable. The limited data available for infants that have been separated from their mother surrogates for a year suggest that these affectional responses show resistance to extinction similar to the resistance previously demonstrated for learned fears and learned pain."

THE INFLUENCE OF MOTIVATIONAL DETERMINANTS ON BEHAVIOR As we have indicated in our introduction, individuals working in the area of motivation have been interested not only in examining the characteristics of motivational determinants but also have been concerned with how their manipulation influences behavior. In this section, we shall be interested in examining the results of some of the experimental studies which have been done within this framework. The area is sufficiently extensive, however, that we shall have to be content to be selective rather than exhaustive in our presentation.

Psychologists do not have any generally accepted classification or taxonomy of behavior, but

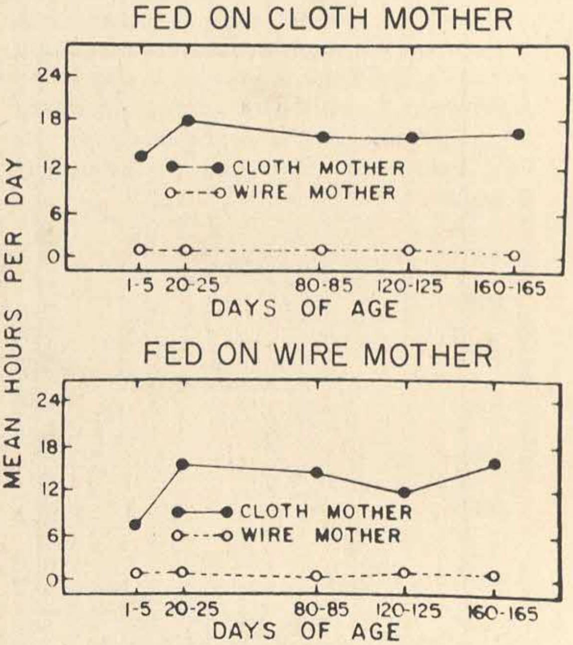


Fig. 6-12. Contact time on cloth and wire mother surrogates. (Adapted from Harlow, 1958)

one which is used from time to time involves (*a*) spontaneous, (*b*) consummatory and (*c*) instrumental behaviors. In keeping with a previous presentation (Hall, 1961), we shall continue to use this classification.

SPONTANEOUS AND CONSUMMATORY ACTIVITY

Both spontaneous and consummatory activity have been viewed as response measures often used to infer the presence of some motivational state as well as to demonstrate the energizing or arousal function of a motive. The presence of these behavior patterns has been viewed from time to time as a kind of automatic, innate response which indicates the presence of a motive state. The present section is an examination of some of the relevant experimental evidence in this area.

Spontaneous Activity as Related to Primary Needs. Experimenters have manipulated primary needs by either depriving the animal for a given amount of time or by reducing its body weight to some specific percentage (i.e., 85 per cent) of what it normally would be. The most common method has been to deprive the animal for a period of time; in the last analysis, however, time is only a conceptual framework which provides the opportunity for certain physiological events related to need states to take place. The weight of the organism is undoubtedly a better reflection of these physiological events, and the use of body weight measure is undoubtedly a better technique for inducing need states than the utilization of deprivation periods.

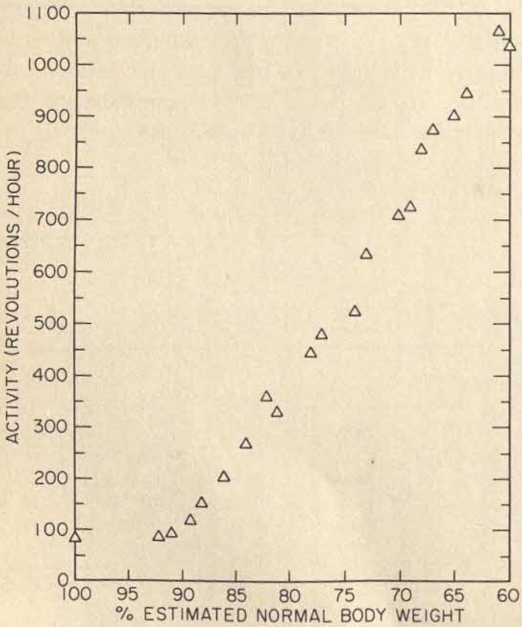


Fig. 6-13. Activity as a function of body weight. (Adapted from Moskowitz, 1959)

The activity measures that have been used have been typically the activity wheel, the stabilimeter or tilt cage, and finally a less restricted locomotor situation exemplified by the Dashiell checkerboard maze or some similiar type of apparatus.

Although early investigators (Richter, 1922, Dashiell, 1925) as well as more recent ones (Siegel and Steinberg, 1949, Adlerstein and Fehrer, 1955) were successful in obtaining a positive relationship between deprivation states and activity as measured either in the stabilimeter or less restricted locomotor situation, most of the recent investigations have not been successful in replicating these findings. Strong (1957) and Treichler and Hall (1962) using the stabilimeter, and Hall, Low, and Hanford (1960) and Treichler and Hall (1962) using the Dashiell checkerboard maze have been unable to obtain any relationship between how deprived an animal is and its general activity.

The activity wheel appears to be the only type of apparatus providing consistent findings which support a need-activity relationship. The studies of Hitchcock (1927), Wald and Jackson (1944), Finger (1951), Finger and Reid (1952), Hall (1956), Moskowitz (1959), and Treichler and Hall (1962) have all revealed that revolutions in the activity wheel increase as a function of the manipulation of either hours of deprivation or of decreases in body weight. As an example of the general findings obtained in this area, Moskowitz (1959) reduced the body weight of rats from 100 per cent to 60 per cent of estimated normal weight over a period of forty-one days, with activity being recorded for one hour each day. Figure 6-13 shows activity as a function of body weight deficit.

Stevenson and Rixon's study (1957) provides the possibility of reconciling the diverse findings between those activity wheel and stabilimeter studies which we have just reviewed. These investigators examined the role of deprivation and body temperature to activity wheel behavior. More specifically, the running activity of rats placed on four days of continuous deprivation was examined under the following range of environmental temperatures: 5, 10, 15, 22, 27, 30, and 33 degrees C. Both skin and colonic temperature records were also obtained from the animals. Results indicated that increases in activity wheel behavior under deprivation was an inverse function of environmental temperatures between 33° and 10°C. (One discrepant result was the finding that activity at 5°C was lower than that at 10°C.) Body temperature during this period was maintained. On the other hand, when the activity of the animals was severely restricted by placing them in a sling, body temperature declined as a function of deprivation.

In discussing their findings, the authors have pointed out that deprivation results in a deterioration in the insulation of the body and a concomitant reduction in body temperature. Muscular activity is one of the several means by which body temperature can be increased. As a result, if the deprived animal is given an opportunity to exercise, body temperature can be maintained. It would appear then, that the activity wheel provides the deprived animal with the opportunity to maintain body temperature, while the stabilimetric cage with its relatively restricted locomotor area does not provide such an opportunity.

Consummatory Responses as Related to Primary Needs. A logical continuation of the work relating the need state of an organism to its spontaneous activity is to relate such need states to consummatory behavior. It has appeared reasonable to assume that valid inferences about the presence of a motivational state may be made from the amount of consummatory activity engaged in by the organism. As we have found with spontaneous activity, food and water deprivation have been the two primary need states most frequently studied.

Although an early study by Bousfield and Elliot (1934) and a more recent one by Baker (1955) revealed, surprisingly enough, that there was no relationship between how hungry an animal was and how much it ate, most of the evidence does not confirm these results. Rather, the studies of Siegel (1947), Horenstein (1951), Stellar and Hill (1952), Lawrence and Mason (1955), Williams and Campbell (1961), and Dufort and Wright (1962) have all supported the position that the amount of consummatory behavior is a negatively accelerated function of amount of deprivation.

In the Stellar and Hill (1952) study, rats were deprived of water for 6, 12, 18, 24, 36, 48, 72, 96, 120, 144, and 168 hours. Four rats, with each animal serving under all conditions were the subjects. The amount of water consumed during a two-hour period following each deprivation period is indicated in Fig. 6-14.

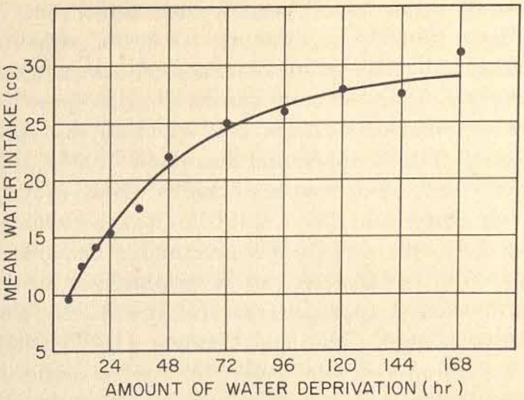


Fig. 6-14. Mean water intake following water deprivations ranging from 6 to 168 hours. (Adapted from Stellar and Hill, 1952)

Dufort and Wright (1962) matched rats on the basis of 24-hour food intake records and then placed them on either 0, 12, 24, 36, 48, 72, 96, 120, 144, 168, or 192 hours of deprivation. The amount consumed in two hours which followed the deprivation period was recorded. Results, similiar to those obtained by Stellar and Hill (1952) are indicated in Fig. 6-15.

The Role of Learning in Spontaneous and Consummatory Activity. At one time it was assumed that the characteristics of both spontaneous and consummatory activity which arose from depri-

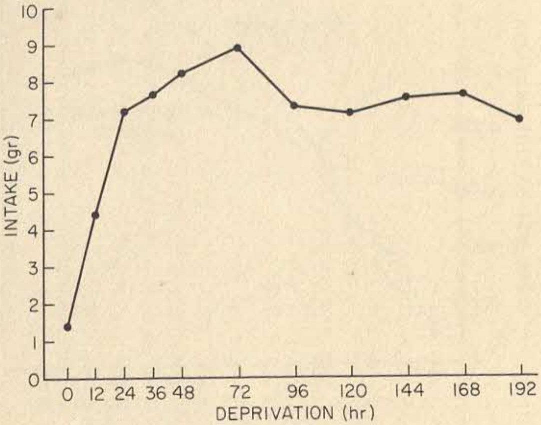


Fig. 6-15. Food intake as a function of the deprivation period. (Adapted from Dufort and Wright, 1962)

vation states were innately determined, or as Cofer (1959) has stated, automatic response indicators of the presence of a motivational state. Recent experimental evidence in this area has been sufficiently extensive to question this early assumption. Sheffield and Campbell (1954), Finger, Reid, and Weasner (1957), Hall (1958), and Amsel and Work (1961) have all demonstrated that activity when measured in the activity wheel or stabilimeter can be manipulated by the utilization of appropriate reinforcement schedules. In the Finger, Reid, and Weasner (1957) study, two groups of rats under twenty-one hours of deprivation were placed in activity wheels for one hour. One group went immediately from the wheels to their home cages and were fed for one hour. When the second group was removed from the wheel, they were placed in a neutral chamber for one hour prior to being fed for one hour in their home cage. As Fig. 6-16 reveals, the group which was fed immediately following running in the wheel increased their activity over that of the group whose feeding had been delayed.

Studies by Ghent (1951, 1957) reveal that the consummatory response is also influenced by learning. In her first study, rats were deprived for 23-hour periods, and the time that the animals spent in eating as well as the amount of food consumed was recorded. During the first eating period, the animals did not compensate for their deprivation loss since body weight was not maintained. The feeding period following the sixth day of deprivation indicated that the animals had doubled their intake relative to the first day although they were still not eating enough to maintain normal body weight. In her second study (Ghent, 1957), the experimental procedure consisted of depriving one group of animals of food for twenty-three hours per day for seven days, and then depriving them of water for twenty-three hours each day for the following seven days. A second group of animals was water deprived on days one to seven and food deprived on days eight through fourteen. The consummatory behavior for both groups of animals was observed for the first seven minutes during which food and water was available following each deprivation session. As with her first study, Ghent found that food-deprived animals did not eat immediately or continuously when first offered food but increased their eating time with practice. Water-deprived animals exhibited a similiar pattern of behavior. Animals tested under water deprivation after their previous experience with food deprivation displayed stronger consummatory behavior than naïve animals. On the other hand, the behavior of the two groups of food-deprived subjects was the same although one group had experienced water deprivation sessions while the other group had not.

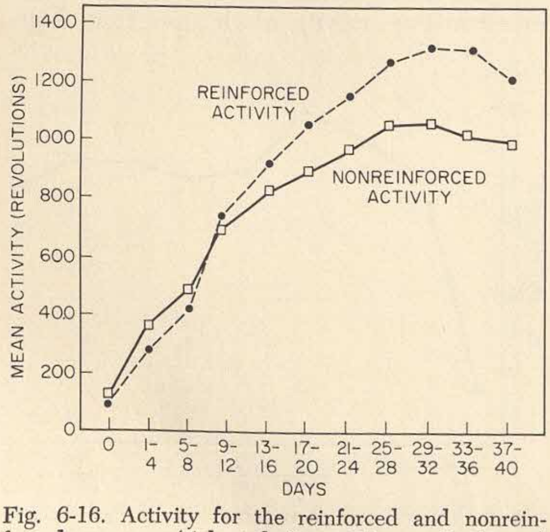


Fig. 6-16. Activity for the reinforced and nonreinforced groups. (Adapted from Finger, Reid, and Weasner, 1957)

INSTRUMENTAL BEHAVIOR Since American psychologists have been primarily preoccupied with learning and the examination of those variables which contribute to the learned response, it is not surprising that the influence of motivational variables has been extensively investigated. In keeping with the traditional distinction that has been made between need states and rewards, we shall organize the material in this section in this manner. Before proceeding, however, a measurement problem should be discussed.

Since the selection of what is to be measured in the instrumental response sequence represents an arbitrary decision, it is not surprising that investigators have used a variety of measures. Although there has been the implicit assumption that there is a high correlation among the various response measures, an examination of the experimental data (Campbell and Hilgard, 1936; Kellogg and Walker, 1938; Brogden, 1949; Hall and Kobrick, 1953) reveals that it is actually difficult to predict one response measure from a knowledge of another.

The problem, however, has not been one of attempting to predict one response from a knowledge of another, but rather, concern has been whether the varying response measures reflect corresponding changes taking place as a result of the manipulation of what has been acknowledged to be basic learning variables. For example, if a certain reinforcement procedure results in the latency of a response decreasing, does the amplitude of that response reveal a similar function?

Kessen's study (1953) illustrates this position. In this experiment, rats learned to avoid a shock by rotating a wheel as a function of the intensity of a CS (light). The response measures which were examined were: (1) latency of the response as measured from the onset of the presentation of the light; (2) probability of the response being made, (3) number of wheel turns made between trials, (4) number of extinction responses. Figure 6-17 reveals the findings for the three response measures examined during training, and it can be noted that they reflect similar functions as a result of the manipulation of the independent variable. In contrast, the extinction data showed no reliable differences among the varying intensities provided.

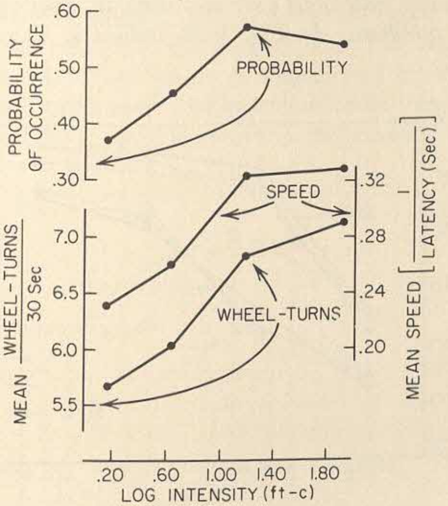


Fig. 6-17. Response strength as a function of log CS intensity. (Adapted from Kessen, 1953)

Many other investigators (Hillman, Hunter and Kimble, 1953; Kobrick, 1956), however, have been unable to find different response measures reflecting similar changes as a function of the manipulation of a particular independent variable.

Some investigators have attempted to handle this problem by considering only a single response measure. Estes (1950) has been interested only in probability of response. Meyer (1953) and Pubols (1960) have made a distinction between measures which involve time or rate of responding (i.e., latency, running speed) and those which are time independent (i.e., errors). The solution that these authors have proposed represents at least a partial answer to this problem, and our findings in this section will be categorized in these terms.

What is the relationship between the intensity of an organism's need and the acquisition of a learned response? A direct approach to this question would be to deprive groups of subjects of food or water for varying amounts of time and examine their performance in a learning situation.

If rate of response is used as a measure of acquisition, the experimental evidence is essentially unequivocal. The findings from most experimental studies (i.e., Deese and Carpenter 1951; Cotton 1953; Lewis and Cotton 1957; Davis 1957; Barry 1958) support the position that performance improves as a function of the intensity of a need.

As an example of the general findings in this area, Barry (1958) examined the speed of the running response of rats placed under 2½ hours (low need) or 26½ hours (high need) of food deprivation. Figure 6-18 shows running speeds

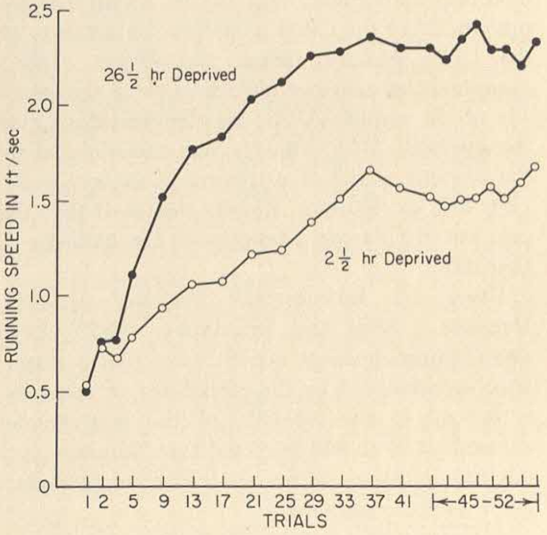


Fig. 6-18. Running speed as a function of hours of deprivation. (Adapted from Barry, 1958)

for the two groups of subjects. When more complex tasks are used, similar findings are obtained. Hillman, Hunter, and Kimble (1953) placed rats under either two or twenty-two hours of water deprivation and measured speed of running a ten unit multiple T-maze. Their findings revealed that the 22-hour deprived group traversed the maze significantly more rapidly than the 2-hour deprived group.

In an interesting study which has examined the mechanism underlying such differential responding, Cotton (1953) analyzed specific running responses as well as competing responses which were made by rats running a straight runway under varying amounts of food deprivation. Cotton found that animals under low deprivation states, in contrast to high, made many responses which competed with the basic running response. If the time taken to make these competing responses was excluded from the analysis, the findings revealed that the influence of the intensity of the need state on running time was virtually nonexistent. Cicala's study (1961), on the other hand, has not supported Cotton's findings. Rather, he has found that even when competing response trials are removed from the data, running speed varies as a function of the deprivation state.

When the probability of a response, as measured by errors or correct responses is used to measure performance, the experimental evidence is controversial. In classical conditioning studies, in which the UCS must be considered as the motivational antecedent, most studies reveal that the probability of the CR is related to the intensity of the UCS. Prokasy, Grant, and Myers (1958) examined the influence of four levels of the intensity of the airpuff (UCS) on the conditioning of the eyeblink. UCS intensity was manipulated by the amount of fall of a column of mercury—50, 120, 190, or 260 mm. Results indicated that the per cent of CRs was a function of the intensity of the UCS.

Using an instrumental learning situation, Hammes (1956) and Broadhurst (1957) have also obtained findings which reveal that performance, as measured by the probability of response, is related to the intensity of the motivational antecedent. It should be noted that Hammes used intensity of shock as his motivator, while Broadhurst used air deprivation.

A number of other studies, however, have not been able to demonstrate that performance, as measured by the probability of the response, is influenced by the intensity of the motivational antecedent. Although we previously noted that Hillman, Hunter, and Kimble (1953) obtained differences in running speed for rats placed under two versus twenty-two hours of water deprivation, there was no difference between these groups when the number of errors made was used as the performance measure. As these authors state, "There is, in short, no evidence that the number of errors is related to strength of motivation at any point in learning."

Studies by Meyer (1951), Warren and Hall (1956), and Miles (1959), in which monkeys have been used as the experimental subjects, have also been unable to find any relationship between the amount of the need as measured by hours of food deprivation and the learning of a discrimination problem. In the Miles (1959) study, naïve monkeys were placed under either one, five, or twenty hours of food deprivation and provided with both easy and difficult discrimination problems. As Fig. 6-19 indicates, there is a difference between the easy and difficult task, but there are no differences in performance as a function of the deprivation variable.

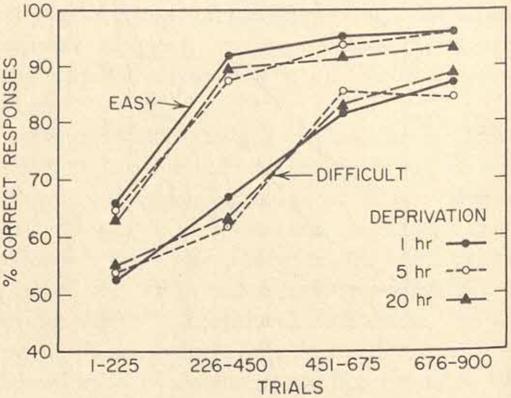


Fig. 6-19. Mean percentage correct responses as a function of trials and deprivation conditions. (Adapted from Miles, 1959)

Further research is needed to settle this controversy, but the critical variable may be related to the kind of motivational antecedent manipulated. There is the suggestion that probability of response measures may be most strongly influenced by those motivational antecedents which are of a noxious nature.

An interesting facet of this motivation-learning area relates to the interaction between intensity of the need state and the ease or difficulty of the learning task. The reference experiment here is one by Yerkes and Dodson (1908), who used dancing mice as experimental subjects and employed a visual discrimination task involving three degrees of difficulty. Different intensities of electric shock was used as the motivational antecedent. Results indicated that with the easy discrimination problem, correct responses increased as a function of the strength of the shock. With medium and difficult discriminations, increases in the intensity of shock resulted in first increasing but then decreasing the number of correct responses. Their findings which provided the basis for the Yerkes-Dodson law, indicated an optimum intensity of shock for a given degree of task difficulty. Studies by Hammes (1956) and Broadhurst (1957) have confirmed these earlier findings. In the Broadhurst study, the intensity of the motivational antecedent was manipulated by delaying the rat for either 0, 2, 4, or 8 seconds under water before releasing the animal into the learning situation. Three levels of difficulty of an underwater brightness discrimination were used. As Fig. 6-20 reveals, increases in the intensity of air deprivation resulted in superior performance on the easy task; on the other hand, slight increases in air deprivation resulted in an increase in performance for the difficult task, but as the deprivation period was further increased, inferior performance was obtained.

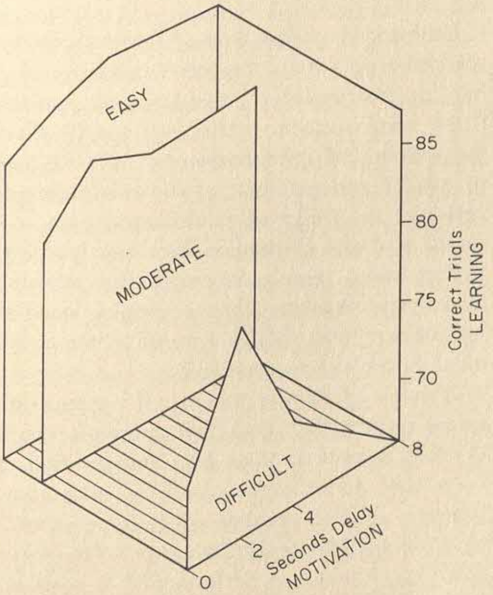


Fig. 6-20. Three-dimensional surface showing learning as a function of the intensity of air deprivation and task difficulty. (Adapted from Broadhurst, 1957)

A number of psychologists, interested in the learning area, have called attention to a distinction between learning and performance. This distinction has been made most explicit by Hull (1943), who postulated that performance was a function of habit strength (learning) multiplied by drive or intensity of the motivational antecedent. It can be noted in this formula that performance, but not learning, is influenced by the intensity of the organism's need. A number of investigations have been made examining this point of view. In the studies which we have just reviewed, the performance curves reflect a learning as well as a motivation component and it is not possible to tease out the influence of each.

A frequently used procedure to make this separation has been to use an extinction operation, since it has been posited that learning does not take place during extinction. If it is desired to examine the influence of the intensity of the organism's need on performance, with learning held constant, all subjects are placed under the same amount of need, and then given an equal number of reinforcements for the learning of a specific task. Following such training, subjects are divided into varying groups, with each group being placed under a different need intensity, and the response extinguished. Differences among the varying groups in their resistance to extinction presumably reflects a performance measure, which is influenced only by the operation of the varying intensities of need which are present.

The reverse situation is used to examine the influence of need intensity on learning or habit strength. Here, subjects are first placed under varying levels of a need state and given a constant number of reinforcements. Following such training, the intensity of the need is equated for all groups and resistance to extinction measured. In this case, it is assumed that performance differences among the groups during extinction trials should reflect the differences in learning which took place during the training.

The experimental findings of Perin (1943), Koch and Daniel (1954), Saltzman and Koch (1948), and Yamaguchi (1951) support the position that performance, as measured by experimental extinction and with habit strength held constant, is a function of the intensity of the organism's need state. The results of these studies are similar to the findings reported earlier, in which investigators examined performance over acquisition trials as a function of a given intensity of need.

When habit strength is investigated as a function of need, the reference experiment has been performed by Strassburger (1950), who had three groups of rats learn a bar-pressing response. With experimental group A, subjects were deprived of food for either 4, 11, 23, or 47 hours, with each group receiving thirty reinforcements. Experimental group B consisted of animals deprived for ½, 1, 4, or 23 hours and permitted to receive ten reinforcements. Experimental group C consisted of rats deprived for either 1, 11, 13, or 47 hours and given only a single reinforcement. All animals in all groups were extinguished under twenty-three hours of reinforcement. Although it was evident that resistance to extinction was a function of the number of reinforcements, it was not related to the amount of deprivation present during the original training situation.

In many of the experimental studies which have examined the role of motivational determinants on habit strength or performance, a major difficulty is related to the inability of the experimenter to control for stimulus generalization effects, which occur when the organism is changed from one deprivation state during the training period to another deprivation state during extinction. In the Strassburger study (1950), for example, we noted that animals were placed under either 4, 11, 23, or 47 hours of deprivation during training, with all groups extinguished under twenty-three hours of deprivation. The problem is that no allowance has been made for the fact that the 23-hour group has not had its deprivation schedule changed while all of the other groups have. Both Yamaguchi (1952) and Barry (1958) have shown that a change in the deprivation condition has an effect of its own, in addition to the effect that the specific deprivation state may have. Cognizant of this problem, a number of investigators have attempted to solve it by using a factorial design, in which the effects of deprivation on both habit strength and performance are examined simultaneously.

Unfortunately, the reasonably consistent findings relating level of need to habit strength and performance obtained in nonfactorial designed studies has given way to equivocality when the findings of factorial studies are considered. Teel (1952) using a probability of response measure, and Campbell and Kraeling (1954) using rate of response, have reported findings which indicate that deprivation level affects neither habit strength nor performance in any systematic way. Hillman, Hunter, and Kimble (1953) have found some support for the intensity of a need state influencing performance but not habit, while Lewis and Cotton (1957) and Barry (1958) have found that amount of deprivation not only affects performance, but that it also has some influence on habit as well.

It is recognized that the distinction between learning and performance is in accord with certain common-sense notions of behavior, and it appears to be a useful distinction in certain situations. There is some question, however, as to whether any design utilizing an extinction measure is an appropriate method for separating the two constructs. The assumption that learning does not take place during extinction, for example, is questionable, for although primary reinforcement is not available, it is quite likely that secondary reinforcing stimuli are present, and as we have previously noted, such stimuli can mediate learning.

Learning As a Function of Reinforcement. The work of Hull (1943) has been primarily responsible for instrumental behavior being related to three reinforcement variables; specifically, (1) the number of reinforcements, (2) the amount, or quality and quantity, of the reinforcing agent, and (3) the delay of reinforcement. It is interesting that the distinction between learning and performance, which occupied the attention of many experimenters when examining need states, has been almost totally ignored when reinforcement variables are considered.

Number of Reinforcements. If we concern ourselves only with a continuously reinforced series of trials, a most common experimental finding has been that acquisition as well as extinction is a function of the number of reinforcements provided. Such a generalization appears to be true regardless of whether performance is measured in terms of rate or probability of response. As an example of the findings in this area, Felsinger, Gladstone, Yamaguchi, and Hull (1947) trained

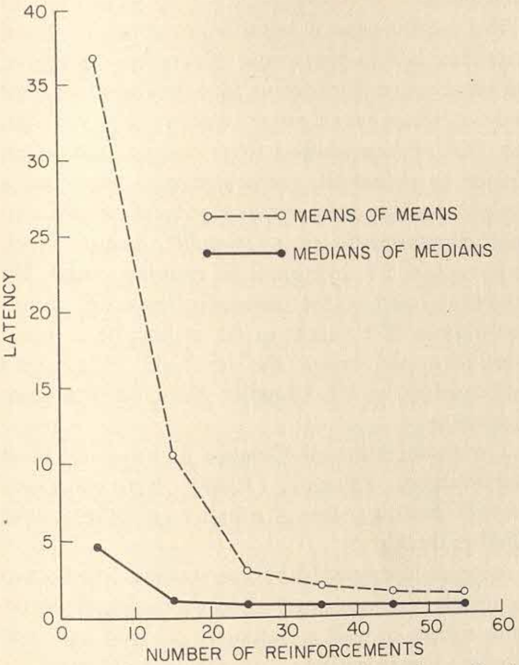


Fig. 6-21. Latency as a function of number of reinforcements. (Adapted from Felsinger, Gladstone, Yamaguchi, and Hull, 1947)

rats to make a bar-pressing response in a Skinner box. A single reinforced response was provided every twenty-four hours. Figure 6-21 indicates how latency decreased as a function of the number of reinforced trials.

Amount of Reinforcement. Most experimental findings in this area have indicated that the rat's rate or probability of responding during acquisition training will reflect different amounts of reward obtained in the experimental situation. Logan's extensive investigations (1960) in this area have resulted in his hypothesizing that the relationship between amount of reward and performance is as Fig. 6-22 indicates. Logan's findings have been supported by a number of investigators who have manipulated reward by varying the concentration of a sucrose solution.

When extinction measures are used, most of the findings parallel those which have been reported for acquisition. It should be noted, however, that the extinction measures simply reflect differences in performances, which were present at the end of training and which continue to be manifested throughout the extinction trials (Metzger, Cotton, and Lewis, 1957).

Of some interest are the experiments of Miller and Estes (1961) and Estes, Miller, and Curtin (1962), who, using nine-year-old boys as subjects in one study, and college students in the second, found that the amount of monetary reward, even in a 50:1 ratio, failed to reflect performance differences in a visual discrimination task.

One interesting aspect of the amount of reward variable is related to the experience that the organism has had with rewards in the experimental situation. Meyer (1951*a*) has posited that a major determinant in the facilitative characteristics of a specific amount of reward is related to the range of rewards provided the organism, and this certainly appears to be true. In a study by Schrier (1958), five groups of monkeys were used in which four groups were designated as "nonshift." These groups received either 1, 2, 4, or 8 pellets as a reward on every problem used in the series of discrimination problems. The fifth group was designated as a "shift" group, and this group experienced each of these reward amounts, but with a different amount being provided on each problem. Figure 6-23 reveals performance as a function of the amount of reward received. Note that for the "nonshift" group, rewards of 1, 2, or 4 pellets produced similar performance, and it required 8 pellets to facilitate better performance. On the other hand, the "shift" group revealed an increasing percentage of correct responses following each increase in reward.

It seems reasonable to assume that the re-

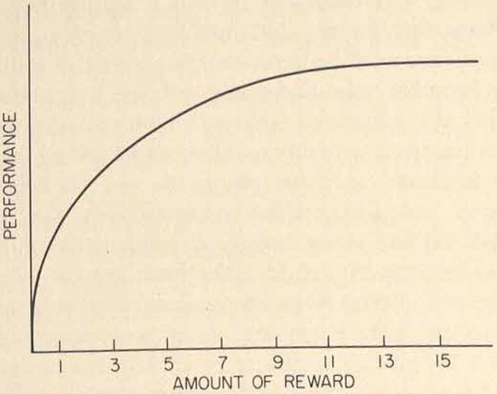


Fig. 6-22. Hypothetical curve showing the relationship between performance and amount of reinforcement. (Adapted from Logan, 1960)

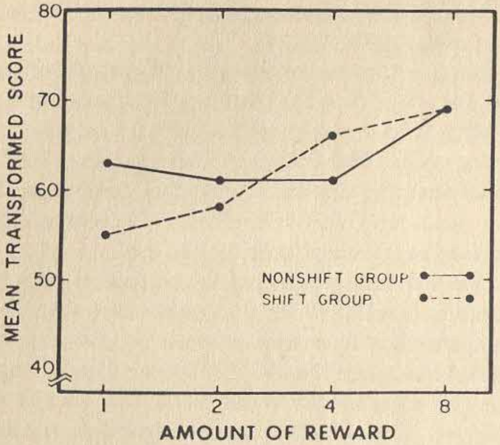


Fig. 6-23. Mean arcsin-transformed percentage of correct responses as a function of the amount of food reward. (Adapted from Schrier, 1958)

ward experiences of an organism result in reward having relative rather than absolute characteristics. Such a position has been proposed by Bevan and Adamson (1960), who have used Helson's adaptation level theory found in psychophysics (1959) as a point of departure for work in this area. More specifically, they have proposed that the magnitude of reinforcement ". . . is defined by the difference between the intensity of the present stimulus process and an internal norm derived from a pooling of previous relevant information." One study using humans (Bevan and Adamson, 1960) and one using rats (Black, Adamson, and Bevan, 1961) have confirmed the relative properties of reinforcement and thus have supported, at least in part, the psychophysical model. In this latter study, three groups of rats received ten trials per day in a shuttle box in which (*a*) strong, (*b*) medium, or (*c*) weak shock was used to motivate the animals to escape to the other side. Following each day's shuttlebox trials, ten additional test trials were provided in a straight runway with medium shock being used to motivate all three groups to run. A control group was used, which received only the test trials on the straightaway. A comparison of the performance curves for the three groups which received pretest shuttlebox experience indicated that the fastest running times were associated with the group which received the weak shock in the shuttlebox, while the slowest running times were obtained from the group receiving the strong shock. The relative effects of reinforcement are obvious since the same level of shock in the runway resulted in performances which varied as a function of the amount of shock obtained in the shuttlebox.

The relativeness of reinforcement is also found in studies in which contrast effects have been obtained. One of the earliest investigators to report such an effect was Crespi (1942), who reported that if a rat was shifted from one or four pellets of food to sixteen, its performance in traversing a straight runway rose above the level of performance characteristic of a 16-pellet group, which had reached its asymptote in running speed. On the other hand, if the amount of reward was reduced from 256 pellets or 64 to just 16, running speed dropped below the level of performance characteristic of the 16-pellet group at their limit of practice.

The replicability of Crespi's findings has been controversial. Zeaman (1949) has confirmed Crespi's findings, but a number of others have failed to do so.

A logical outgrowth of those studies, which have separately demonstrated that the amount of reinforcement as well as intensity of need can contribute to performance, are the experimental investigations recently undertaken, in which these variables have been manipulated simultaneously. Studies by Reynolds and Pavlik (1960), Weiss (1960), Stabler (1962), and Ehrenfreund and Badia (1962) are all relevant. Unfortunately, agreement among them is lacking. Reynolds and Pavlik (1960) provided ninety male albino rats with seventy-two reinforced trials on a straight runway. Three magnitudes of reinforcement (.1, 1.0, and 2.0 g of wet mash) were provided and three levels of food deprivation employed (3, 22, or 44 hours). Running speed, as measured from start box to goal box, grew more rapid as the levels of both variables increased, but no significant interaction was obtained. Weiss' experiment (1960) was similar to the Reynolds and Pavlik study. A runway task was used to examine the influence of four levels of food deprivation (2, 12, 24, or 46 hours) and four amounts of reinforcement (1, 3, 6, or 12 pellets). Response measurements included starting as well as running speed. Starting speed data supported the Reynolds and Pavlik findings, in that this response was an increasing function of reward at each of the four levels of deprivation, but no interaction effects were obtained. Running speed findings, however, fell short of significance, when asymptotic running speeds were examined, although an analysis of early trials suggested that running speed findings paralleled those obtained with starting speed.

Ehrenfreund and Badia (1962), using two levels of deprivation (85 or 95 per cent of body weight) and two amounts of reinforcement (45 or 260 mg of food) also found that increases in either deprivation level or reinforcement resulted in increasing performance in the runway. A plotting of the acquisition trials revealed, however, that under conditions of either high drive or high reward, the adding of drive or incentive increments would result in little change in performance, at least until the organism's habit strength developed some appreciable magnitude. Such a prediction, the authors argued would not be forthcoming from the findings of either Reynolds and Pavlik (1960) or Weiss (1960); thus the Ehrenfreund and Badia findings do not confirm the results of these earlier investigators.

Finally, Stabler (1962), also using the runway apparatus with two levels of food deprivation (4 and 20 hours) and three levels of reinforcement (8, 16, or 32 per cent sucrose solution) found that the analysis of speed of running data for the last thirty trials (sixty trials were provided) resulted in a significant interaction between level of food deprivation and concentration of sucrose. Running speed for the 20-hour deprived group increased as a function of sucrose concentration, but contrary to the findings of other investigators, the four-hour deprived group exhibited a decline in running speed as a function of sucrose concentration.

Since most experimental investigations revealed rate of response to reflect both deprivation and amount of reinforcement variables, the majority of the factorial studies provide confirming evidence of these effects, although the characteristics of the interaction effects are controversial. Until we begin to introduce some standardization into response measurement, techniques for inducing deprivation as well as providing similarity of reinforcement, we shall be plagued with controversies of this variety.

Delay of Reinforcement. In an examination of the influence of delay of reinforcement on the learning of an instrumental response, two types of delay must be recognized; (1) temporal delay, and (2) response delay.

Temporal delay involves the introduction of a time period between the making of the instrumental response and the securing of reinforcement. An early study by Perin (1943) indicated that, if a delay of thirty seconds or more was introduced between the animal's making a bar-pressing response and its receiving of food, the animal was unable to learn. Later studies by Perkins (1947), Grice (1948), and others have indicated that this time interval should be further reduced. In Grice's study, groups of rats were required to learn a black-white discrimination problem, and delays of 0, .5, 1.2, 2.5, 5 and 10 seconds were introduced between the animal's making the correct response and its securing of food. It should be noted that all of the delay periods used in Grice's study were not of the temporal variety. In some cases, a response delay was utilized. The criterion of learning was eighteen correct responses out of twenty trials. Learning curves for the varying groups revealed that the zero delay group learned most rapidly followed by the .5, 1.2, and 2.0 groups. When a delay of five seconds was introduced, the animals experienced great difficulty in learning; by 700 trials, the animals were responding at only about 80 per cent correct. The group provided a delay of ten seconds did not learn, although in some cases animals were given as many as 1400 trials.

Fowler and Trapold (1962) have shown that for rats running down a charged alley, speed of running is significantly decreased when reinforcement, defined by shock reduction, is delayed following the behavior which is instrumental in obtaining it. The delays used by these investigators were 0, 1, 2, 4, 8, or 16 seconds. These findings, it can be noted are analogous to appetitive reward studies which we have reported, and they suggest that similar laws underlie both types of experimental situations.

Spence (1956) has theorized that learning under temporal delay involves two factors, the first of which involves *K*, an incentive motivation variable, which involves the classically conditioned consummatory response. He has proposed that the consummatory response is weakened as a function of the length of the delay, and since this response is related to the strength of *K*, any reduction in *K*, of course, is reflected in poorer learning.

The second, and presumably more potent factor in producing poorer performance with delayed reinforcement is related to interfering responses, which take place during the delay period. When a delay is introduced, the organism makes a variety of extraneous responses during this interval, and since these responses are made to the same stimulus components as is the to-be-learned instrumental response, they likewise become conditioned to them. Since such responses take place during training trials, poorer learning is reflected in the increased time taken to run off the appropriate response sequence.

An analysis of the responses which are made during the delay interval would indicate that if the organism can maintain an orientation toward the response manipulandum (i.e., bar in a Skinner box) it is possible to reduce or perhaps eliminate any response decrement which might take place. On the other hand, if the delay period employed is beyond the limit to which an organism's orientation can be maintained, or if the situation is such that it does not permit the organism to make such an orientation, competing responses would be expected to occur, which in turn would result in performance decrement. Spence's analysis (1956) of a number of experiments performed in his laboratory, using rats as subjects, have supported such a position.

Recently, Champion and McBride (1962) examined the role of activity during the delay interval with humans. A paired-associate task was provided, and the subjects were required to read a list of words between the response which was made by the subject and the exposure of the word. Thus, the word *needle* was used as a stimulus word to which the individual had to respond with *thread*. Between the subject's response, and the exposure of the response word in the apparatus, the subject had to read a list of words: i.e., sharp, pin, sew, steel, eye, instrument, point, thimble, sewing. Two- and five-second delay periods were used, with half of the subjects in each delay period required to read such a list of words aloud during the delay, whereas the remaining subjects were instructed to fixate the slot in the exposure apparatus. Response latencies for the varying groups are indicated in Fig. 6-24 and in essence confirm Spence's position that performance is related to the characteristics of the competing responses which fill the delay period.

Response delay is found in those situations in which the delay interval is filled by the organism having to make specific responses. Spence (1956) has hypothesized that the important variable in this type of situation, unlike temporal delay, is related to the strength of fractional anticipatory goal response. The longer the chain of responses which make up the total behavior pattern, the less the similiarity of the stimulus cues at the beginning and ends of the chain and hence the less strong will be the generalized fractional anticipatory goal response. Since the fractional anticipatory goal response also contributes to the strength of *K*, it is the difference in the value of this construct which accounts for the differential learning which takes place when the response delays are manipulated.

MOTIVATIONAL THEORIES At this point we should like to briefly present some of the current motivational theories, emphasizing those which have been presented in the experimental tradition. A summary of many of the personality or clinical theories of motivation can be found in Madsen (1961).

Hull–Spence. The motivational theory of Hull (1943), modified by Spence (1956), is undoubtedly the most clearly developed of present motivational positions. Behavior is postulated to be a product of a motivational variable, drive (*D*), and habit strength (*sHr*), with the drive state being conceptualized as operating exclusively to energize habits or associative tendencies. Two general sources of drive have been postulated, primary and secondary. Hull (1943) originally believed that the primary sources of drive were generated from an organism's need state, but as

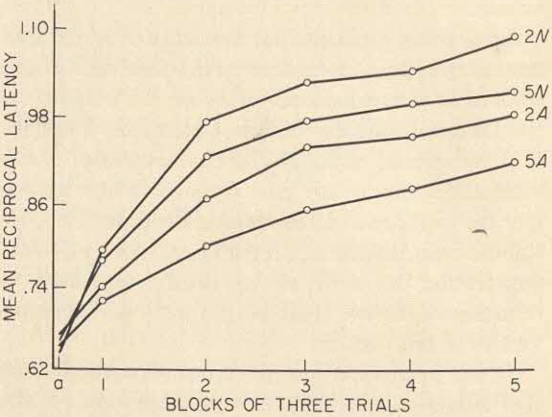


Fig. 6-24. Performance curves for groups learning with 2- and 5-second delay of reinforcement, and with activity (*A*) or inactivity (*N*) during the delay period. (Adapted from Champion and McBride, 1962)

we have noted earlier, considerable criticism has been directed at this position. As a result, these antecedents have been elaborated upon, revised, or expanded. Spence's (1956) classification divides such sources into: (1) appetitional and (2) aversive, with appetitional sources defined in terms of those maintenance schedules, periods of deprivation, etc., which are related to survival of the organism or of the species. Aversive sources include noxious forms of environmental stimulation. Other investigators, working within the Hull–Spence position, have given consideration to other sources: curiosity (Berlyne, 1950), frustration (Brown and Farber, 1951, Amsel, 1958), and the withholding of opportunities to play or explore (Brown, 1961). Secondary drives are conceptualized to arise from primary ones, and it will be recalled that we have examined some of the experimental evidence obtained in this area.

A basic assumption made with regard to the operation of drives is that the varying sources of drive present in the organism summate in order to energize response tendencies. Such a position has given rise to a whole host of experimental studies investigating the role of irrelevant drives.

A second motivational construct in the system, more closely identified with Spence than Hull, is incentive motivation, or K. It is assumed that K combines with D in an additive manner and, like D, multiplies reaction tendencies. When an organism makes a consummatory response in an instrumental learning situation, associations via classical conditioning are formed between the stimuli which are present in the situation and this response. Through stimulus generalization, stimuli found in other parts of the experimental apparatus are assumed to acquire the capacity to elicit those parts of the consummatory response which can be made without the presence of the goal object. This "part" response has been designated as a fractional anticipatory goal response and denoted as r_g. It is r_g which forms the basic mechanism which underlies K. The term expectancy is frequently used in connection with r_g, since the observation of these fractional anticipatory goal responses constitute an objective criterion that the organism is expecting or anticipating the presence of the goal object. Since r_g represents a classically conditioned response, a number of hypotheses regarding its operation, and hence the strength of K, have been made. One would assume that the strength of r_g should vary as a function of the number of conditioning trials given in the goal box, while its strength at points distant from the goal box should be a function of the similarity of the environmental cues found at that point and those found in the goal box. Finally, any property of the goal object that produces an unconditioned consummatory response of different intensity or vigor will presumably contribute to the value of K.

Drive Stimulus Theory. In contrast to the Hull–Spence position is a drive stimulus theory of motivation, developed primarily by Estes (1958), and basically related to his statistical association model of learning. Briefly, this model assumes that the stimulus complex present on any given trial consists of a group of stimulus elements, and that the associations of any stimulus element with a response takes place through contiguity. Moreover, this relationship is of full associative strength on the occasion of its first pairing. Increased probability of responding, or learning, takes place as a function of practice, since each trial provides the opportunity for more and more stimulus elements to become conditioned to the response.

Within such a system, Estes (1958) maintains that separate motivational constructs, i.e., drive, need, etc., are not necessary in the explanation of learning phenomena. Rather, he has held that motivational variables contribute to performance only insofar as they represent a set of stimulus elements which represent a part of the total stimulus pattern to which a response is attached. As Estes (1958) has written, ". . . drive stimuli are simply stimuli, with no special properties whatsoever."

Estes (1958) has been primarily concerned with relating his theoretical position to deprivation experiments, and he has pointed out that, in addition to the set of drive stimuli arising from the deprivation operation, other sets of stimuli arise from satiation effects, as well as from the external environment, thus forming a total stimulus complex. Each set of stimuli can be assigned a weight, with such a weight being a function of the number of stimulus elements comprising the set, as well as the average sampling probability of the elements. It is assumed that the weight of stimuli arising from deprivation should increase, while the weight of stimuli arising from satiation operations should decrease, as a function of deprivation time. Finally, it is hypothesized that the probability of any response on any given trial is determined by the weight of cues which are present and have been conditioned to the response, divided by the weight of all cues which are present.

A number of different manipulation of drive experiments have been examined by Estes, and he has been able to demonstrate that the findings from these experiments can be interpreted as readily within a drive stimulus position as within the Hull–Spence position, which assumes that motivational variables have an energizing function.

As an example of this kind of interpretation, we can utilize a hypothetical study in which rats have been trained to run a straightaway under twenty-three hours of deprivation, and then extinguished under deprivation times of less than twenty-three hours. The experimental results from such a study would indicate that running times during the extinction trials would be a function of the number of hours of deprivation under which the response was extinguished. The drive stimulus interpretation of these findings would be that, since the animals were originally trained to respond under twenty-three hours of deprivation, the running response was conditioned to a stimulus complex, in which the set of drive stimuli arising from the deprivation state had considerable weight. When deprivation states of less than twenty-three hours were introduced, it would be assumed that many of the 23-hour deprivation stimulus elements would be replaced by extraneous and unconditioned stimuli, which would normally elicit responses which were incompatible with running. Thus, the weight of those cues which had been conditioned to the running response would be drastically decreased, with such reduced weight being reflected in decreased probability (but not vigor) of running. It would be hypothesized, however, that whenever an animal did run, running time would be constant, regardless of deprivation level. With regard to this position, Estes has referred to Cotton's study (1953) for support, which demonstrated essentially no differences in running time among 0, 6, 16, and 22 hours of deprivation if those trials, on which competing responses have occurred, are eliminated from the analysis.

AFFECTIVE STATE THEORIES *P. T. Young.* A prominent theory of motivation based upon the affective state of the organism has been proposed by Young (1955, 1959, 1961). Trained under Titchener, it is not surprising that Young has long held that any complete psychology must consider the role of conscious content, and that affective processes represent an important component of such experience. The arousal of the affective process is viewed as being basic to the study of motivation—in fact, Young states that such arousal is motivating in the sense that it evokes action, regulates the course of behavior, and organizes patterns of approach and avoidance behavior. The affective process should not be confused with sensory processes, however, since sensory processes convey specific information to the brain centers which provide the basis for the discriminative capacity of the organism, while affective processes convey little or no information of this variety.

Young's position can be better understood by reference to the following experiment (Young and Shuford, 1954). Sated rats were run in a type of open field apparatus, in which a concentration of sucrose solution (2, 6, 18, or 54 per cent) was provided at the end of the run. Animals were permitted to have contact with the solution for 1, 4, or 16 seconds. Trials were given one per day for eighteen days. The findings of this experiment are revealed in Fig. 6-25 and indicate behavior which is not unlike that found in many motivation studies, some of which we have reviewed, in which the quantity or quality of the reinforcement has been manipulated.

In analyzing the findings of this study, Young has rejected any explanation based upon the action of need or drive states, since his subjects were satiated, but rather he has posited that the sweet taste of sucrose solution aroused an affective process in the animal. If the rat had experienced shock in this situation, avoidance behavior would have been observed, and a negative affective process would have been inferred. In examining the affective process, Young (1955) has pointed out that such processes differ not only in being positive or negative, but also vary as a function of intensity, duration, and frequency. In the study cited, the concentration of sugar solution was a variable related to affective intensity, while the amount of contact time was an indicator of duration. The variations in approach behavior found in Fig. 6-25 reflect differences in the affective state of the subjects. It is concluded that "The data justify the postulate that the strength of a motive to approach, as shown by speed of locomotion and latency is directly related to the in-

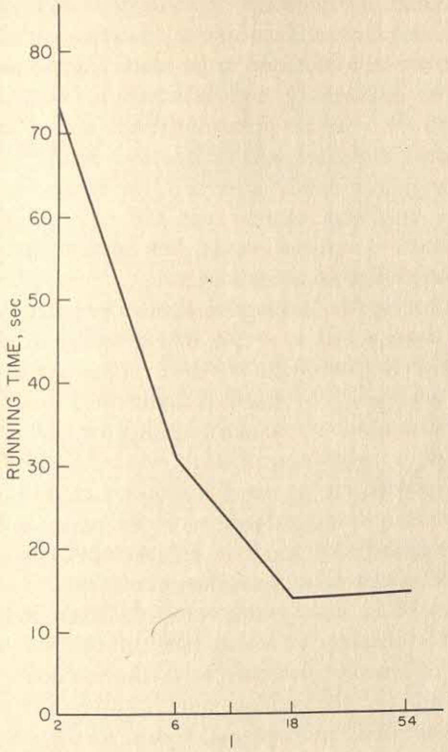


Fig. 6-25. Median running time on the first trial as a function of the concentration of sucrose solution. (Adapted from Young and Shuford, 1954)

tensity, duration, and frequency of affective arousals." (Young and Shuford, 1954).

Continuing with the study cited, Young (1961) writes that, since the strength of the approach motive as measured by running time was measured twenty-four hours following contact with the sugar solution, this meant that there were at least two factors which must be included in the concept of a motive: (1) the primary affective arousal state, and (2) secondary motivation, or a learned neural organization that orients the animal toward the goal and regulates the pattern of approach.

Many current motivational concepts are examined as well as reinterpreted. Need states are important only as a factor which influences the characteristics of the incentive which in turn influences affective arousal. Palatability is related to taste, aroma, and texture of a foodstuff, and it refers to the hedonic value of an incentive, which is also related to affective arousal. In contrast to many contemporary learning theorists who have held that motivation is necessary for learning to occur, Young (1961) posits that affective arousal is not necessary for human learning. The college sophomore who learns a list of nonsense syllables, Young avers, learns regardless of whether the task is interesting, boring, or affectively neutral. "What appears necessary for learning to occur is the simultaneous excitation of contiguous neurons."

D. C. McClelland. A second theoretical position, which uses the organism's affective state as a point of departure for a motivational theory, is found in the works of McClelland and his associates. Adopting the position that in order best to understand the nature of human motivation, it is most appropriate to work with the kinds of motives which comprise a large share of the human organism's everyday striving, an extensive experimental program has been undertaken to achieve this objective.

A motive is defined as a relatively stable disposition to strive for rather general kinds of goal objects. The attainment of these goals is accompanied by feelings of satisfaction, while disruption of goal directed activity, or the nonattainment of a desired goal, is accompanied by feelings of dissatisfaction. A basic assumption is that these dispositions or motives are learned, with their acquisition being dependent upon the arousal of affective states in the individual. Using the positions of Helson (1947) and Hebb (1949), affective arousal is assumed to be a function of the size of the discrepancy between the stimulus (perception) and the adaptation level (expectation) of the organism. According to this position, pleasure is maximal when the event as perceived is moderately discrepant from the expectation of the individual.

Motives, or dispositions, it is assumed, have their origins in childhood experience and are relatively enduring. They are carried about from situation to situation, and they constitute the core of what is called personality. In contrast to the construct of motive, motivation refers to a temporarily aroused state, produced when the cues of a situation elicit an expectancy of goal attainment which engages the motive. Motivation is thus a tendency to perform an act and is a joint function of the motive of the individual and the expectancy or cognition that appropriate instrumental behavior will provide satisfying consequences. It can be noted that the particular aim of the momentary state of motivation is situationally defined, and that it relates to the final strength of the response pattern. In making this distinction between motive and motivation, Atkinson (1958) has written: "One may experience strong *motivation* to eat a hamburger or a piece of lemon pie; the general aim of the *motive* is the satisfaction that accompanies eating something." From such a position, it follows that motivation is ever changing. It is a joint function of a stable element, the motive, and a changing element, the momentary expectancy of attaining some satisfaction of the motive which has been aroused by situational cues.

A basic assumption made by McClelland and his associates is that human motives can be measured, using phantasy or imaginative responses. An early study was designed to examine this assumption by using a condition which everyone could accept as motivating. Atkinson and McClelland (1948) deprived human subjects of food for 1, 4, or 16 hours and then had them write brief imaginative stories to pictures which were flashed on a screen for twenty seconds. Four questions were provided to guide the subject's writing: (1) What is happening? Who are the persons? (2) What has led up to this situation? That is what has happened in the past? (3) What is being thought? What is wanted? By whom? (4) What will happen? The content of the stories were then analyzed on the basis of a number of characteristics related to food deprivation as well as to instrumental and consummatory activity in which food played an important role. Results, which indicated that some of the scoring categories changed as a function of hours of food deprivation, sufficiently encouraged the authors to clear the track for work on their main objective—the measurement of an achievement motive. Such a motive was defined as a learned, affective anticipation of the pleasurable consequences of success in situations where an individual's performance is evaluated.

In one of the early studies examining the achievement motive, McClelland, Clark, Roby, and Atkinson (1949) had college students take a series of short tests (anagrams, scrambled words, etc.) under one of the following "arousal of achievement" conditions: (1) a relaxed condition in which the tasks were introduced casually as part of an experimental study; (2) neutral conditions in which the tasks were described as experimental, but in which the subjects were urged to do their best; (3) a failure condition in which the tests were indicated to be standardized measures of intelligence and leadership, but norms read to the students following their performance indicated that they had failed; and finally (4) a success-failure condition which was similar to the failure condition except that the subjects were permitted to succeed on the first part of the test and then failed on the whole test.

Following the taking of these tests, the subjects were asked to write five-minute stories in response to four pictures which appeared to depict achievement-related situations. Results of content analysis of the stories indicated that when a single *n* achievement score was computed for each subject, the mean of these scores increased significantly in accordance with the presumed increase in induced need for achievement from the relaxed to neutral to the failure conditions.

Since this early study as Atkinson's volume (1958) testifies, a whole host of studies have been conducted, not only to further examine the operation of the achievement motive, but also to isolate other motivational states. At the present time, human motives, which have been more or less successfully aroused include the affiliation motive, the sexual motive, and a power motive in addition, of course, to the achievement motive.

REFERENCES

ADLERSTEIN, A., AND FEHRER, E. The effect of food deprivation on exploratory behavior in a complex maze. *J. comp. physiol. Psychol.*, 1955, **48**, 250-253.

ALLPORT, G. W. *Personality, a Psychological Interpretation.* New York: Henry Holt and Company, Inc., 1937.

AMSEL, A. The role of frustrative nonreward in noncontinuous reward situations. *Psychol. Bull.*, 1958, **55**, 102-119.

———. Frustrative nonreward in partial reinforcement and discrimination learning: Some recent history and a theoretical extension. *Psychol. Rev.*, 1962, **69**, 306-328.

———, AND HANCOCK, W. Motivational properties of frustration: III. Relation of frustration effect to antedating goal factors. *J. exp. Psychol.*, 1957, **53**, 126-131.

———, AND PENICK, E. C. The influence of early experience on the frustration effect. *J. exp. Psychol.*, 1962, **63**, 167-176.

———, and Roussel, J. Motivational properties of frustration: I. Effect on a running response of the addition of frustration to the motivational complex. *J. exp. Psychol.*, 1952, **43**, 363-368.

———, and Work, M. S. The role of learned factors in "spontaneous" activity. *J. comp. physiol. Psychol.*, 1961, **54**, 527-532.

Anand, B. K. and Brobeck, J. R. Hypothalamic control of food intake in rats and cats. *Yale J. Biol. & Med.*, 1951, **24**, 123-140.

Anderson, E. E. The externalization of drive: I. Theoretical considerations. *Psychol. Rev.*, 1941, **48**, 204-224.

Andersson, B. The effect of injections of hypertonic NaCl-solutions into different parts of the hypothalamus of goats. *Acta physiol. Scandinav.*, 1953, **28**, 188-201.

———, and Larsson, S. An attempt to condition hypothalamic polydipsia. *Acta physiol. Scandinav.*, 1956, **36**, 377-382.

———, and McCann, S. M. Drinking, antidiuresis and milk ejection from electrical stimulation within the hypothalamus of the goat. *Acta physiol. Scandinav.*, 1955, **35**, 191-201.

Armus, H. L., and Garlich, M. M. Secondary reinforcement strength as a function of schedule of primary reinforcement. *J. comp. physiol. Psychol.*, 1961, **54**, 56-58.

Atkinson, J. W. (ed.). *Motives in Fantasy, Action, and Society.* Princeton, N.J.: D. Van Nostrand Company, Inc., 1958.

———, and McClelland, D. C. The projective expression of needs. II. The effect of different intensities of the hunger drive on thematic apperception. *J. exp. Psychol.*, 1948, **38**, 643-658.

Baker, R. A. The effects of repeated deprivation experience on feeding behavior. *J. comp. physiol. Psychol.*, 1955, **48**, 37-42.

Barnes, G. W., and Kish, G. B. Reinforcing properties of the onset of auditory stimulation. *J. exp. Psychol.*, 1961, **62**, 164-170.

Baron, A. Functions of CS and US in fear conditioning. *J. comp. physiol. Psychol.*, 1959, **52**, 591-593.

Barry, H., III. Effects of strength of drive on learning and on extinction. *J. exp. Psychol.*, 1958, **55**, 473-481.

Bash, K. W. An investigation into a possible organic basis for the hunger drive. *J. comp. Psychol.*, 1939, **28**, 109-134.

Bellows, R. T. Time factors in water drinking in dogs. *Amer. J. Physiol.*, 1939, **125**, 87-97.

Berkun, M. M., Kessen, M. L., and Miller, N. E. Hunger-reducing effects of food by stomach fistula versus food by mouth measured by a consummatory response. *J. comp. physiol. Psychol.*, 1952, **45**, 550-554.

Berlyne, D. E. Novelty and curiosity as determinants of exploratory behavior. *Brit. J. Psychol.*, 1950, **41**, 68-80.

———. *Conflict, Arousal, and Curiosity.* New York: McGraw-Hill Book Company, Inc., 1960.

Bernstein, L. M., and Grossman, M. I. An experimental test of the glucostatic theory of regulation of food intake. *J. clin. Investig.*, 1956, **35**, 627-633.

Bersh, P. J. The influence of two variables upon the establishment of a secondary reinforcer for operant responses. *J. exp. Psychol.*, 1951, **41**, 62-73.

Bertocci, P. A. A critique of G. W. Allport's theory of motivation. *Psychol. Rev.*, 1940, **47**, 501-532.

Bevan, W., and Adamson, R. Reinforcers and reinforcement: their relation to maze performance. *J. exp. Psychol.*, 1960, **59**, 226-232.

Bexton, W. H., Heron, W., and Scott, T. H. Effects of decreased variation in the sensory environment. *Canad. J. Psychol.*, 1954, **8**, 70-76.

Black, R., Adamson, R., and Bevan, W. Runway behavior as a function of apparent intensity of shock. *J. comp. physiol. Psychol.*, 1961, **54**, 270-274.

Bousfield, W. A., and Elliot, M. H. The effect of fasting on the eating behavior of rats. *J. genet. Psychol.*, 1934, **45**, 227-237.

Bower, G. H. The influence of graded reductions in reward and prior frustrating events upon the magnitude of the frustration effect. *J. comp. physiol. Psychol.*, 1962, **55**, 582-587.

———, and Miller, N. E. Rewarding and punishing effects from stimulating the same place in the rat's brain. *J. comp. physiol. Psychol.*, 1958, **51**, 669-674.

Broadhurst, P. L. Emotionality and the Yerkes-Dodson law. *J. exp. Psychol.*, 1957, **54**, 345-352.

Brobeck, J. R., Tepperman, J., and Long, C. N. H. Experimental hypothalamic hyperphagia in the albino rat. *Yale J. Biol. Med.*, 1943, **15**, 831-853.

Brogden, W. J. Acquisition and extinction of a conditioned avoidance response in dogs. *J. comp. physiol. Psychol.*, 1949, **42**, 296-302.

Brown, G. W., and Cohen, B. D. Avoidance and approach learning motivated by stimulation of identical hypothalamic loci. *Amer. J. Physiol.*, 1959, **197**, 153-157.

Brown, J. S. *The Motivation of Behavior.* New York: McGraw-Hill Book Company, Inc., 1961.

———, and Farber, I. E. Emotions conceptualized as intervening variables—with suggestions toward a theory of frustration. *Psychol. Bull.*, 1951, **48**, 465-480.

———, and Jacobs, A. The role of fear in the motivation and acquisition of responses. *J. exp. Psychol.*, 1949, **39**, 747-759.

———, Kalish, H. I., and Farber, I. E. Conditioned fear as revealed by magnitude of startle response to an auditory stimulus. *J. exp. Psychol.*, 1951, **41**, 317-328.

Bulatao, E., and Carlson, A. J. Contributions to the physiology of the stomach. Influence of experimental changes in blood sugar level on gastric hunger contractions. *Amer. J. Physiol.*, 1924, **69**, 107-115.

Butler, R. A. Discrimination learning by rhesus monkeys to visual-exploration motivation. *J. comp. physiol. Psychol.*, 1953, **46**, 95-98.

———. The effect of deprivation of visual incentives on visual exploration-motivation in monkeys. *J. comp. physiol. Psychol.*, 1957, **50**, 177-179.

———. Discrimination learning by rhesus monkeys to auditory incentives. *J. comp. physiol. Psychol.*, 1957(*a*), **50**, 239-241.

———, and Harlow, H. F. Persistence of visual exploration in monkeys. *J. comp. physiol. Psychol.*, 1954, **47**, 258-263.

Butter, C. M., and Thomas, D. R. Secondary reinforcement as a function of the amount of primary reinforcement. *J. comp. physiol. Psychol.*, 1958, **51**, 346-348.

Calvin, J. S., Bicknell, E. A., and Sperling, D. S. Establishment of a conditioned drive based on the hunger drive. *J. comp. physiol. Psychol.*, 1953, **46**, 173-175.

Campell, A. A., Hilgard, E. R. Individual differences in ease of conditioning. *J. exp. Psychol.*, 1936, **19**, 561-571.

Campbell, B. A., and Kraeling, D. Response strength as a function of drive level during training and extinction. *J. comp. physiol. Psychol.*, 1954, **47**, 101-103.

———, and Pickleman, J. R. The imprinting object as a reinforcing stimulus. *J. comp. physiol. Psychol.*, 1961, **54**, 592-596.

Cannon, W. B. A consideration of the nature of hunger. *Harvey Lect.*, 1911-12, **7**, 130-152.

———. The physiological basis of thirst. *Proc. Roy. Soc., London, sB.*, 1918, **90**, 283-301.

———. *The Wisdom of the Body.* New York: W. W. Norton & Company, Inc., 1932.

———, and Washburn, A. L. An explanation of hunger. *Amer. J. Physiol.*, 1912, **29**, 441-454.

Carlson, A. J *The Control of Hunger in Health and Disease.* Chicago: University of Chicago Press, 1916.

Carlson, N. J., Black, A. H., Walters, P. A., and Kirman, J. H. Traumatic avoidance learning: the effect of preventing escape responses. *Amer. Psychol.*, 1957, **12**, 454.

Carr, H. A. *Psychology, a Study of Mental Activity.* New York: Longmans, Green & Co., Inc., 1925.

Chambers, R. M. Effects of intravenous glucose injections on learning, general activity, and hunger drive. *J. comp. physiol. Psychol.*, 1956, **49**, 558-564.

Champion, R. A., and McBride, D. A. Activity during delay of reinforcement in human learning. *J. exp. Psychol.*, 1962, **63**, 589-592.

Cicala, G. S. Running speed in rats as a function of drive level and presence or absence of competing response trials. *J. exp. Psychol.*, 1961, **62**, 329-334.

Clayton, F. L. Light reinforcement as a function of water deprivation. *Psychol. Repts.*, 1958, **4**, 63-66.

Cofer, C. N. Motivation. *Annual Review Psychology*, 1959, **10**, 173-202.

Cohen, B. D., Brown, G. W., and Brown, M. L. Avoidance learning motivated by hypothalamic stimulation. *J. exp. Psychol.*, 1957, **53**, 228-233.

Collier, G., Knarr, F. A., and Marx, M. H. Some relations between the intensive properties of the consummatory response and reinforcement. *J. exp. Psychol.*, 1961, **62**, 484-495.

Coppock, H. W., and Chambers, R. M. Reinforcement of position preference by automatic intravenous injections of glucose. *J. comp. physiol. Psychol.*, 1954, **47**, 355-357.

Cotton, J. W. Running time as a function of amount of food deprivation. *J. exp. Psychol.*, 1953, **46**, 188-198.

Cowles, J. T. Food tokens as incentives for learning by chimpanzees. *Comp. Psychol. Monogr.*, 1937, **14**, No. 5.

Crespi, L. P. Quantitative variation of incentive and performance in the white rat. *Amer. J. Psychol.*, 1942, **55**, 467-517.

Crowder, W. F., Gay, B. R., Bright, M. G., and Lee, M. F. Secondary reinforcement or response facilitation?: III. Reconditioning. *J. Psychol.*, 1959, **48**, 307-310.

———, ———, Fleming, W. C., and Hurst, R. W. Secondary reinforcement or response facilitation?: IV. The retention method. *J. Psychol.*, 1959, **48**, 311-314.

———, Gill, K., Jr., Hodge, C. C., and Nash, F. A. Secondary reinforcement or response acquisition?: II. Response acquisition. *J. Psychol.*, 1959, **48**, 303-306.

———, Morris, J. B., and McDaniel, M. H. Secondary reinforcement or response facilitation?: I. Resistance to extinction. *J. Psychol.*, 1959, **48**, 299-302.

D'Amato, M. R. Secondary reinforcement and magnitude of primary reinforcement. *J. comp. physiol. Psychol.*, 1955, **48**, 378-380.

———, Lachman, R., and Kivy, P. Secondary reinforcement as affected by reward schedule and the testing situation. *J. comp. physiol. Psychol.*, 1958, **51**, 737-741.

Dashiell, J. F. A quantitative demonstration of animal drive. *J. comp. Psychol.*, 1925, **5**, 205-208.

———. *Fundamentals of Objective Psychology.* Boston: Houghton Mifflin Company, 1928.

———. *Fundamentals of Objective Psychology.* Boston: Houghton Mifflin Company, 2d ed., 1937.

Davis, J. D. The reinforcing effect of weak-light onset as a function of amount of food deprivation. *J. comp. physiol. Psychol.*, 1958, **51**, 496-498.

Davis, R. C., Garafolo, L., and Gault, F. P. An exploration of abdominal potentials. *J. comp. physiol. Psychol.*, 1957, **50**, 519-523.

———, ———, and Kveim, K. Conditions associated with gastrointestinal activity. *J. comp. physiol. Psychol.*, 1959, **52**, 466-475.

Davis, R. H. The effect of drive reversal on latency, amplitude and activity level. *J. exp. Psychol.*, 1957, **53**, 310-315.

Deese, J., and Carpenter, J. A. Drive-level and reinforcement. *J. exp. Psychol.*, 1951, **42**, 236-238.

Delgado, J. M. R., Roberts, W. W., and Miller, N. E. Learning motivated by electrical stimulation of the brain. *Amer. J. Physiol.*, 1954, **179**, 587-593.

Denny, M. R., and Behan, R. A. Conditioned hunger drive or conditioned approach? *Psychol. Repts.*, 1956, **2**, 192-193.

Dinsmoor, J. A. A quantitative comparison of the discriminative and reinforcing functions of a stimulus. *J. exp. Psychol.*, 1950, **40**, 458-472.

Dollard, J., Doob, L. W., Miller, N. E., Mowrer, O. H., and Sears, R. R. *Frustration and Aggression.* New Haven, Conn.: Yale University Press, 1939.

Dufort, R. H., and Wright, J. H. Food intake as a function of duration of food deprivation. *J. Psychol.*, 1962, **53**, 465-468.

Ehrenfreund, D., and Badia, P. Response strength as a function of drive level and pre- and postshift incentive magnitude. *J. exp. Psychol.*, 1962, **63**, 468-471.

Estes, B. W., Miller, L. B., and Curtin, M. E. Supplementary report: Monetary incentive and motivation in discrimination learning—sex differences. *J. exp. Psychol.*, 1962, **63**, 320.

Estes, W. K. Toward a statistical theory of learning. *Psychol. Rev.*, 1950, **57**, 94-107.

———. Stimulus-response theory of drive. In Jones, M. R. (ed.), Nebraska Symposium on Motivation. Lincoln, Nebraska: University of Nebraska Press, 1958.

Feather, N. T. The study of persistence. *Psychol. Bull.*, 1962, **59**, 94-115.

Fehrer, E. The effects of hunger and familiarity of locale on exploration. *J. comp. physiol. Psychol.*, 1956, **49**, 549-552.

Felsinger, J. M., Gladstone, A. I., Yamaguchi, H. G., and Hull, C. L. Reaction latency (s^tr) as a function of the number of reinforcements (N) *J. exp. Psychol.*, 1947, **37**, 214-228.

Finger, F. W. The effect of food deprivation and subsequent satiation upon general activity in the rat. *J. comp. physiol. Psychol.*, 1951, **44**, 557-564.

———, and Reid, L. S. The effect of water deprivation and subsequent satiation upon general activity in the rat. *J. comp. physiol. Psychol.*, 1952, **45**, 368-372.

———, ———, and Weasner, M. H. The effect of reinforcement upon activity during cyclic food deprivation. *J. comp. physiol. Psychol.*, 1957, **50**, 495-498.

Forgays, D. G., and Levin, H. Learning as a function of change of sensory stimulation in food-deprived and food-satiated animals. *J. comp. physiol. Psychol.*, 1958, **51**, 50-54.

Fowler, H., and Trapold, M. A. Escape performance as a function of delay of reinforcement. *J. exp. Psychol.*, 1962, **63**, 464-467.

Fox, R. E., and King, R. A. The effects of reinforcement scheduling on the strength of a secondary reinforcer. *J. comp. physiol. Psychol.*, 1961, **54**, 266-269.

Ghent, L. The relation of experience to the development of hunger. *Canad. J. Psychol.*, 1951, **5**, 77-81.

———. Some effects of deprivation on eating and drinking behavior. *J. comp. physiol. Psychol.*, 1957, **50**, 172-175.

Gilman, A. The relation between blood osmotic pressure, fluid distribution and voluntary water intake. *Amer. J. Physiol.*, 1937, **120**, 323-328.

Glickman, S. E., and Jensen, G. D. The effects of hunger and thirst on Y-maze exploration. *J. comp. physiol. Psychol.*, 1961, **54**, 83-85.

Goodrich, K. P. Running speed and drinking rate as functions of sucrose concentration and amount of consummatory activity. *J. comp. physiol. Psychol.*, 1960, **53**, 245-250.

Granich, L. An analysis of motivation. *Psychol. Rev.*, 1932, **39**, 235-244.

Greenberg, I. The acquisition of a thirst drive. Unpublished Ph.D. dissertation, University of Pennsylvania, 1954.

Grice, G. R. The relation of secondary reinforcement to delayed reward in visual discrimination learning. *J. exp. Psychol.*, 1948, **38**, 1-16.

Grossberg, J. M. Pseudoconditioning, drive reduction, and the acquired fear-drive hypothesis. *Psychol. Rec.*, 1962, **12**, 299-307.

HALL, J. F. Studies in secondary reinforcement: I. Secondary reinforcement as a function of the frequency of primary reinforcement. *J. comp. physiol. Psychol.*, 1951, **44**, 246-251.

———. The relation between external stimulation, food deprivation, and activity. *J. comp. physiol. Psychol.*, 1956, **49**, 339-341.

———. The influence of learning in activity wheel behavior. *J. genet. Psychol.*, 1958, **92**, 121-125.

———. *Psychology of Motivation*. Philadelphia, Pa.: J. B. Lippincott Company, 1961.

———, AND KOBRICK, J. L. The relationships among three measures of response strength. *J. comp. physiol. Psychol.*, 1952, **45**, 280-282.

———, LOW, L., AND HANFORD, P. The activity of hungry, thirsty, and satiated rats in the Dashiell checkerboard maze. *J. comp. physiol. Psychol.*, 1960, **53**, 155-158.

HAMMES, J. A. Visual discrimination learning as a function of shock-fear and task difficulty. *J. comp. physiol. Psychol.*, 1956, **49**, 481-484.

HARLOW, H. F. The nature of love. *Amer. Psychol.*, 1958, **13**, 673-685.

———, HARLOW, M. K., AND MEYER, D. R. Learning motivated by a manipulation drive. *J. exp. Psychol.*, 1950, **40**, 228-234.

———, AND ZIMMERMANN, R. R. Affectional responses in the infant monkey. *Science*, 1959, **130**, 421-432.

HEBB, D. O. *The Organization of Behavior*. New York: John Wiley & Sons, Inc., 1949.

———. Drives and the C. N. S. (conceptual nervous system). *Psychol. Rev.*, 1955, **62**, 243-254.

———. Introduction to cognitive and physiological effects of perceptual isolation. In Solomon, P., Kubzansky, P. E., Leiderman, P. H., Mendelson, J. H., Trumbull, R., and Wexler, D. (eds.), *Sensory Deprivation*. Cambridge: Harvard University Press, 1961.

HELSON, H. Adaptation-level as frame of reference for prediction of psychophysical data. *Amer. J. Psychol.*, 1947, **60**, 1-29.

———. Adaptation-level theory. In Koch, S. (ed.), *Psychology: A Study of a Science*. Vol. I. New York: McGraw-Hill Book Company, Inc., 1959.

HENDRY, D. P., AND RASCHE, R. H. Analysis of a new nonnutritive positive reinforcer based on thirst. *J. comp. physiol. Psychol.*, 1961, **54**, 477-483.

HETHERINGTON, A. W., AND RANSON, S. W. Hypothalamic lesions and adiposity in the rat. *Anat. Record*, 1940, **78**, 149-172.

HILL, W. F. Activity as an autonomous drive. *J. comp. physiol. Psychol.*, 1956, **49**, 15-19.

———. The effect of varying periods of confinement on activity in tilt cages. *J. comp. physiol. Psychol.*, 1958, **51**, 570-574.

———. The effect of long confinement on voluntary wheel-running by rats. *J. comp. physiol. Psychol.*, 1958*a*, **51**, 770-773.

HILLMAN, B., HUNTER, W. S., AND KIMBLE, G. A. The effect of drive level on the maze performance of the white rat. *J. comp. physiol. Psychol.*, 1953, **46**, 87-89.

HITCHCOCK, F. A. The total energy requirement of the albino rat for growth and activity. *Amer. J. Physiol.*, 1927, **83**, 28-36.

HOLMES, J. H., AND GREGERSEN, M. I. Relation of the salivary flow to the thirst produced in man by intravenous injection of hypertonic salt solution. *Amer. J. Physiol.*, 1947, **151**, 252-257.

———, AND ———. Observations in drinking induced by hypertonic solution. *Amer. J. Physiol.*, 1950, **162**, 326-337.

———, AND ———. Role of sodium and chloride in thirst. *Amer. J. Physiol.*, 1950*a*, **162**, 338-347.

HOPKINS, C. O. Effectiveness of secondary reinforcing stimuli as a function of the quantity and quality of food reinforcement. *J. exp. Psychol.*, 1955, **50**, 339-342.

HORENSTEIN, B. R. Performance of conditioned responses as a function of strength of hunger drive. *J. comp. physiol. Psychol.*, 1951, **44**, 210-224.

HOWARD, T. C., AND YOUNG, F. A. Conditioned hunger and secondary rewards in monkeys. *J. comp. physiol. Psychol.*, 1962, **55**, 392-397.

HULL, C. L. *Principles of Behavior*. New York: Appleton-Century-Crofts, Inc., 1943.

———. *Essentials of Behavior*. New Haven: Yale University Press, 1951.

———, LIVINGSTON, J. R., ROUSE, R. O., AND BARKER, A. N. True, sham, and esophageal feeding as reinforcements. *J. comp. physiol. Psychol.*, 1951, **44**, 236-245.

HUNT, J. MCV., AND QUAY, H. C. Early vibratory experience and the question of innate reinforcement value of vibration and other stimuli. *Psychol. Rev.*, 1961, **68**, 149-156.

HURWITZ, H. M. B., AND DE, S. C. Studies in light reinforced behavior: II. Effect of food deprivation and stress. *Psychol. Repts.*, 1958, **4**, 71-77.

JANOWITZ, H. D., AND GROSSMAN, M. I. Effect of parenteral administration of glucose and protein hydrolysate on food intake in the rat. *Amer. J. Physiol.*, 1948, **155**, 28-32.

———, AND ———. Some factors affecting the food intake of normal dogs and dogs with esophagostomy and gastric fistula. *Amer. J. Physiol.*, 1949, **159**, 143-148.

Jenkins, W. O. A temporal gradient of derived reinforcement. *Amer. J. Psychol.*, 1950, **63**, 237-243.

Jones, A., Wilkinson, H. J., and Braden, I. Information deprivation as a motivational variable. *J. exp. Psychol.*, 1961, **62**, 126-137.

Kagan, J., and Berkun, M. The reward value of running activity. *J. comp. physiol. Psychol.*, 1954, **47**, 108.

Kellogg, W. N., and Walker, E. L. An analysis of the bilateral transfer of conditioning in dogs, in terms of the frequency, amplitude, and latency of the responses. *J. gen. Psychol.*, 1938, **18**, 253-265.

Kessen, W. Response strength and conditioned stimulus intensity. *J. exp. Psychol.*, 1953, **45**, 82-86.

Kimble, G. A. *Hilgard and Marquis' Conditioning and Learning.* New York: Appleton-Century-Crofts, Inc., 1961.

Kish, G. B. Learning when the onset of illumination is used as reinforcing stimulus. *J. comp. physiol. Psychol.*, 1955, **48**, 261-264.

Klein, R. M. Intermittent primary reinforcement as a parameter of secondary reinforcement. *J. exp. Psychol.*, 1959, **58**, 423-427.

Kling, J. W., Horowitz, L., and Delhagen, J. E. Light as a positive reinforcer for rat responding. *Psychol. Rep.*, 1956, **2**, 337-340.

Kobrick, J. L. The relationships among three measures of response strength as a function of the numbers of reinforcements. *J. comp. physiol. Psychol.*, 1956, **49**, 582-585.

Koch, S., and Daniel, W. J. The effect of satiation on the behavior mediated by a habit of maximum strength. *J. exp. Psychol.*, 1945, **35**, 167-187.

Kohn, M. Satiation of hunger from food injected directly into the stomach versus food ingested by mouth. *J. comp. physiol. Psychol.*, 1951, **44**, 412-422.

Lawrence, D. H., and Mason, W. A. Food intake in the rat as a function of deprivation intervals and feeding rhythms. *J. comp. physiol. Psychol.*, 1955, **48**, 267-271.

Lawson, R. Amount of primary reward and strength of secondary reward. *J. exp. Psychol.*, 1953, **46**, 183-187.

———. Brightness discrimination performance and secondary reward strength as a function of primary reward amount. *J. comp. physiol. Psychol.*, 1957, **50**, 35-39.

Lazarus, R. S., and Baker, R. W. Motivation and personality in psychological stress. *Psychol. Newsltr.*, 1957, **8**, 159-193.

Lewis, D. J., and Cotton, J. W. Learning and performance as a function of drive strength during acquisition and extinction. *J. comp. physiol. Psychol.*, 1957, **50**, 189-194.

Logan, F. A. *Incentive.* New Haven: Yale Univ. Press, 1960.

Madsen, K. B. *Theories of Motivation.* Cleveland: Howard Allen, 1961.

Marx, M. H. Some relations between frustration and drive. In Jones, M. R. (ed.), Nebraska Symposium on Motivation. Lincoln, Nebraska: University of Nebraska Press, 1956.

———, Henderson, R. L., and Roberts, C. L. Positive reinforcement of the bar-pressing response by a light stimulus following dark operant pretests with no aftereffect. *J. comp. physiol. Psychol.*, 1955, **48**, 73-76.

Mason, D. J. The relation of secondary reinforcement to partial reinforcement. *J. comp. physiol. Psychol.*, 1957, **50**, 264-268.

Mayer, J. Regulation of energy intake and the body weight: the glucostatic theory and the lipostatic hypothesis. In Miner, R. W. (ed.), *The Regulation of Hunger and Appetite.* New York: Annals of the New York Academy of Sciences, 1955.

———, and Bates, M. W. Blood glucose and food intake in normal and hypophysectomized, alloxan treated rats. *Amer. J. Physiol.*, 1952, **168**, 812-819.

McClelland, D. C. Functional autonomy of motives as an extinction phenomenon. *Psychol. Rev.*, 1942, **49**, 272-283.

———, Atkinson, J. W., Clark, R. A., and Lowell, E. L. *The Achievement Motive.* New York: Appleton-Century-Crofts, Inc., 1953.

———, Clark, R. A., Roby, T. B., and Atkinson, J. W. The projective expression of needs. IV. The effect of need for achievement on thematic apperception. *J. exp. Psychol.*, 1949, **39**, 242-255.

McGeoch, J. A., and Irion, A. L. *The Psychology of Human Learning.* New York: David McKay Company, 1952.

McGuigan, F. J., and Crockett, F. Evidence that the secondary reinforcing stimulus must be discriminated. *J. exp. Psychol.*, 1958, **55**, 184-187.

Melton, A. W. Learning. In Monroe, W. S. (ed.), *Encyclopedia of Educational Research.* New York: The Macmillan Co., 1950.

Menzel, E .W., Jr., Davenport, R. K., Jr., and Rogers, C. M. Some aspects of behavior toward novelty in young chimpanzees. *J. comp. physiol. Psychol.*, 1961, **54**, 16-19.

Metzger, R., Cotton, J. W., and Lewis, D. J. Effect of reinforcement magnitude and of order of presentation of different magnitudes on runway behavior. *J. comp. physiol. Psychol.*, 1957, **50**, 184-188.

Meyer, D. R. Food deprivation and discrimination reversal learning by monkeys. *J. exp. Psychol.*, 1951, **41**, 10-16.

———. The effects of differential rewards on discrimination reversal learning by monkeys. *J. exp. Psychol.*, 1951*a*, **41**, 268-274.

———. On the interaction of simultaneous responses. *Psychol. Bull.*, 1953, **50**, 204-220.

Miles, R. C. The relative effectiveness of secondary reinforcers throughout deprivation and habit-strength parameters. *J. comp. physiol. Psychol.*, 1956, **49**, 126-130.

———. Discrimination in the squirrel monkey as a function of deprivation and problem difficulty. *J. exp. Psychol.*, 1959, **57**, 15-19.

———, and Wickens, D. D. Effect of a secondary reinforcer on the primary hunger drive. *J. comp. physiol. Psychol.*, 1953, **46**, 77-79.

Miller, L. B., and Estes, B. W. Monetary reward and motivation in discrimination learning. *J. exp. Psychol.*, 1961, **61**, 501-504.

Miller, N. E. Studies of fear as an acquirable drive: I. Fear as motivation and fear-reduction as reinforcement in learning of new responses. *J. exp. Psychol.*, 1948, **38**, 89-101.

———. Learnable drives and rewards. In Stevens, S. S. (ed.), *Handbook of Experimental Psychology*. New York: John Wiley & Sons, Inc., 1951.

———. Learning and performance motivated by direct stimulation of the brain. In Sheer, D. E. (ed.), *Electrical Stimulation of the Brain.* Houston: University of Texas Press, 1961.

———. Analytical studies of drive and reward. *Amer. Psychol.*, 1961*a*, **16**, 739-754.

———, and Kessen, M. L. Reward effects of food via stomach fistula compared with those of food via mouth. *J. comp. physiol. Psychol.*, 1952, **45**, 555-564.

Miner, R. W. (ed.). *The Regulation of Hunger and Appetite*. New York: Annals of the New York Academy of Sciences, 1955.

Montgomery, K. C. Spontaneous alternation as a function of time between trials and amount of work. *J. exp. Psychol.*, 1951, **42**, 82-93.

———. The effect of hunger and thirst drives upon exploratory behavior. *J. comp. physiol. Psychol.*, 1953, **46**, 315-319.

———. The effect of activity deprivation upon exploratory behavior. *J. comp. physiol. Psychol.*, 1953*a*, **46**, 438-441.

———. The role of exploratory drive in learning. *J. comp. physiol. Psychol.*, 1954, **47**, 60-64.

———, and Segall, M. Discrimination learning based upon exploratory drive. *J. comp. physiol. Psychol.*, 1955, **48**, 225-228.

Montgomery, M. F. The role of the salivary glands in the thirst mechanism. *Amer. J. Physiol.*, 1931, **96**, 221-227.

Moon, L. E., and Lodahl, T. M. The reinforcing effect of changes in illumination on lever-pressing in the monkey. *Amer. J. Psychol.*, 1956, **69**, 288-290.

Morgan, C. T., and Morgan, J. D. Studies in hunger: II. The relation of gastric denervation and dietary sugar to the effect of insulin upon food intake in the rat. *J. genet. Psychol.*, 1940, **57**, 153-163.

Morrison, S. D., Ju Lin, H., Eckel, H. E., Van Itallie, T. B., and Mayer, J. Gastric contractions in the rat. *Amer. J. Physiol.*, 1958, **193**, 4-8.

———, and Mayer, J. Adipsis and aphagia in rats after lateral subthalamic lesions. *Amer. J. Physiol.*, 1957, **191**, 248-254.

Moskowitz, M. J. Running-wheel activity in the white rat as a function of combined food and water deprivation. *J. comp. physiol. Psychol.*, 1959, **52**, 621-625.

Moss, F. A. Study of animal drives. *J. exp. Psychol.*, 1924, **7**, 165-185.

Myers, A. K., and Miller, N. E. Failure to find learned drive based on hunger; evidence for learning motivated by "exploration." *J. comp. physiol. Psychol.*, 1954, **47**, 428-436.

Nissen, H. W. A study of exploratory behavior in the white rat by means of the obstruction method. *J. genet. Psychol.*, 1930, **37**, 361-376.

Novin, D., and Miller, N. E. Failure to condition thirst induced by feeding dry food to hungry rats. *J. comp. physiol. Psychol.*, 1962, **55**, 373-374.

Olds, J., and Milner, P. Positive reinforcement produced by electrical stimulation of septal area and other regions of rat brain. *J. comp. physiol. Psychol.*, 1954, **47**, 419-427.

Pack, G. T. New experiments on the nature of the sensation of thirst. *Amer. J. Physiol.*, 1923, **65**, 346-349.

Peak, H. Attitude and Motivation. In Jones, M. R. (ed.), Nebraska Symposium on Motivation. Lincoln, Nebraska: University of Nebraska Press, 1955.

Penney, R. K. The effects of non-reinforcement on response strength as a function of number of previous reinforcement. *Canad. J. Psychol.*, 1960, **14**, 206-215.

Perin, C. T. Behavior potentiality as a joint function of the amount of training and the degree of hunger at the time of extinction. *J. exp. Psychol.*, 1942, **30**, 93-113.

———. A quantitative investigation of the delay of reinforcement gradient. *J. exp. Psychol.*, 1943, **32**, 37-51.

Perkins, C. C., Jr. The relation of secondary reward to gradients of reinforcement. *J. exp. Psychol.*, 1947, **37**, 377-392.

Prokasy, W. F., Jr., Grant, D. A., and Myers, N. A. Eyelid conditioning as a function of unconditioned stimulus intensity and inter-trial interval. *J. exp. Psychol.*, 1958, **55**, 242-246.

Pubols, B. H., Jr. Incentive magnitude, learning and performance in animals. *Psychol. Bull.*, 1960, **57**, 89-115.

Quigley, J. P., and Hallaran, W. R. The independence of spontaneous gastrointestinal motility and blood sugar levels. *Amer. J. Physiol.*, 1932, **100**, 102-110.

Rethlingshafer, D. Experimental evidence for functional autonomy of motives. *Psychol. Rev.*, 1943, **50**, 397-407.

Reynolds, W. F., and Pavlik, W. B. Running speed as a function of deprivation period and reward magnitude. *J. comp. physiol. Psychol.*, 1960, **53**, 615-618.

Richter, C. P. A behavioristic study of the activity of the rat. *Comp. Psychol. Monogr.*, 1922, **1**, No. 2.

Roberts, C. L., Lebow, K. E., and Yoder, R. M. Secondary reinforcement based on stimulus-change primary reinforcement. *J. exp. Psychol.*, 1961, **61**, 339-344.

———, Marx, M. H., and Collier, G. Light onset and light offset as reinforcers for the albino rat. *J. comp. physiol. Psychol.*, 1958, **51**, 575-579.

Roberts, W. W. Rapid escape learning without avoidance learning motivated by hypothalamic stimulation in cats. *J. comp. physiol. Psychol.*, 1958, **51**, 391-399.

———. Both rewarding and punishing effects from stimulation of posterior hypothalamus of cats with same electrode at same intensity. *J. comp. physiol. Psychol.*, 1958*a*, **51**, 400-407.

Robinson, J. S. The reinforcing effects of response-contingent light increment and decrement in hooded rats. *J. comp. physiol. Psychol.*, 1961, **54**, 470-473.

Rohrer, J. H. A motivational state resulting from nonreward. *J. comp. physiol. Psychol.*, 1949, **42**, 476-485.

Saltzman, I. J., and Koch, S. The effect of low intensities of hunger on the behavior mediated by a habit of maximum strength. *J. exp. Psychol.*, 1948, **38**, 347-370.

Sargent, S. S. Reaction to frustration—a critique and hypothesis. *Psychol. Rev.*, 1948, **55**, 108-114.

Schoenfeld, W. N., Antonitis, J. J., and Bersh, P. J. A preliminary study of training conditions necessary for secondary reinforcement. *J. exp. Psychol.*, 1950, **40**, 40-45.

Schrier, A. M. Comparison of two methods of investigating the effect of amount of reward on performance. *J. comp. physiol. Psychol.*, 1958, **51**, 725-731.

Schulman, J. L., Carleton, J. L., Whitney, G., and Whitehorn, J. C. Effect of glucagon on food intake and body weight in man. *J. appl. Physiol.*, 1958, **11**, 419-421.

Sheffield, F. D., and Campbell, B. A. The role of experience in the "spontaneous" activity of hungry rats. *J. comp. physiol. Psychol.*, 1954, **47**, 97-100.

———, and Roby, T. B. Reward value of a non-nutritive sweet taste. *J. comp. physiol. Psychol.*, 1950, **43**, 471-481.

———, ———, and Campbell, B. A. Drive reduction versus consummatory behavior as determinants of reinforcement. *J. comp. physiol. Psychol.*, 1954, **47**, 349-354.

———., Wulff, J. J., and Backer, R. Reward value of copulation without sex drive reduction. *J. comp. physiol. Psychol.*, 1951, **44**, 3-8.

Sheffield, V. F. Extinction as a function of partial reinforcement and distribution of practice. *J. exp. Psychol.*, 1949, **39**, 511-526.

———. Resistance to extinction as a function of the distribution of extinction trials. *J. exp. Psychol.*, 1950, **40**, 305-313.

Shirley, M. Studies in activity: II. Activity rhythms; age and activity; activity after rest. *J. comp. Psychol.*, 1928, **8**, 159-186.

Siegel, P. S. Activity level as a function of physically enforced inaction. *J. Psychol.*, 1946, **21**, 285-291.

———. Alien drive, habit strength and resistance to extinction. *J. comp. physiol. Psychol.*, 1946*a*, **39**, 307-317.

———. The relationship between voluntary water intake, body-weight loss, and number of hours of water privation in the rat. *J. comp. physiol. Psychol.*, 1947, **40**, 231-238.

———, and Alexander, I. E. A further observation on the effect of physically enforced inaction on the activity level of the rat. *J. genet. Psychol.*, 1948, **72**, 57-62.

———, and MacDonnell, M. F. A repetition of the Calvin-Bicknell-Sperling study of conditioned drive. *J. comp. physiol. Psychol.*, 1954, **47**, 250-252.

———, and Steinberg, M. Activity level as a function of hunger. *J. comp. physiol. Psychol.*, 1949, **42**, 413-416.

Simon, C. W., Wickens, D. D., Brown, U., and Pennock, L. Effect of the secondary reinforcing agents on the primary thirst drive. *J. comp. physiol. Psychol.*, 1951, **44**, 67-70.

Smith, M., and Duffy, M. Some physiological factors that regulate eating behavior. *J. comp. physiol. Psychol.*, 1957, **50**, 601-608.

Smith, M. P., and Buchanan, G. Acquisition of secondary reward by cues associated with shock reduction. *J. exp. Psychol.*, 1954, **48**, 123-126.

Smith, O. A., Jr., McFarland, W. L., and Taylor, E. Performance in a shock-avoidance conditioning situation interpreted as pseudoconditioning. *J. comp. physiol. Psychol.*, 1961, **54**, 154-157.

Snyder, H. L., and Hulse, S. H. Effect of volume of reinforcement and number of consummatory responses on licking and running behavior. *J. exp. Psychol.*, 1961, **61**, 474-479.

Solomon, P., Kubzansky, P. E., Leiderman, P. H., Mendelson, J. H., Trumbull, R., Wexler, D. *Sensory Deprivation.* Cambridge: Harvard University Press, 1961.

Spence, K. W. *Behavior Theory and Conditioning.* New Haven: Yale University Press, 1956.

Sporn, J., and Necheles, H. Effect of glucagon on gastrointestinal motility. *Amer. J. Physiol.*, 1956, **187**, 634.

Stabler, J. R. Performance in instrumental conditioning as a joint function of time of deprivation and sucrose concentration. *J. exp. Psychol.*, 1962, **63**, 248-253.

Stebbins, W. C. Relation of amount of primary reinforcement to discrimination and to secondary reinforcement strength. *J. comp. physiol. Psychol.*, 1959, **52**, 721-726.

Steggerda, F. R. The relation of dry mouth to thirst in the human. *Amer. J. Physiol.*, 1939, **126**, 635.

Stellar, E., and Hill, J. H. The rat's rate of drinking as a function of water deprivation. *J. comp. physiol. Psychol.*, 1952, **45**, 96-102.

Stevenson, J. A. F., and Rixon, R. H. Environmental temperature and deprivation of food and water on the spontaneous activity of rats. *Yale J. Biol. Med.*, 1957, **29**, 575-584.

Strassburger, R. C. Resistance to extinction of a conditioned operant as related to drive level at reinforcement. *J. exp. Psychol.*, 1950, **40**, 473-487.

Strong, P. N., Jr. Activity in the white rat as a function of apparatus and hunger. *J. comp. physiol. Psychol.*, 1957, **50**, 596-600.

Stunkard, A. J., Van Itallie, T. B., and Reis, B. B. The mechanism of satiety. Effect of glucagon on gastric hunger contractions in man. *Proc. Soc. Exper. Biol. & Med.*, 1955, **89**, 258-261.

———, and Wolff, H. G. Correlation of arteriovenous glucose differences, gastric hunger contractions and the experience of hunger in man. *Federation Proc.*, 1954, **13**, 147.

Teel, K. S. Habit strength as a function of motivation during learning. *J. comp. physiol. Psychol.*, 1952, **45**, 188-191.

Teitelbaum, P. Sensory control of hypothalamic hyperphagia. *J. comp. physiol. Psychol.*, 1955, **48**, 156-163.

———, and Epstein, A. N. The lateral hypothalamic syndrome: Recovery of feeding and drinking after lateral hypothalamic lesions. *Psychol. Rev.*, 1962, **69**, 74-90.

———, and Stellar, E. Recovery from the failure to eat produced by hypothalamic lesions. *Science*, 1954, **120**, 894-895.

Towbin, E. J. Gastric distention as a factor in the satiation of thirst in esophagostomized dogs. *Amer. J. Physiol.*, 1949, **159**, 533-541.

Treichler, F. R., and Hall, J. F. The relationship between deprivation weight loss and several measures of activity. *J. comp. physiol. Psychol.*, 1962, **55**, 346-349.

Tsang, Y. C. Hunger motivation in gastrectomized rats. *J. comp. Psychol.*, 1938, **26**, 1-17.

Vinacke, W. E. The drive-modification theory of human motivation. *J. genet. Psychol.*, 1960, **96**, 245-268.

Wald, G., and Jackson, B. Activity and nutritional deprivation. *Proc. Nat. Acad. Sci.*, Washington, 1944, **30**, 255-263.

Wang, G. H. The relation between "spontaneous" activity and oestrous cycle in the white rat. *Comp. Psychol. Monogr.*, 1923, **2**, No. 6.

Wangensteen, O. H., and Carlson, A. J. Hunger sensations in a patient after total gastrectomy. *Proc Soc. exp. Biol.*, New York, 1931, **28**, 545-547.

Warren, J. M., and Hall, J. F. Discrimination of visual patterns as a function of motivation and frequency of reinforcement. *J. genet. Psychol.*, 1956, **88**, 245-250.

Warren, R. P., and Pfaffman, C. Early experience and taste aversion. *J. comp. physiol. Psychol.*, 1959, **52**, 263-266.

Webb, W. B., and Nolan, C. Y. Cues for discrimination as secondary reinforcing agents: a confirmation. *J. comp. physiol. Psychol.*, 1953, **46**, 180-181.

Weiss, R. F. Deprivation and reward magnitude effects on speed throughout the goal gradient. *J. exp. Psychol.*, 1960, **60**, 384-390.

Williams, R. A., and Campbell, B. A. Weight loss and quinine-milk ingestion as measures of "hunger" in infant and adult rats. *J. comp. physiol. Psychol.*, 1961, **54**, 220-222.

Wolf, A. V. *Thirst: Physiology of the Urge to Drink and Problems of Water Lack.* Springfield, Ill.: Charles C Thomas, Publisher, 1958.

Wyckoff, L. B., Sidowski, J., and Chambliss, D. J. An experimental study of the relationship between secondary reinforcing and cue effects of a stimulus. *J. comp. physiol. Psychol.*, 1958, **51**, 103-109.

Yamaguchi, H. G. Drive (*D*) as a function of hours of hunger (*h*) *J. exp. Psychol.*, 1951, **42**, 108-117.

———. Gradients of drive stimulus (S_d) intensity generalization. *J. exp. Psychol.*, 1952, **43**, 298-304.

Yerkes, R. M., and Dodson, J. D. The relation of strength of stimulus to rapidity of habit-formation. *J. comp. Neurol. & Psychol.*, 1908, **18**, 459-482.

Young, P. T. The role of hedonic processes in motivation. In Jones, M. R. (ed.), Nebraska Symposium on Motivation. Lincoln, Nebraska: University of Nebraska Press, 1955.

———. The role of affective processes in learning and motivation. *Psychol. Rev.*, 1959, **66**, 104-125.

———. *Motivation and Emotion.* New York: John Wiley & Sons, Inc., 1961.

———, and Shuford, E. H., Jr. Intensity, duration, and repetition of hedonic processes as related to acquisition of motives. *J. comp. physiol. Psychol.*, 1954, **47**, 298-305.

Zeaman, D. Response latency as a function of the amount of reinforcement. *J. exp. Psychol.*, 1949, **39**, 466-483.

Zimbardo, P. G., and Montgomery, K. C. The relative strengths of consummatory responses in hunger, thirst, and exploratory drive. *J. comp. physiol. Psychol.*, 1957, **50**, 504-508.

Zimmerman, D. W. Durable secondary reinforcement. Method and theory. *Psychol. Rev.*, 1957, **64**, 373-383.

———. Sustained performance in rats based on secondary reinforcement. *J. comp. physiol. Psychol.*, 1959, **52**, 353-358.

CHAPTER 7

Sensory Processes: Systematic Developments and Related Data

JOHN F. CORSO
State University of New York
(Cortland)

INTRODUCTION

HISTORICAL PERSPECTIVE The study of sensory processes as an integral part of the subject matter of contemporary psychology can be traced to three historical antecedents. The oldest of these has its roots in the epistemological problems of philosophy which focused attention on the mind as the seat of all knowledge and stressed the significance of the various senses as mediative structures interposed between the external environment and the observer. Classical empiricism, exemplified by the doctrines of Aristotle, Locke, Hume, and Condillac, took the position that sensory experience formed the basis of knowledge and that man possessed no innate ideas. The task of psychology, according to this view, was to analyze the "contents of consciousness" through introspection, with the ultimate objective of describing the fundamental elements from which all human experience could be reconstructed, given the underlying laws of mental association. Since introspection invariably revealed sensory elements, at least for the followers of content psychology, much of the early work in psychology both before and after Wundt was directed towards the study of sensations. In the transition which occurred following the establishment of psychology as a formal scientific discipline in 1879, most of this work was given an experimental orientation in contrast with its earlier philosophical setting.

The second line of influence centers around the physiological developments primarily of the nineteenth and twentieth centuries. With the discovery of Bell and Magendie of the dichotomous structure of the nervous system in terms of sensory and motor fibers, physiologists became interested in problems of sensation. Müller's doctrine of specific nerve energies provided further impetus to this movement, and physiological psychology gradually emerged as a unique area of study through the monumental contributions of Lotze, Helmholtz, and Wundt. Considerable research was subsequently performed by physiologists as well as by physiological psychologists, to determine the characteristics of nerve conduction, the relation of specific nerve energies to sensory quality, the projection of receptor fields upon various cortical areas, and the extent of localization of brain functions. Among others, the names of du Bois-Reymond, Bernstein, Lucas, Adrian, Lashley, and Wever highlight this era. In general, the physiological developments conclusively demonstrated that sensory experience is bound to and dependent upon afferent neural activity.

The third historical antecedent which led directly to the study of sensory processes was the development of psychophysical measurement. The existence of intensive and qualitative differences in sensation had for centuries been widely recognized, but it remained for Fechner (1860) to develop psychophysics from the fragmentary notions of threshold proposed earlier by Leibnitz and Herbart. Starting with the threshold concept and utilizing the observation by Weber that two sensations are just noticeably different, provided a given constant ratio obtains between the intensities of the corresponding stimuli, Fechner succeeded in working out several psychophysical methods by means of which the absolute threshold, the differential threshold, and sensory equivalence could be measured. The Fechnerian tradition was continued and expanded by such psychologists as Plateau, Delboeuf, Merkel, G. E. Müller, Jastrow, Titchener, Urban, G. H. Thomson, Thurstone, and Guilford. It was during this period that additional methods were devised to measure supraliminal sense distances and sense ratios. The approach of psychophysics, while scorned by some like William James (1890), has served to emphasize that lawful relations can be demonstrated between the properties of a physical-stimulus situation and certain specifiable types of behavioral reactions. Although psychophysics was originally concerned only with sensory problems, it has been given such a broad interpretation that in recent years its quantitative methods have been applied to a wide range of problems in all areas of psychology, from attitude measurement to the design of radar consoles.

CONTINUA AND FUNCTIONAL RELATIONS Psychology as a behavioral science is grounded on the assumption that lawful relations obtain between the members of certain classes of variables, one class of which contains variables related to behavior. In the study of sensory processes, it is somewhat more apparent than in the study of other psychological processes that the variables to be considered in the attempt to establish lawful relations may be drawn from among the various fields of knowledge, i.e., from within psychology, biology, physics, and the social sciences. This has led to an increased awareness that while the "empty organism" approach to behavior is logically defensible and systematically valid, caution must be exercised in psychological theorizing to prevent the generation of hypotheses about behavorial mechanisms as processes which are, or may be, incompatible with established findings in other disciplines. Psychology must keep abreast of advances on all fronts and must realize that theories of behavior fostered on isolated islands of knowledge are not likely to reach maturity. A systematic consideration of the variables which are significant for behavior immediately reveals that there are interpenetrations with other disciplines which must be considered; this is most apparent in the so-called interdisciplinary sciences, such as psychoacoustics and psychopharmacology. Whether or not the variables selected for study are taken from the conventionally designated areas of knowledge or from the fairly narrow realm of the interdisciplinary sciences, the goal of contemporary psychology, particularly within the area of sensory processes, is to establish a collection of functional relationships among classes of variables, with at least one class in the relation containing referents to observable behaviors.

Within the framework of an experiment, the classes of empirical variables under study are usually designated as independent and dependent variables. The independent variables are those aspects of the experimental situation which are controlled by the experimenter, physically, statistically, or by appropriate selection procedures, to determine whether their presence has any effect on the behavior to be observed and, if it has, to establish the extent of the effect. The dependent variables are those changes in behavior, if any, which are produced by the independent variable or variables under consideration; they are the resulting changes which the experimenter is attempting to predict or to explain. The behavorial reactions are said to be dependent variables since their observed magnitudes are contingent upon the values of the independent variable(s) or, stated in slightly different terms, are influenced by the occurrence of a particular set of antecedent conditions. Thus, in the latter case, the term "treatment" is often used in experimental designs to indicate that the criterion measure, i.e., the dependent variable, is being measured after the subjects in the experiment have been exposed to a complex set of independent variables. Nevertheless, the intent is to obtain observations under a set of controlled conditions which, when appropriately verified, will establish the existence of certain relations between or among the variables investigated. The formal statement of such a relationship, preferably in the form of a mathematical function, comprises a scientific principle or an empirical law. It is this law which provides the basis for the prediction of subsequent instances of this relationship.

Among others, Spence (1948) has elaborated on the classification of variables with which psychologists deal. Two general classes are defined: (1) empirical variables and (2) hypothetical state variables, with each class giving rise to a corresponding type of law. The empirical variables are divided further into three subclasses which relate, respectively, to the measurable properties of the stimulus, organism, and response; the hypothetical state variables are a class of variables which consist of "guesses as to how the uncontrolled or unknown factors in the system under study are related to the experimentally-known variables" (p. 72). MacCorquodale and Meehl (1948) have presented an explication of the differences between hypothetical constructs and intervening variables, but the point to be clearly understood here is that psychology as a science deals with two languages: data language and construct language. Of course, within a given theoretical framework, the terms in the vocabularies of the two languages are (or should be) explicitly and precisely related by rules of correspondence. Carnap (1928), for example, has stressed the need for the explication of terms. Whether or not hypothetical constructs or intervening variables are useful devices which aid in the formulation of laws of behavior, these laws are empirically derived and, as such, posit the nature of the quantitative relations which obtain between and among the properties of observable objects and events.

In the study of sensory processes, functional relations may be established within the area of psychophysics and neurophysiology. Narrowly defined, psychophysics deals with the relationship of discriminatory responses to properties of stimuli, while neurophysiology is concerned with effects of environmental events (stimuli) upon the electrical activity of the nervous system. Thus, neuroelectric responses are considered to intervene, both spatially and temporally, between stimuli and discriminatory responses, and the findings within the two disciplines are expected to be empirically compatible. It should also be evident that the two approaches, psychophysics and neurophysiology, describe behavior at different levels of analysis with psychophysics favoring the use of molar rather than molecular concepts. This is not to imply that one size of descriptive unit is necessarily more valid than another; the units employed are a matter of convenience, with each discipline incorporating those units of measurement which are believed to lead most readily to the formulation of lawful relations. Regardless of the size of the unit which is used to describe the various segments of a behavioral situation, the empirical variables of concern in the study of sensory processes are lodged primarily within physics, neurophysiology, and psychology.

In each case, the concept of an empirical variable implies that the objects and events of ordinary experience are characterized by properties; the properties are the observable aspects of the empirical world and can be differentiated qualitatively, if not quantitatively. Although in the early stages of inquiry some properties can be treated only in a qualitative or classificatory manner, the discovery and formulation of functional relations requires that qualitative concepts be refined to quantitative concepts, i.e., that some form of measurement be applied to the property under consideration. Variables, then, are those properties of an object or event which are abstracted for description and measurement in a particular setting. Sometimes these measurable properties are called "attributes," or "dimensions." Since different objects and events may possess differing amounts of a given property, the term "continuum" is used to designate the class of all possible magnitudes of that property. Thus, considering a specific measurable property, magnitude corresponds to a point on the continuum of that property, while attribute or dimension corresponds to the ordered succession of such points. In certain situations, variables are further designated as "continuous" or "discrete." Whenever the magnitude of the variable is specifiable only in terms of rational integers, as in determining the number of cases of a particular kind by counting, the variable is said to be discrete; i.e., the amount of the property or characteristic is represented by the frequency of occurrence of some complete entity and requires only classification and counting. Whenever the numerals designating the magnitude of the variable are assigned by some measurement procedure other than counting, the variable is said to be continuous. The accuracy of the numerals which are assigned to describe the magnitudes of the observed characteristic will depend upon the precision of the measuring instrument. Since a continuous variable is generally more amenable to mathematical treatment than a discrete variable, the latter are sometimes handled as though they were continuous. Furthermore, it is commonplace in psychology to postulate the existence of continuous variables, dimensions, or abilities which are assumed to underlie objective performance.

Contemporary psychophysical theory assumes the existence of three parallel classes of quantitative variables: physical, sensory, and judgmental (Guilford, 1954). Notice that, in terms of certain earlier comments, the physical and judgmental variables are empirical variables, whereas the sensory variables refer to a hypothetical continuum of events. Psychophysical laws are, thus, empirical laws, which state the functional relations obtaining between the properties of physical events and the characteristics of observable judgments. However, hypothetical laws of the type suggested by Spence (1948) may also be formulated, in which sensory constructs are related to the underlying physical continuum. If the intermediate sensory continuum is replaced by a physiological continuum, the interrelations discussed earlier in terms of neurophysiology are readily apparent, with observation and measurement occurring directly in the three planes. This does not mean that sensory events are assumed to be identical with or equivalent to the events of neuroanatomy or neurochemistry. The neural processes are, however, expected to have certain measurable properties which critically determine those of the judgmental or behavior continuum. The study of sensory processes is, thus, a study of the relations which obtain or may be expected to obtain between or among these several classes of variables, regardless of whether sensory or neurophysiological concepts are interposed between the antecedent environmental conditions and the consequent conditions of judgment.

SENSORY STIMULATION AND BEHAVIOR The sophisticated reader will readily note that the discussion in the preceding section concerning the relations among *three* classes of variables, while grossly correct, is an oversimplified statement of the problem. The chain of event-classes which intervene between a stimulus and a particular motor response can be expanded into a further oversimplification: stimulus → afferent neural events → central neurosensory events → central decision events, if the response falls into the so-called voluntary category → central neuromotor events → efferent neural events → observable motor response. Aside from the lack of congruency between this formulation and the actual structure of the human nervous system, two implications of the formulation should be discernible: (1) that lawful relations may be sought between and among the variables of the several classes of events, and (2) that the continued refinement of the chain of events, with the addition of other appropriate elements and pathways, would provide an explanation of the behavior under consideration.

The refinement of this chain leads directly into a consideration of the meaning of stimulus and of response or, in more general terms, sensory stimulation and behavior. Gibson (1960) has recently presented an extended discussion of the various meanings of stimulus but, in the study of sensory processes, the most appropriate concept still appears to be centered around an analysis of the antecedent event in physical terms. This may involve such variables as the level of energy which impinges on the organism, frequency or wave length, and the duration of stimulation; however, this classical view involving single dimensions has been supplemented by newer ways of conceptualizing the nature of the stimulus. These include the specification of the "stimulus" in terms of multidimensional analysis, information value, temporal patterns obtaining among the components of the "stimulus" or with respect to its repeated application in a given setting, gradients of energy flux which engulf the organism, and isomorphically invariant relationships which may be preserved in the transmission and transformation of energy passing from objects and events to terminal loci in the central nervous system. It is clear that psychology is moving towards a broader conceptualization of stimulus events, although the specification of the relevant characteristics of events, which are capable of providing sensory stimulation, are still prescribed in physical terms. Furthermore, it may well be that many properties of objects and events which are critical for a given behavior have not as yet been isolated; considerable attention remains to be directed towards the significance of *relationships* among properties, or elements, which are no less real than the properties themselves.

The chain of events which begins with a stimulus situation is, for experimental convenience, generally considered to terminate with a response; this chain, however, may be repeated as often as necessary in a particular experiment. Since the behavior of the experimental animal or subject is highly complex and essentially continuous, it cannot be observed and measured in its totality. For convenience, certain segments of this behavior are isolated for study and provide the basis for obtaining empirical data, qualitative or quantitative. Behavior is accordingly reduced to simpler units termed "responses," with the specific definition of the response posited by the experimenter. Consequently, in a general sense, the term "response" is applicable to any point in the *S-R* chain of events at which the experimenter desires to make a measurement, once the organismic-environmental boundary has been crossed at the beginning of the chain. Some of the confusion in psychology concerning the meaning of "behavior" might be resolved, if a more expanded technical vocabulary were adopted to indicate precisely the loci of these measurements and their characterizations.

SENSATION AND PERCEPTION AS CONCEPTUAL DISTINCTIONS The history of psychology is replete with arguments concerning the meaning of the two terms "sensation" and "perception." For some time, it was generally held that sensation, or the attributes of sensation, were the simplest elements which, according to introspective evidence, formed the basis of "conscious" experience or "awareness." Perception, then, was that aspect of experience resulting from the combination of these simpler elements. This view, however, is no longer prevalent, primarily since the introspective analysis of perception has failed in some degree to reveal the assumed substrate of sensational "building stones."

Hebb (1958) has proposed that the two terms "sensation" and "perception" be retained to distinguish between two theoretically known processes. Sensation is regarded as an afferent process, which is bound to sense-organ activity, with its resulting neural representation in the cortical sensory area, while perception is defined as the mediating central process initiated by sensation but not completely determined by it. Thus, sensation is a process which is directly and completely controlled by environmental factors; perception is a process only partially under environmental control, thereby permitting the introduction of other relevant variables such as those commonly designated as motivational, clinical, and personal.

Attneave (1962), although avoiding a definition of perception, asserts that "perception has to do with the input side of the organism, with certain short-term consequences of variations in stimulating conditions" (p. 620). Both Hebb and Attneave and others are in agreement that perception at least involves sensory stimulation. It appears, therefore, that sensation and perception are applied in an attempt to restrict or to define the number of phases of activity which occur in the chain of events between the stimulus and the response. The terms do not refer to any psychological effect, but they are used to encompass a class of variables. The distinction between sensation and perception is not a substantive one, but a conceptual or theoretical one. There are no differences in the fundamental operations of an experiment, which can form the basis for a clear distinction between sensory discrimination and perceptual discrimination. If the experimenter believes that he is manipulating some simple and elementary aspect of the stimulus situation to produce a given class of response, he may be inclined to consider his experiment as one dealing with sensation; if the stimulus situation is more complex, or variables other than environmental are under investigation, the experiment may be considered as dealing with perception. The application of these terms, however, adds nothing to the clarification of the problems under study. The result sought by the investigator is the establishment of a relation between or among variables.

Numerous psychological theories are currently available, which more or less independently encompass the so-called sensory and perceptual variables. If it is realized that sensation and perception are merely terms of convenience and historical heritage, a broader perspective of the general study of perception may be obtained, wherein established facts and principles of behavior related to sensory input may be integrated. In short, while psychology has produced many subtheories involving different levels of abstract concepts and empirical relations, there is need for broader theories of perception in which the traditional sensory variables are included. Sensation and perception are conceptual distinctions, which the experimentalist or theorist may find convenient in everyday language, but their distinction as separate systematic concepts has not been established in the scientific vocabulary.

CLASSICAL PSYCHOPHYSICS AND CONTEMPORARY FORMULATIONS

The Psychophysical Experiment. Since extended treatments are readily available regarding the techniques of psychophysical measurement and scaling (Guilford, 1954; Torgerson, 1958; Thurstone, 1959), this section need only point out the essential features of a psychophysical experiment. Simply stated, a psychophysical experiment is one which uses a particular (psychophysical) method of experimenting in order to obtain data relating the probability of a given response to a given stimulus cue or configuration. According to Thurstone (1927), the psychophysical methods may be divided into two classes: (1) one class, such as the method of average error or the method of minimal changes, requires the physical measurement of some aspect of the stimulus and its control throughout the experiment, and (2) the other class, such as the method of paired comparisons, is applicable in those instances where precise measurement and control of the physical characteristics of the stimuli are not possible. Guilford (1954) refers to the first class of methods as psychophysical methods and to the second class as psychological scaling methods. The two classes of methods differ in that the first class yields end results stated in terms of values on physical scales, whereas the second class provides values on psychological scales. Nevertheless, if the stimuli *are* physically measurable, either class of methods may be used.

The psychophysical experiment in its narrowest meaning is generally considered to encompass those experiments in which the stimuli are measured and controlled as independent variables, regardless of the particular method used in collecting the data. In the psychophysical situation, both stimuli and responses are measured under a highly restrictive set of conditions. Frequently, given a single presentation of the stimulus under a given set of instructions, the subject is permitted to make only one of two alternative responses, "yes" or "no" or their key-pressing equivalents. The exact nature of the subject's task, however, depends upon the specific problem under investigation.

Stevens (1948) has classified the problems of psychophysics into seven categories: absolute thresholds, differential thresholds, equality, order, equality of intervals, equality of ratios, and stimulus rating. The optimal methods for attacking the problems within these seven categories differ from category to category. Torgerson (1958) has provided a classification of the principal ways in which the methods differ, and he has categorized three broad approaches to the measurement problem in the social and behavioral sciences, based upon differences in the source of response variability. These are: (1) the subject-centered approach, in which systematic response variability among subjects is attributed to individual differences, (2) the stimulus-centered approach, in which systematic response variability is attributed to differences in some designated attribute of the stimuli, and (3) the response approach, in which systematic response variability is attributed to variations in both subjects and stimuli. The psychophysical experiment, according to this schema, falls closest to the stimulus-centered approach. One of the features of the commonality of psychophysical experiments, then, resides in the psychologists' search for the conditions and limits of response invariance attributable to specific, measurable properties of the stimulus situation. This holds whether these responses are of the yes-no variety or involve seemingly introspective reports such as louder, warmer, red, or moving. The end result of the classical psychophysical experiment generally provides a graphic plot of some measure of response, e.g., probability of occurrence against some appropriate measure of the controlling stimulus variable. Such a plot is called a psychometric function; from this plot, new data may be derived concerning the subject's discriminatory ability. It should be clear, however, that the particular result of any psychophysical experiment will depend upon the specific theoretical approach from which the problem was derived, the experimental method used in obtaining the data, and the processes involved in the analytical treatment of the data.

THEORETICAL EXPLANATIONS OF SENSORY DISCRIMINATION

The Classical Theory. Much of the work in psychophysics has been centered around the individual's ability to discriminate among different stimulus situations. In the case of absolute thresholds, the experimenter attempts to determine a point on the stimulus continuum such that the stimuli are divided into two classes; the stimuli in one class are capable of eliciting a response, while those in the other class are not. In the case of differential thresholds, a similar attempt is made to locate the point on the continuum of stimulus increments which divides the continuum into two parts such that the increments in one part evoke a response, while those in the other do not. Ekman (1959) has presented a theoretical formulation in which the absolute threshold and differential threshold are treated as facts of the same order, with the absolute threshold taken as a special case of differential sensitivity.

The establishment of the critical threshold value, however, is a difficult matter since this value, whether absolute or differential, is not invariant with time. High, Glorig, and Nixon (1961) have presented four categories of factors affecting the reliability of threshold measurements: physical, physiological, psychological, and methodological. Twenty different types of representative variables, which may affect the threshold value, are listed among these four categories. Thus, it is necessary to resort to a statistical definition of threshold, in which the critical value of the stimulus condition is taken as that value which corresponds to some predetermined response criterion, e.g., 50 or 75 per cent, depending upon the particular psychophysical procedure used in the experiment. The empirically derived measure of threshold is in this way an arbitrary value falling within certain limits of variability.

A theoretical issue which underlies the matter of threshold as a substantive concept is whether the critical value on the stimulus continuum is fixed and precise for a given individual under a given set of conditions, with the variability in time resulting as an experimental artifact. This question is intimately bound to the problem of the relation existing between continuous changes in the magnitude of the stimulus (environmental energy) and consequent changes in the magnitude of the corresponding sensation. Does sensory discrimination proceed in a manner which is best described by an infinite, continuous mathematical function, or is the process discrete and discontinuous? In other words, given a set of data collected in a discrimination experiment, the experimenter is faced with the task of performing some analytical treatment in order to derive the empirical measure or measures in which he is interested. Logically, these measures should be computed from the best-fitting curve relating stimuli to responses; this means, in effect, that the mathematical function describing this curve represents the most probable theory from which the data could have arisen.

The classical theory of sensory discrimination (Fechner, 1860) asserts that "since sensitivity (the inverse value of the absolute or differential threshold) is a variable matter, we have not to measure it as we should a constant; but we can seek to determine (1) its extreme and (2) its mean values; we can investigate (3) the dependency of its changes upon circumstances; and we can (4) make search for laws which hold throughout its variations" (p. 54). The fundamental notion in the classical formulation is that a discriminatory response to a given stimulus depends not only on the controlled magnitude of the stimulus but also upon the independent random influences of an infinite number of minute uncontrolled factors, which combine their effects according to chance (the normal law of error). Whenever the stimulus is applied, these factors interplay to aid or hinder discrimination. It is assumed in this development that at any given moment a fixed value of the stimulus or stimulus increment exists, which divides the physical continuum into two segments, one related to response and the other to no response. Müller (1878) also derived his formulae from this assumption, although expressing the view that a limen is a variable quantity following some form of frequency distribution. Thus, while the threshold may be assumed to have a single definite value, the classical view contends that in the course of experimental determination this value is subject to increases or decreases through random influences.

The most common statement of the classical position is found in the phi-gamma hypothesis. This hypothesis states that the relationship between proportions of response and stimulus magnitudes is best described by the integral of the normal curve of probability. In classical terminology, the psychometric function was known as the phi function of gamma or, more simply, the phi-gamma function. This may be expressed as:

$$p = \int_{-\infty}^{\gamma} \frac{1}{\sqrt{\pi}} e^{-\gamma^2} d\gamma \qquad (1)$$

in which, basically, two parameters are involved, h and γ. The h measures the precision or steepness of the psychometric function and is inversely related to the standard deviation (σ) of the distribution of judgments. Mathematically, $h = 1/\sigma\sqrt{2}$. The deviate measure γ is obtained by multiplying h and δ, where δ is a deviation from the mean expressed in stimulus units, i.e., $\gamma = h\ (S - L)$, in which S is stimulus magnitude and L is the threshold value.

If γ is replaced by the general expression for the normal deviate z such that $z = (X - M)/\sigma$, and σ is used in place of h, the phi-gamma equation obtains the more familiar form of the cumulative normal distribution:

$$p = \int_{-\infty}^{z} \frac{1}{\sqrt{2\pi}} e^{-z^2/2} dz \qquad (2)$$

* A test of the classical theory of sensory discrimination would involve, therefore, a comparison of the proportions obtained in a given psychophysical experiment with the proportions expected or predicted from the phi-gamma function. Support for the theory depends upon the goodness of fit of the theoretical curve to the experimental data. It should be noted, however, that while the phi function of gamma has been the most widely used expression of the classical hypothesis, any other suitable curve might be employed, such as the logistic function recommended by Berkson (1944), provided the curve smooths off the irregularities in the obtained data. However, the logic of the classical theory (Boring, 1917) clearly suggests that an error function of some sort is needed.

An example of the normal ogive applied to data on pitch discrimination has been selected from a study by Flynn (1943). In this study, the subject listened at a sensation level of 55 db to a continuous tone which periodically changed for 0.30 sec from its standard frequency of 1000 cps. His task was to indicate whenever he noted a change. The tone lasted 1.25 min per block of twenty-five trials, with the same size of frequency increment present in each block. The size of increment in the first block was clearly discriminable, while the remaining increments were chosen at random from a predetermined range of values. Figure 7-1 shows a plot of the proportion of correct judgments as a function of stimulus increment (for Subject A, data set III) with the best-fitting normal ogive superimposed on the data points. Each point is based on 70 to 140 judgments. The reported chi-square value obtained in testing the goodness of fit on ten points is 3.815, with a p value of 0.80. Thus, it may be concluded that the obtained data are consistent with the predictions made from the classical theory (phi-gamma hypothesis).

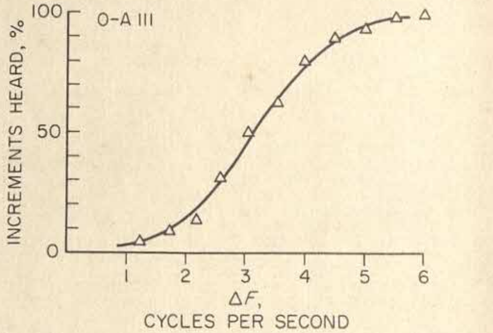


Fig. 7-1. Per cent of increments heard as a function of stimulus increments in cps, with the best-fitting phi-gamma function superimposed on the data points. Data from Flynn (1943, p. 28; Subject A, set III).

Other explanations of the normal ogive commonly found in research on sensory discrimination have been provided by Fullerton and Cattell (1892) and by Thurstone (1927) in his judgment-scaling model. The general view of these authors is that a given stimulus when presented to an observer gives rise to a discriminal process (sensation?), which has some value on a postulated psychological continuum. The same stimulus, when repeated a large number of times, does not give rise to the same discriminal process but to a frequency distribution of processes (discriminal dispersion) because of momentary fluctuations in the organism. This distribution is assumed to be normal. The results obtained in the repeated comparison of two stimuli are then explained in terms of the overlapping distributions associated with each stimulus. If there is considerable overlap in the two distributions, there will be a large number of errors committed in

* To be added from the notes.

making the specified judgments. As the amount of overlapping is changed systematically, the form of the curve describing the course of judgments can be predicted. Given a two-category judgmental task, such as "greater" or "less," the predicted form of the psychometric function is the normal ogive. A discussion of alternative distribution functions may be found in Torgerson (1958).

The Theory of the Neural Quantum. Unlike the classical theory of sensory discrimination which holds that in a psychophysical experiment the relationship of the proportion of given judgments to stimulus values may be accurately described by the integral of the normal probability curve, the theory of the neural quantum (Stevens, Morgan, and Volkmann, 1941) asserts that this relationship is best fitted by a linear function. This implies that discrimination is fundamentally discontinuous and proceeds in a quantal manner by finite, discrete steps. The theory is intended to be consistent with the notion that the basic neural processes, which mediate pitch and loudness discrimination, operate according to an all-or-none principle.

The fundamental ideas involved in the theory of the neural quantum are represented schematically in Fig. 7-2 for the condition of loudness discrimination. Two continua are shown: (1) a stimulus continuum with an arbitrary scale of intensity and (2) a sensory continuum with assumed discrete neural units or quanta, i.e., units related to neural structures or processes which are functionally distinct and discrete. On the stimulus continuum, S is the magnitude of the standard stimulus which completely excites a given number of neural units and may or may not leave some "residual excitation," p; ΔSq is the magnitude of the stimulus increment which will always excite one quantum more than the number completely excited by S; and ΔS is the amount of energy (magnitude of the stimulus increment) which is required to activate a "partially" excited neural unit.

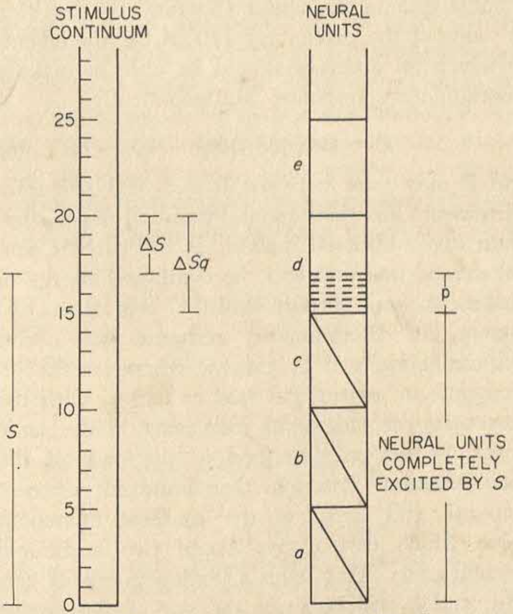


Fig. 7-2. A schematic representation of the basic concepts involved in the theory of the neural quantum. Reproduced from Corso (1956, p. 375).

In an experiment on loudness discrimination, suppose that a certain stimulus (S) excites completely a given number of quanta and that no "residual" excitation (p) is produced by S. If the neural units are stable and constant in size and stimulus increments (ΔS) are added to S in accordance with specified procedures, the increments will not excite an additional quantum until their magnitude reaches a particular value. On the assumption that the observer has adopted a "one-quantum" criterion of discrimination (which is unlikely), this particular value of ΔS when added to S will produce a just noticeable difference (jnd) in loudness. Any value of ΔS greater than this particular value will also produce a difference in loudness. Thus, assuming only the existence of neural quanta and a "one-quantum" judgment criterion, the resulting psychometric function would be of the form of a perpendicular straight line, i.e., plotting the percentage of judgments against incremental magnitude, 0 per cent response would be obtained up to a certain point on the stimulus scale, and 100 per cent response would be obtained for all increments above this point. Such functions, however, are not obtained in experiments on loudness discrimination; hence, a further assumption must be introduced.

This additional assumption holds that the overall sensitivity of the human observer does not remain fixed at a constant level but fluctuates momentarily and randomly through magnitudes considerably larger than a single quantum. Thus, the magnitude of the stimulus increment required to activate a fixed number of neural units will vary with fluctuations in sensitivity. Conversely, a stimulus of given magnitude will activate a varying number of neural units. Since the variation in the number of activated units is assumed to be quantal, all of the available energy in a given ΔS will not necessarily be utilized in a given presentation. At a particular moment, the given ΔS may excite completely a certain number of quanta, but a small amount of residual energy may remain which "partially" excites an additional quantum. While this residual is ineffective by itself to activate an additional quantum, it becomes available for summation with the energy provided in the succeeding stimulus increment and may, thereby, produce a discriminatory response.

The theory of the neural quantum may be summarized by a series of simple equations. At any given moment, the magnitude of ΔS (stimulus increment) necessary to activate a quantum in addition to the total number excited by S (standard stimulus) depends upon the amount of residual energy or "partial" excitation (p). Since ΔSq is the magnitude of ΔS which will always excite an additional quantum, this relation may be written:

$$\Delta S = \Delta Sq - p \qquad (3)$$

This indicates that ΔS will completely stimulate the additional quantum needed for discrimination whenever $\Delta S \geqq \Delta Sq - p$. Consequently, as the size of ΔS increases from some small value, there is a corresponding increase in the expected number of discriminatory responses. The precise manner in which the number of discriminations increases as a function of ΔS depends upon the relative frequency with which the different surplus values (p) occur. If it is assumed that the sensitivity of the observer fluctuates due to the influence of a large number of unknown, independent factors, the surpluses (p) will occur over the same range of neural units stimulated during the course of the random fluctuations in sensitivity, and the distribution of surpluses over this range will approximate a normal curve. By summating the values of p from zero to neural unit size over the several neural units covered by the normal distribution of surpluses, the relative frequency of occurrence of each p value may be determined. The result is a rectangular probability function, which indicates that given a standard stimulus any surplus value is as likely to occur as any other. Since the surplus (p) fluctuates between $0 \leqq p \leqq \Delta Sq$, and any value of p is as likely to occur as any other, the proportion of times that an increment will activate an additional neural unit is given by:

$$f_1 = \Delta S/\Delta Sq \qquad (4)$$

where f_1 is the relative frequency of the instants during which ΔS excites one additional quantum. Equation (4) indicates that the proportion of responses increases as a linear function of ΔS and that f_1 may vary between zero and one.

The assumption of a "one-quantum" criterion of judgment, however, has not been supported by the evidence of Békésy (1930), Miller and Garner (1944), and Blackwell (1953); usually *two* additional quantal units must be activated before a discriminatory response is reported. The two-quanta criterion makes it easier for the observer to distinguish between the excitation resulting from the combined effect of surplus and adequate increment and that associated with random fluctuations, i.e., surplus alone, which may attain neural unit size. In this case, the proportion of times that a given increment will produce a discriminatory response may be expressed as:

$$f_2 = (\Delta S/\Delta Sq) - 1 \qquad (5)$$

where f_2 is the relative frequency of occurrence of the instants during which ΔS excites two additional quanta. Equation (5) may be rewritten in terms of the percentage (P) of the increments to which an observer should be able to make a discriminatory response. Mathematically,

$$P = 100[(\Delta S/\Delta Sq) - 1] \qquad (6)$$

and P may vary between 0 and 100 per cent. Increments less than quantal size will never stimulate two additional quanta; as the surplus cannot exceed one unit and the combined energy of increment and surplus will be less than two quanta, no discriminatory response will occur. Discrimination will be present whenever the increments are of quantal size or larger, with the percentage of judgments increasing in the same linear manner as described in the case of the "one-quantum" criterion. One hundred per cent response will occur at the smallest increment value which can always excite two additional quantal units. This value is independent of surplus, and it will be twice the size of the largest increment to which a response never occurs. The largest increment to which a response "just never" occurs is taken as a measure of one quantal unit; the smallest increment which yields 100 per cent response is taken as two quantal units. On the assumption that neural units are equal in size, a two-to-one ratio may be expected to obtain between the value at which the psychometric function reaches 100 per cent and the value at which it first departs from 0 per cent, provided the observer has adopted a two-quanta criterion of judgment.

If the experimental conditions of the quantal method and the underlying assumptions of the quantum theory are satisfied, a typical psychometric function such as that for loudness discrimination would be expected to exhibit three characteristics: (1) a linear relationship from 0 to 100 per cent between the percentage of judgments and the magnitude of stimulus increments presented; (2) a two-to-one ratio between the values of stimulus increment at the 100 per cent (2 quanta) and 0 per cent points (one quantum) of response; and (3) a slope of the psychometric function which is inversely proportional to the intercept on the *X*-axis, i.e., size of stimulus increment. These characteristics of the psychometric function should hold for any quantal experiment on differential sensitivity, whether it involves loudness, pitch, brightness or any other psychological dimension.

The applicability of the quantal hypothesis to sensory data is shown in Fig. 7-3 in which the function was selected from among those presented by Stevens, Morgan, and Volkmann (1941) for the "quantum of frequency discrimination." In this experiment, the general procedure was to have the observer press a key whenever a steady, continuous tone of 1000 cps presented in an earphone at a particular sensation level seemed to change in frequency. An increment lasting 0.3 sec was added every 3 sec to the base tone of 1000 cps. The increments, all of which were less than 10 cps, were presented in a series of 25, with the same-sized increment throughout the series. A total of 100 judgments was made by each observer at each of several (8 to 10) frequency increments. In the treatment of data, linear functions were fitted to the experimental values by the method of least squares and, according to chi-square tests, showed better fits than corresponding phi-gamma functions.

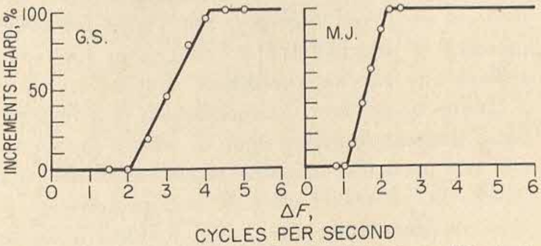


Fig. 7-3. Linear psychometric functions fitted to sets of experimental data on frequency discrimination, supporting the neural quantum theory. Data from Stevens, Morgan, and Volkmann (1941, p. 325; observers, G. S. and M. J.)

Despite certain criticisms on the treatment of data (Flynn, 1943; Lewis and Burke, 1949), it is clear that the data in Fig. 7-3 may be adequately represented by a rectilinear function. Furthermore, the prediction of a two-to-one ratio between the values of the function at 100 per cent and 0 per cent seems to hold in both cases; also, the inverse relation of slope to intercept seems to be confirmed. Observer GS has a neural quantum of 2 cps (intercept at 0 per cent) and observer MJ, a quantum of 1 cps; accordingly, the slope of the function for observer GS is less than that for observer MJ.

Whether the theory of the neural quantum provides, in general, a better explanation of sensory discrimination than the classical theory remains to be determined. Corso (1956) performed a review of the literature on this issue and concluded that "unequivocal support of the neural quantum theory is, for the most part, lacking" (p. 391). The tenability of the hypothesis as opposed to the phi-gamma function is extremely difficult to evaluate due to certain severe limitations in methodology and in the statistical treatment of data. Furthermore, it seems that the observers must be competent, well-motivated, and performing at a level which eliminates all nonphysiological sources of variance. It is contended that "noises" of any kind from different sources will obscure the quantal function and will serve to generate sigmoid psychometric functions resembling normal ogives.

It should be noted that the primary emphasis of quantum theory has been on problems of differential sensitivity, with relatively little regard for measurements related to absolute threshold. However, in a series of experiments designed to test the applicability of the theory to hearing thresholds, Corso (1961) concluded that 'steps' in the audibility function, rather than representing neural or quantal processes, could be accounted for in terms of experimental methodology. Furthermore, it was found that both the quantal and phi-gamma hypotheses were equally acceptable in predicting the 90 per cent audibility function for pure tones of low frequency.

Barlow (1961) has presented a penetrating analysis in which he proposes an alternative theory to account for observed rectilinear psychometric functions in discrimination experiments. Briefly, this position states that "thresholds are internal decision processes that are automatically adjusted to such a value that false positive responses occur at a constant, low rate. The hypothesis . . . requires only that there should be some kind of random disturbance or noise, and that the threshold should be adjusted to a level such that false positive responses would rarely be caused by it" (p. 788). The development of this view predicts that the slope of the psychometric function "will be equal to 1/threshold, or, allowing for different false positive rates and departures from normality, the slope will be inversely proportional to threshold" (p. 789). In general, the alternative theory accounts for the three predictions derived from the neural quantum theory of sensory discrimination and asserts "that the apparently rectilinear functions are approximations to, or distortions of, ogival functions" (p. 787).

Recent evidence in neurophysiology also tends to detract from the neural quantum theory. It is now recognized that the all-or-none principle of nerve activity, a fundamental assumption in the derivation of the theory, is confined to the axon process of a neuron; graded responses, which vary in accordance with the strength of a given stimulus, occur at both ends of the nerve cell (Bishop, 1956). Thus, the nerve activity underlying sensory discrimination may not be a simple all-or-none mechanism, but it may reflect the resultant synthesis or additive combination of a large number of graded responses which, although delivered via the axon as successive discrete impulses, are nevertheless reconverted into graded responses within the branching terminals of each neuron. It appears that the processes of neural analysis and synthesis occur at the presynaptic and postsynaptic regions of the nerve cell, with the all-or-none impulse serving merely to transport graded responses within the nervous system, where necessary.

The Theory of Adaptation Level. Both the classical theory and the quantum theory of sensory discrimination are concerned with the manner in which the proportion of judgments reflects changes in a highly restricted aspect of the experimental situation, specifically, the magnitude of the stimulus or stimulus increments. These approaches do not explicitly take into account the many ways in which the judgments may be affected by numerous other factors operating within that situation. It is now recognized that the judgments of human observers are relative, i.e., they are made with respect to a "frame of reference," and a general theory of judgment has been developed to account for a wide range of perceptual phenomena, from psychophysics to attitude formation and social behavior (Helson, 1947; 1948). This theory embodies the concept of *adaptation level* as an intervening variable, which provides the primary unifying principle underlying an otherwise apparently divergent set of psychological studies.

The theory of adaptation level is based on the assumption that order prevails in the spatio-temporal patterns which characterize sensory stimulation. This order is revealed in psychophysical judgments and qualitative categorizations, such as "very, very heavy," with the result that stimuli may be scaled on a continuum extending in opposite directions from a neutral zone or point of indifference. Stimuli near this value fail to elicit an organismic response, other than neutral, doubtful, equal, or some similar judgment, whereas stimuli above and below this point elicit positive (acceptance) and negative (rejection) responses, respectively, in varying degrees. Stimuli located at the point of indifference are said to be at adaptation level (AL), and the AL may be quantitatively specified by giving the value of the stimulus eliciting the neutral response. AL is not a static and fixed quantity, but varies for every moment of stimulation; it changes in time and reflects any varying conditions of stimulation. As a "frame of reference" concept, AL is a function of all the stimuli acting upon or within the organism at a particular moment, as well as in the past. Thus, the effects of all past and present conditions of stimulation are pooled to form a single, given AL with respect to a specified class of stimuli.

Specifically, the quantitative theory of AL is derived from the notion that there are three primary determiners of AL during a given experiment: (1) the experimental stimuli, i.e., the stimuli being judged or rated; (2) the contextual stimuli, i.e., the stimuli forming the background for the judgments; and (3) the residual factor, i.e., the effects of all related, previous stimulation and constitutional factors having their locus within the organism. The AL of the organism represents an adjustment to the combined effects of these three classes of stimuli and provides a "frame of reference" or "equilibrium level" to which all responses are referable on a bipolar scale. This holds whether the AL concept is applied to the behavior of individuals or to the behavior of groups (Rosenbaum and Blake, 1955). As opposed to the classical or quantal approach to psychophysics, AL theory provides an objective basis for introducing and assessing the relative contribution of the inner determinants of behavior (needs, drives, motives) as well as the outer determinants and, hence, it is applicable to a range of phenomena broader than sensory processes alone.

If the assumptions of AL theory are appropriate and operative in behavior, it should be possible to derive a formula for a given set of circumstances, which will contain terms for the three primary determiners of AL with their appropriate weights such that the predicted AL will correspond to the obtained AL for that particular situation. Helson (1947) has shown that for a wide variety of experimental conditions AL can be closely approximated by considering that it is the weighted-geometric mean of all stimuli being judged. For viewing achromatic stimuli, the most adequate equation for predicting AL is of the form:

$$A = K(\bar{S}B^3)^{1/4} \tag{7}$$

where A is the adaptation level or adaptation reflectance, $\bar{S}$ is the logarithmic mean of the reflectances of the test stimuli of equal area exposed on a given background, B is the reflectance of the background, and K is a fractional constant. Helson's principle of color conversion states that in every viewing situation an AL is established, such that stimuli above this level take on the hue of the illuminant, stimuli below this level take on the hue which corresponds to the negative afterimage of the illuminant (complementary color contrast), and stimuli at or near this level are seen as achromatic or weakly saturated colors of indeterminate hue.

The weighted log mean formula exemplified by Eq. 7 has been extended to other types of psychological studies, such as lifted weights and sound intensities, and it seems to hold extremely well. It should be noted, however, that of the three factors influencing AL, Eq. 7 includes only two, experimental and contextual stimuli. To account for individual differences in particular experimental situations, an additional term representing the residual factor must be included in the general formula. If Helson's principle is accepted that AL is the weighted geometric mean of all stimuli affecting it, the general equation becomes:

$$A = \bar{S}^x B^n R^m \tag{8}$$

where A is the adaptation level, $\bar{S}^x$ is the weighted logarithmic mean of the experimental stimuli, B^n is the weighted logarithmic mean of the contextual stimuli, and R^m is the weighted logarithmic mean of the residual stimuli. To eliminate the need for a constant multiplier for the product on the right-hand side of Eq. 8, the restriction is imposed that $x + n + m = 1$. Rewriting Eq. 8 in logarithmic form:

$$\log A = x \log \bar{S} + n \log B + m \log R \tag{9}$$

An example of a solution to the general expression given by Eq. 9 may be found in Philip (1949), who applied AL theory to data previously published by Turchioe (1948) on the duplication of time intervals. By substituting appropriate values for the several variables in the fundamental equation, including A, a number of equations containing the weighting coefficients were obtained. Simultaneous solutions of these equations yielded the exact values of the desired constants. If it is assumed that experience previous to a particular experiment has no effect on AL, a simpler solution is possible since the weight m will equal zero and the third term on the right-hand side of Eq. 9 will vanish. In this case, the values of A, $\bar{S}$, and B are known from the experiment and $x + n = 1$ is given by definition; consequently, two linear equations involving x and n may be set up and solved simultaneously to obtain the values for the weighting coefficients. By forming a ratio of these two values, the relative importance of the two classes of stimuli in predicting AL may be determined.

Although the weighted log mean formula defines the value of AL and provides an estimate of the relative contributions of primary, contextual, and residual stimuli, it does not permit the prediction of responses to the *particular* stimuli used in a given experiment. Helson (1948), however, has developed a mathematical equation for such prediction which specifies the relations between responses, stimuli, and AL. Three assumptions are required in the development: (1) for a given attribute of judgment, the response to any experimental stimulus depends on the distance of the stimulus from AL; (2) the number of JNDs from AL to a given stimulus is directly related to the distance between AL and the stimulus; and (3) the magnitude of a perceived difference depends upon the values of both the stimuli being judged and the AL, but *equal* weighting of the two factors is not a necessary condition. Since the derivation of the predictive equation does not require that the number of JNDs in assumption (2) be known, the experimental stimuli may be judged in terms of the qualitative categories of a rating scale, provided the categories can be translated into a numerical interval scale for computational purposes. From these assumptions, the derived equation (Helson, 1959) is:

$$J = \frac{K(X - A)}{X + (1 + b)/(1 - b)A} + 0.5K \qquad (10)$$

where J is the response or judgment in numerical terms, A is the adaptation level, K is the numerical value of the highest point on the scale into which rating categories have been translated and is equal to twice the value given to the middle or neutral category of the scale, and b is a constant. When Eq. 10 is written in linear form, the constant b is the value of the y (response) intercept. Omitting the constant $0.5K$, the linear form of Eq. 10 is:

$$\frac{K + J}{K - J} = \frac{X}{A'} + b \qquad (11)$$

where $A' - bA' = A$. Given the values of K, J, and X for a particular experiment, a least-square solution may be applied to Eq. 11, such that the values for A' (and therefore, A) and b can be determined by means of the normal equations for the slope ($1/A'$) and y intercept (b) of linear functions.

Referring to Eq. 10, it may be seen that the judgment (J) corresponding to the point of indifference or neutral category is given when $X = A$. Also, the contribution of A to the judgment or response is given by the constant $(1 + b)/(1 - b)$ and is relative to the unit weighting of the experimental stimuli (X). The general form of the curves yielded by Eq. 10 is negatively accelerated and resembles a logarithmic function; consequently, this indicates that equal differences in judgment require increasing increments in stimulus magnitude.

Michels and Helson (1949) have shown that, accepting the concept of AL, a significant change in the application and interpretation of Fechner's logarithmic psychophysical law is in order. The derivation of the new formula does not require that JNDs be equal or "integrated," and it does not employ the absolute threshold on the stimulus scale as the unit of measurement. The Michels-Helson revision is of the form:

$$J = C \log S/A, \qquad (12)$$

where J is judgment or response, C is the slope constant of the equation in expanded form, S refers to stimuli, and A is adaptation level. In this expression, A replaces the traditional absolute threshold and the magnitude of the response is a function of the distance of the stimuli from AL, rather than from threshold. It follows immediately that, accepting the Michels-Helson derivation, the origin of sensory scales may be considered to correspond to the adaptation level of the observer, rather than to the absolute threshold or some other "effective" (Stevens, 1961) threshold.

The reformulated Fechner law has been widely applied in the construction of sensory scales, such as the veg scale (Michels and Helson, 1954), the bril scale (Michels, 1954), and loudness scales (Michels and Doser, 1955). The importance of AL theory in this type of problem is that scaling judgments have tended to show a strong series effect (Garner, 1954). Thus, a stimulus presented as a standard in the method of fractionation will be expected to have a different scaled value from when it is presented as a member of a series of stimuli. This means that adaptation, contrast, and residual factors must be considered in deriving psychological scales, despite any attendant difficulties which may be introduced by these factors.

The outstanding feature of AL theory is that it provides a quantitative, analytical approach to a wide range of psychological problems. An example of the application of AL theory to verbal behavior may be found in a study by Helson, Dworkin, and Michels (1956). Four groups of a change in [illegible] with respect to

seventy-five subjects each were required to specify the numbers implied by twenty-six common terms or phrases, such as "many" and "a considerable number," while at the same time maintaining a particular number as the size of the population to which these terms were applied. The reference numbers were 100; 1,232; 144,690; and 1,728,583, with one number assigned to each group. The results verified the predictions of AL theory: the high background numbers, presumably by raising AL, lowered the estimates of the descriptive terms, and the low background numbers, presumably by lowering AL, increased the numerical estimates. It was also found that real differences existed between the words and phrases in terms of the changes they exhibited with respect to the background (population) influence. Helson (1959) has provided a relatively complete account of AL theory in which numerous other studies are reviewed, showing the applicability of the theory to problems on learning, affective and motivational behavior, personality and social behavior, and intelligence.

Some data have been obtained, however, which indicate that while the weighted log mean formula provides a good first approximation of AL, additional refinements to or modifications of AL theory are required. Parducci, Calfee, Marshall, and Davidson (1960), Parducci and Marshall (1961*a*) and Parducci and Marshall (1961*b*) have been concerned with the effects of stimulus context on simple perceptual judgments involving size of numbers and lengths of lines. The results of these studies indicate that AL varies systematically with variation in either the midpoint or median of the series of stimuli, but not with the mean. Weiss and Ten Eyck (1961) have found that, contrary to expectations derived from AL theory, shock administered to human subjects judging the size of squares did not lower AL; also, Weiss and Ten Eyck (1960) found that prior experience with a three- or seven-category scale did not differentially affect judgments of the size of squares on a five-category scale. The interpretation of the findings nevertheless suggested that the data did not establish that judgments could not be affected by prior experience with other kinds of response systems. Meier, Foshee, Wittrig, Peeler, and Huff (1960) have questioned the meaning of the term "residual factor" and indicate that, in a particular experiment, the "intrinsic aversive character of flashing illumination," i.e., innate $S - R$ relations rather than the influence of previous experience, is the factor which underlies the preference of mice for constant ambient illumination. Morikiyo (1959) has stressed the need for the introduction of certain time parameters into AL theory, with particular reference to the effects of time order in perceptual judgments. It appears, therefore, that while AL theory provides a substantial framework embodying a diverse set of psychological problems, current research indicates some adjustments may be required in the quantitative formulation of the theory or its underlying concepts.

concep

A CURRENT S-R MODEL FOR SENSORY PROBLEMS

BEHAVIORISTIC VERSUS PHENOMENOLOGICAL TERMINOLOGY In a preceding section of this chapter, it was emphasized that the scientist in psychology attempts to establish the degrees and types of systematic relationships which obtain among variables related to behavior. Zener and Gaffron (1962), in their detailed exposition on perceptual experience, have questioned the appropriateness of the term "behavior" to designate the entire sphere of psychological events, and they have proposed that "behavior" be subdivided into those events which have an experimental or phenomenal character, among other properties, and those which do not. Given this distinction, four types of relationships are suggested with respect to the range of problems encompassed in "perception": (*a*) experiential relationships—those between direct experience and its antecedent conditions or its behavioral consequences; (*b*) cognitive relationships—those between cognitive or inferential acts and their determinants or behavioral consequences; (*c*) discrimination—relationships between different stimulus conditions and differential behavior; and (*d*) behavior control—relationships between specific behaviors and their external releasing or stimulating conditions.

A review of these four types of relationships reveals that a phenomenological terminology cannot be avoided in establishing experiential relationships, if this is held to be an appropriate objective in psychology. The remaining categories, however, can be managed by adherence to a set of nonexperiential terms and, of these categories, that of discrimination is most relevant to the study of sensory processes. To be certain, terms such as "loud," "hue," and "hot" are used in sensory experiments, but, when the significance and meaning of these terms is analyzed in relation to these experiments, it is seen that these terms are not to be interpreted as providing experiential data or comprising a phenomenal report. In the objective language of contemporary psychology (Graham, 1958), such terms are used as labels which relate stimulus variables to differential responses made by a subject under a particular set of experimental instructions. In this situation, the experimenter is concerned primarily with the frequency of occurrence of a given response and not whether two subjects reporting "blue" under the same set of conditions have the same perceptual experience. Furthermore, a given word, e.g., "heavy," used by the experimenter in his instructions to the subject does not have the same analytical meaning as that word used by the subject in his verbal response. In the first case, "heavy" is taken as a term in everyday language; in the second case, the term implies an inferred effect occurring within the instruction-stimulus-response chain of events, either in the material characterizations of physiology or in the symbolic representations of behavioral theories.

Thus, while some terms in psychophysical experiments seem to denote experiential properties similar to those in phenomenological experiments, the similarity is nonexistent when it is realized that in an objective psychology such terms derive their meaning from relationships involving antecedent conditions and discriminatory responses or from relationships involving abstract mathematical concepts, not from their expressed content as introspective reports elicited under a given set of testing conditions. Since the introspective experiment does not attempt to quantify either stimuli or responses, quantitative relations of stimuli and responses cannot be established in this situation, and the reports from such experiments serve mainly as leads to hypotheses which can be tested in more rigorous psychophysical experiments.

A GENERAL THEORETICAL FORMULATION In developing a stimulus-response approach to psychophysics and perception, i.e., in deriving a formulation based on the behavioristic assumption that response is a function of certain specifiable variables, Graham (1950) has proposed that psychophysical functions may be described as special cases of a general behavior function. The data of sensory and perceptual experiments are in this way included in the broader field of behavior theory. Specifically, the general relation is given by:

$$R = f(a, b, c, d \ldots n \ldots t \ldots x, y, z) \quad (13)$$

where R refers to response; a, b, c, d refer to specified properties of stimuli; n refers to the number of stimulus presentations; t to time; and x, y, z refer to specified conditions of the organism. The organismic variables (x, y, z) may be physiologic or inferred from the effects of instructions which serve as stimuli, or they may represent the historical consequences of prior stimulation. The terms in Eq. 13 need not be independent of each other.

Psychophysical experiments, according to this view, are those which involve relationships between R and stimulus variables $(a, b, c, d \ldots)$, with all other terms of Eq. 13 as parameters. For example, the data from an experiment involving the method of constant stimuli or method of limits may be expressed as a special case of Eq. 13:

$$R = F(a, x_1) \quad (14)$$

where the response R is some function F of stimulus property a, with the effect of instructions denoted by the constant x_1.

Other expressions may be readily derived from Eq. 13 to designate experiments in areas outside psychophysics. Learning reduces to $R = f_1(n)$, where n refers to the number of presentations of constant stimuli a, b, c, d . . . , and all other variables assume parametric values. Studies in motivation may be represented by $R = f_2(x)$, where x refers to the deprivation state of the organism, with all other variables held constant. In these and other instances, the task of the experimenter is to establish a precise statement of the relations involved among the several classes of variables, stimulus, response, and organismic whether "real" or inferred. It should be noted, however, that the classification of experiments as "psychophysical," "learning," "motivational," "perceptual," etc., adds little or nothing to the understanding of the nature of the underlying relationships. It may even be that such designations may hinder advances in an area by focusing the experimenter's attention on a restricted segment of variables, thereby precluding a more expansive view of the problem under consideration. The objective is to define relationships, not to categorize experiments into logictype pigeonholes. Equation 13 is simply a general expression to encompass the wide range of problems encountered in contemporary psychology. It makes no attempt to specify the particular aspects of variables to be studied nor to order the relative importance of functional relations which may be investigated. It does, nevertheless, provide a general theoretical framework, within which the behavioral psychologist may orient his thinking. Graham (1952) has, in fact, shown how experiments dealing with figural aftereffects, binocular space perception, and the influence of motives on the estimation of coin size can be fitted into his formulation.

SOME BEHAVIORAL STUDIES OF SENSORY PROCESSES

Vision. An example of the manner in which the symbolic representation expressed in Eq. 14 may be applied is presented with respect to the color-naming data of Bauman and Walraven (1957). In this study, a test flash of monochromatic light was presented in the fovea of the dark-adapted right eye of a normal trichromatic observer. The experimental arrangement made it possible to appropriately control the wavelength, intensity, flash time, and visual angle of the flash. Part of the study involved small and short flashes in which the diameter of the circular test stimulus was 2 min of arc and the flash time 0.05 sec, with the flash occurring every 3 sec. As the intensity of the flash was fixed at different levels by adjusting the current in the calibrated light source, the observer was required to "indicate whether the perception was reddish, yellowish, greenish, bluish, whitish, or colorless, or absent, or of uncertain color, but not reddish, not yellowish, etc." The experiment was considered to represent a "yes-no response study in which the procedure of the absolute thresholds investigation is followed . . . . The simple yes-no answers now correspond with color or not, reddish or not, etc. The experimental procedure is thus essentially the same as in the measurements of the absolute thresholds" (p. 835).

The results of this part of the study showed that thirty repetitions of a particular flash frequently lead to contradictory color names. A flash of 550 mμ, for example, was sometimes called greenish, but sometimes also whitish or even reddish. Going from 700 mμ to 550 mμ at threshold intensity for color recognition, the percentage of "red" responses decreased while that for "green" increased, in accordance with the Young-Helmholtz theory. "Yellow" did not occur outside the region 570 to 590 mμ. For all wavelengths, even up to 700 mμ, there was found to exist a region of achromatic brightness, in which visual observation was present but hue was absent. The general conclusions from these and other results presented in the study indicate that, by comparison with quantum theoretical studies conducted earlier on absolute thresholds, "green" excitation requires three quanta of light and "red," two quanta.

The types of functional relation developed in this study may be readily expressed in terms of Eq. 14, where R is the color-naming response, a is the variable intensity of the test flash, and x_1 is the constant effect of instructions which limit the range of possible responses. The parameters of Eq. 14 include such factors as the retinal area of stimulation, the size of the test flash, the wavelength of the flash, the duration of the flash, and the dark-adapted state of the observer's eye.

A more extended treatment of the application of Eq. 14 may be found in Graham (1952). In that paper, the notion of a perceptual or critical value function is developed from the fundamental concept of the differential threshold. Given a set of experimental conditions, the differential threshold or just-discriminable increment (ΔL) is determined according to the requirements of some psychophysical method such as the method of constant stimuli. This threshold value, then, specifies a critical value of the stimulus corresponding to a predetermined response criterion. By systematically varying some aspect of the basic experiment such as the luminance of the standard stimulus and repeating the experiment successively, a series of threshold values is generated which may be plotted to yield a perceptual function. This function indicates how the second-order datum, the determined threshold ΔL, varies with changes in some independent variable, e.g., the luminance of the standard. The perceptual function derived from a sequence of psychophysical experiments may be expressed symbolically as:

$$b_c = (a, x_1, R_{\%}) \qquad (15)$$

where b_c is a critical aspect of the stimulus which is represented as a function of another aspect of the stimulus a, x_1 is the constant effect of instruction, and $R_{\%}$ is the constant criterion of response.

Audition. The situation described by Eq. 15 with respect to differential thresholds in vision has its parallel in audition. The classic data on auditory intensity discrimination are those of Riesz (1928).

In this investigation the differential intensive threshold for twelve observers was measured as a function of intensity and of frequency using the method of beating tones. The measurements were made at frequencies from 35 to 10,000 cycles per second (cps) and over an intensity range from the threshold of hearing to the threshold of feeling. Changes in intensity were produced by sending two alternating sinusoidal currents, whose frequencies differed by 3 cps, through a special telephone receiver held firmly against the observer's ear by an elastic headband. Since the two tones differed in frequency by 3 cps, the observer heard a single tone whose intensity fluctuated from maximum to minimum three times per sec. This procedure was adopted to produce a simple intensity fluctuation, which was not complicated by the scattering of energy to frequencies higher and lower than the impressed frequency. For a given stimulus presentation, the intensity of one tone was set at a desired value above threshold (sensation level), and the intensity of the second tone was set far below threshold. The intensity of the second tone was then increased until the observer was just able to detect beats. About twenty judgments were made for each condition to determine the size of ΔE required to produce noticeable beats.

The relative differential threshold ($\Delta E/E$) obtained by averaging the data of the twelve observers is shown in Fig. 7-4 as a function of sensation level, i.e., 10 $\log_{10}$ (E/E_0), where E_0 is threshold intensity and E is the intensity value of the standard.

It was found that at any frequency, an expression of the form:

$$\Delta E/E = S_\infty + (S_0 - S_\infty)(E_0/E)^r \qquad (16)$$

could be made to represent $\Delta E/E$ as a function of intensity by a suitable choice of the constants S_∞, S_0, and r. S_∞ is the value (from .1 to .3) which is approached by $\Delta E/E$ at high intensities, S_0 is the value (from .5 to 14) of $\Delta E/E$ at the threshold of hearing, and r is some appropriate exponent between .25 and .5. S_∞, S_0, and r are parameters which as they vary with frequency determine the family of curves relating the Weber fraction and the intensity of the standard stimulus.

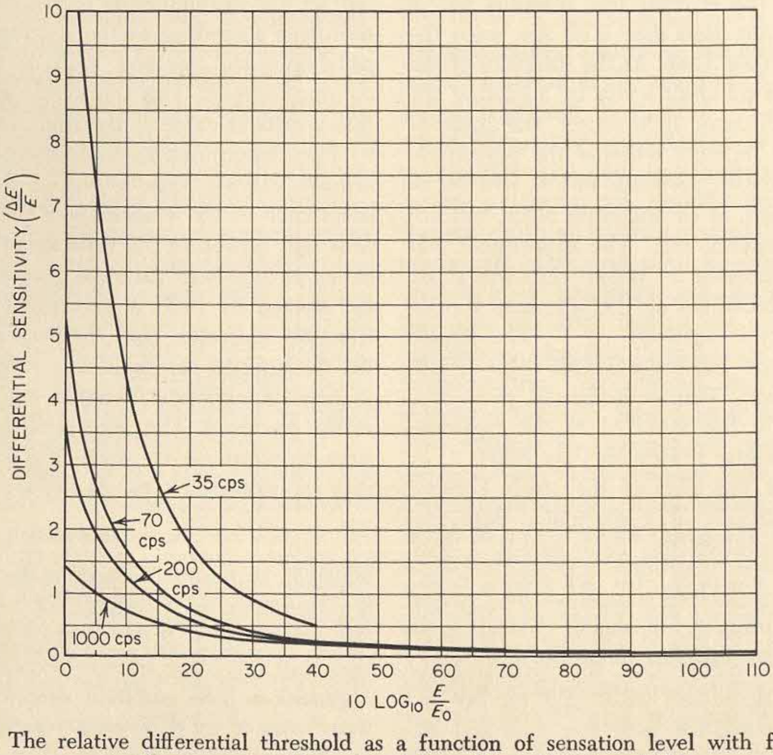


Fig. 7-4. The relative differential threshold as a function of sensation level with frequency as the parameter. Reproduced from Riesz (1928, p. 872)

The explicit relationship given by Eq. 16 can readily be rewritten in the general form of Eq. 15, and any one of the curves in Fig. 7-4 may be considered to represent a perceptual or critical value function. For a given frequency, the curve shows how the threshold value varies as a function of a controlling variable, i.e., intensity.

Gustation. In reviewing the sensory processes and their relation to behavior, Pfaffmann (1962) has presented the sense of taste as a model *S-R* system. Although a portion of this article was concerned with problems on the electrophysiology of taste and the mechanism underlying sensory discrimination, considerable emphasis was placed upon the informational content of sensory inputs. Specifically, attention was directed toward functions of the stimulus other than those related to discrimination per se, with particular reference to "reinforcement." It is well known that certain stimuli have specific properties such that their presentation tends to increase the probability of a given response upon subsequent presentations. Reinforcement is taken by many behavior theorists as a stimulus function intimately bound to the sensory control of behavior, but the particular property of the stimulus (or response) which serves as reinforcement remains to be determined in most cases.

To illustrate the *S-R* model in gustation, reference is made to a study by Collier and Siskel (1959). In this study, sixteen male albino rats, after an appropriate training period, were maintained on a 23-hr food privation schedule and an ad libitum water schedule for a 64-day experimental period. During this period the subjects were tested under a total of sixteen treatment combinations, with the treatment combinations consisting of the factorial pairing of four amounts of reinforcement (4, 8, 16, and 32 per cent sucrose solutions) and four periodic reinforcement intervals (.5, 1, 2, and 4 min). The subjects were tested 45 min per day for four days under each treatment and the number of bar-pressing responses in a Skinner box was recorded cumulatively at 5-min intervals. Each bar-pressing response delivered .02 ml of fluid reward, ±.002 ml.

The rate of bar pressing as a function of sucrose concentration and the interval of periodic reinforcement for the first 5 min of the last session (4th day) in each treatment combination is shown in Fig. 7-5. The curves in Fig. 7-5 indicate that the rate of responding is higher for the stronger concentrations of solution and the shorter intervals of reinforcement. Although additional data are presented by Collier and Siskel (1959), these will suffice for the present purpose.

The results of this study, combined with numerous others, suggest that a sweet taste is rewarding, whether or not it is nutritive, and that performance is an increasing function of stimulus concentration whenever sensory effects are maximized. Theoretically, it should be possible to obtain a monotonic stimulus function (among other functions) by selecting the appropriate combination of concentration, volume, interval between reinforcement, and number of reinforcements. This relationship may be expressed in terms of the general equation, Eq. 13, as follows:

$$R = f(a, b, n_r, t_r) \tag{17}$$

where R is the rate of bar pressing, a is the concentration of the sucrose solution, b is the volume of liquid delivered per response given as an explicit constant, n_r is the number of reinforcements and t_r is the interreinforcement time interval. According to Smith and Duffy (1957), when the reinforcement is limited to taste, whether the "rat is hungry (or thirsty) has nothing to do with it"; hence, no term has been included in Eq. 17 for the hunger or thirst drive component.

It should be stressed at this point that the theoretical appropriateness of Eq. 17 in terms of learning theory is of no immediate concern in the

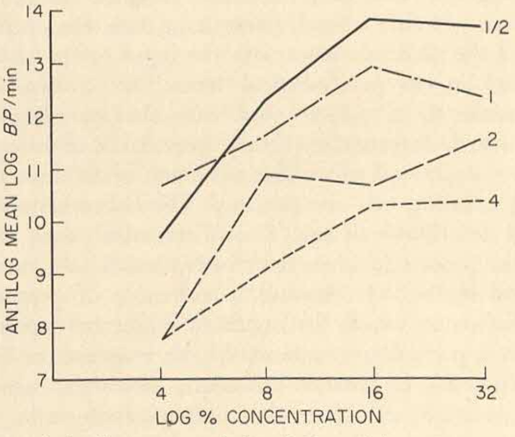


Fig. 7-5. Illustration of the *S-R* model in gustation, in which rate of bar pressing for the first 5 min of the last session (4th day) is shown as a function of sucrose concentration and interval of periodic reinforcement in min. Data from Collier and Siskel (1959, p. 116).

present paper. The example is included to show that certain classes of behavioral response are related to sensory inputs and may be treated symbolically within the general theoretical formulation of Eq. 13. In terms of gustation, sensory stimulation of the taste receptors is relevant to the control of ingestive behavior and the associated instrumental responses leading to ingestion. Equation 13 does not, however, provide a theoretical account of these or any other effects related to the study of sensory processes; such accounts must be developed from theoretically oriented programs of research.

INTEGRATION THROUGH INFORMATION THEORY

THE INFORMATION MODEL APPLIED TO PSYCHOLOGY The *S-R* model described by Eq. 13 was presented as a general formulation within which problems of sensory processes could be viewed in the context of behavior theory. Information theory provides another integrative approach to the study of behavior, specifically, the behavior of communication systems. The application of information theory in psychology requires that the behaving organism be regarded as a communication channel and, consequently, makes it inappropriate to consider the situation in terms of the traditional S-O-R paradigm.

Information theory is a formal or mathematical theory and, strictly speaking, it can be regarded as advanced correlation theory. Given a behaving organism as a communication channel, the problem becomes one of determining how the output of the channel varies with the input to the channel or, in psychological terms, how responses made by a subject vary with the stimuli presented. Information theory provides a technique by means of which the efficiency of information processing can be evaluated. The entire situation is describable in analytic and objective terms, and the process involves the observation of two physical facts: the stimulus or ensemble of possible events to which the organism might be exposed in a particular situation and the response, or the ensemble of possible behaviors, which the organism might exhibit in that particular situation. As far as information theory is concerned, the organism is considered to be a "black box." The significance of the "box" or mediating system lies in what it can accomplish and not on its internal design. The task is to measure the efficiency with which the input information to the channel is coded and transmitted in the channel to yield an output of responses, i.e., output information. There is not direct observation of the events occurring within the channel, only observations of input and output. As in the earlier case when considering the chain of events linking stimuli and responses, observations in terms of information theory may be made at any loci within this chain, whether at the retina, visual cortex, motor cortex, or any other place in the "black box." The observations, however, are always in terms of inputs and outputs and their location of measurement determines the outer boundaries of the mediating system under investigation.

Information theory is one theory among many which is concerned with the analysis and description of probabilistic events occurring in a time series, i.e., stochastic processes. This implies that analyses made within this framework, including those in psychology, relate to processes which are fundamentally statistical in character. The "black box," therefore, operates only in terms of conditional probabilities: a given input will be followed by a given output with a particular probability under a given set of circumstances. It should be noted, however, that adherence to such a position does not demand that one subscribe specifically to information theory; consider, for example, some of the current theories of learning or perception based on concepts of probability. The application of information theory in psychology does, nevertheless, require that behavior be viewed as a temporal sequence of events occurring within a sampling space—or an ensemble of possible events. If a complete set of statements can be made covering all possible pairs of inputs and outputs, then the description of the communication channel is complete and, for psychology, contains all there is to be known about the ability of the behaving organism to perform the functions of information processing.

It must be stressed that information theory should not be regarded simply as a model of behavior. More appropriately, it provides a tool which has been found useful in approaching both old and new problems in psychology; the theory has given psychology a model from which to develop psychological experiments. These have tended in large part to be designed around the method of absolute judgment. One of the basic problems has been to increase the amount of input information and to measure the amount of transmitted information, noting the increase in errors and the asymptotic value of transmission (channel capacity). Although information measures have been found useful, they should not be applied uncritically or mechanically. Information theory is derived from a specific set of assumptions; these involve the randomness of the variables under consideration and place constraints on the planned experimental design. In psychology, as in any science, only a judicious application of the available models, tools, and techniques will lead to a fuller understanding of the processes being studied.

INFORMATION AS A QUANTITATIVE CONCEPT Information theory provides a mathematical approach to problems of the "real" world; the solution of these problems requires that decisions or choices be made in the light of certain information or data available to an individual. In order to remove or reduce the uncertainty in a given situation, an individual needs to gain information by observing, directly or indirectly, those aspects of the world which have a bearing on that situation. If the observation tells the individual something he already knows, no information has been gained and the uncertainty has not been reduced. If, on the other hand, the observation provides some data not previously available, some information has been gained and uncertainty reduced. In general, those observations which convey information involve some sort of selection from a range of processes extending from extreme predictability, lawfulness, or redundancy to extreme unpredictability, disorder, or uncertainty. Whenever the selection is from a highly predictable process, little information is gained since the outcome is already known with a high degree of certainty. Whenever a highly inprobable event occurs, considerable information or knowledge is provided. No information can be transmitted, however, if the process is completely indeterminate. Information and uncertainty are, thus, related concepts and can be specified in similar terms of information measurement.

In a communications setting, information transmission requires that there be some "common language" established between the information source (transmitter) and the receiver, i.e., some agreement concerning the elements used in the communication process (Frick, 1959). This means that the selection (information) processes must be made from a specific set of alternatives and that the possible choices or alternatives must be known to the receiver, if the sequence of selections is to convey any information. This restriction on the set of alternatives forms the basis for the quantitative meaning of information or degrees of uncertainty which lie between extreme order ("complete knowledge") and chaos ("complete ignorance"). Added to this restriction is the notion that uncertainties involve probabilities. To arrive at any information measure for a given situation, both the number of choices available and their associated probabilities must be considered. Specific definition of the unit of information, the "bit," will be provided in the next section of this chapter. Given this particular measure, it is possible to establish the equivalences among messages ("coding theorem") and to determine the limit of correlation (association) which may obtain between messages ("channel theorem"). The measure of information is strictly analytic and objective; it has no surplus meaning related to message importance, truthfulness, exactness, value, or any other qualitative description.

Transmission. Granted that man can function as a link in a communications system, two questions are immediately suggested: (1) what is the maximum rate of information transmission, and (2) what is the greatest quantity of information that can be conveyed to an observer by means of a single stimulus presentation, unidimensional or multidimensional? The answers to these questions require the quantitative measure of information. The basic unit, the binary digit or "bit," is defined as the amount of information associated with a selection between two alternatives which are equally probable or likely. Such a selection involves maximal uncertainty, and the binary choice denotes the minimal condition under which any information can be transmitted. If a particular choice is to be made of N equally likely alternatives, the amount of information or the number of bits (H) required will be equal to the logarithm to the base 2 of N, i.e., $H = \log_2 N$ or $N = 2^H$. The number of bits of information is the power to which the base 2 must be raised to equal the number of equiprobable alternatives. (While it is customary to use logarithms to the base 2, the theory does not require this as a basic condition.) Stated differently, H is the amount of information in bits associated with the independent selection of one out of N equally probable alternatives and denotes the minimal number of binary digits required to uniquely represent each element of set N, i.e., optimal coding.

Since the N alternatives are equally likely, the probability of occurrence p of any alternative is equal to $1/N$ and N is equal to $1/p$, such that $H = \log_2 1/p = -\log_2 p$. Further consideration must be afforded this equation, however, if the H measure is to cover those situations in which the various alternatives have unequal probabilities. Letting h_i represent the informational value of a particular alternative i of n alternatives with unequal probabilities, $h_i = \log_2 1/p_i$. While h_i refers to the informational content of a particular alternative, H gives in a single measure the amount of information associated with all alternatives. To obtain the average value of H for a given situation involving unequal probabilities, one could take the mean of all the h's, but it should be remembered that the occurrence of an event with a high probability transmits very little information, whereas the occurrence of an event with a low probability transmits a great deal of information. This suggests that the H measure should be computed from the sum of the weighted h values, in which the weighting factor for each alternative is the probability of its occurrence. This provides n alternatives, each with its own p value and h value, such that $H = p_1h_1 + p_2h_2 \ldots + p_ih_i \ldots + p_nh_n$, or $H = \overset{i}{\Sigma} p_ih_i$. However, $h = \log 1/p$ and the equation for the average amount of information (uncertainty, entropy) may be written as $H = \overset{n}{\underset{i=1}{\Sigma}} p_i \log_2 1/p_i$ or:

$$H = -\sum_{i=1}^{n} p_i \log_2 p_i \qquad (18)$$

It can be shown that when all the values of p_i are equal, Eq. 18 reduces to $\log_2 1/p$ as given earlier. Also, for any given number of alternatives, the greatest possible value of H (the greatest uncertainty) occurs when all the alternatives are equiprobable, i.e., $H_{\max} = \log_2 N$. The smallest possible value of H is zero; this occurs when one alternative has a probability of "one" and all others have probabilities of zero. Relative uncertainty R, or the particular value of H in a given situation stated as a proportion of the maximum, is given by $R = H/H_{\max}$. This ratio provides a measure of the efficiency of the source in information transmission and forms the basis for the index of behavioral stereotypy (Miller and Frick, 1949). Redundancy C is a complementary quantity given by $C = 1 - R$; it measures the amount of unutilized possibilities to carry information in a given situation.

The quantitative concepts developed to this point can be readily applied to a psychological experiment. Consider a situation in which a subject is shown various stimuli in a class, ordered or unordered, and is instructed to identify each by a name or number whenever it is presented. For each stimulus there is one and only one correct response. In the course of the experiment each stimulus is presented a number of times and the experimenter records the particular response made to each stimulus. The problem is to ascertain how much of the information in the stimulus is actually transmitted to the subject and how much is lost.

The solution to the problem can be obtained in a number of different ways, but the basic notion is that the stimuli and responses may be considered as input to and output from a communication channel or as two sources of information. In addition, the possibility of "noise" or extraneous factors affecting the communication channel in an unpredictable manner must be considered; the effect of the "noise" is that the message at the destination (output) will seldom, if ever, be identical with the message generated by the source (input). Given a particular stimulus, there is some uncertainty about which response will occur (response equivocation) and, conversely, given a particular response, there is some uncertainty about which stimulus did occur (stimulus equivocation). Letting x and y represent the events in the input and output (the two sources), respectively, and letting $p(j)$ be the probability of $y = j$, then response equivocation is given by:

$$H_x(y) = -\sum_{i,j} p(ij) \log_2 p_i(j) \qquad (19)$$

and stimulus equivocation is given by:

$$H_y(x) = -\sum_{i,j} p(ij) \log_2 p_j(i) \qquad (20)$$

where $p(ij)$ is the probability of the joint occurrence of stimulus category i and response category j, $p_i(j)$ is the conditional probability of response j having occurred given stimulus i, and $p_j(i)$ is the conditional probability of occurrence of stimulus i, given response j.

Recalling that the amount of information in the stimuli $H(x)$, and the amount of information in the responses, $H(y)$, may be computed by using the basic Eq. 18, and defining the information measure for the joint distribution of stimuli and responses $H(x, y)$, as:

$$H(x, y) = -\sum_{i,j} p(ij) \log_2 p(ij) \qquad (21)$$

the general equation for the solution of the problem may be set up. Thus,

$$\begin{aligned} T = H(x) + H(y) - H(x, y) &= H(x) - H_y(x) \\ &= H(y) - H_x(y) \qquad (22) \end{aligned}$$

where T is the amount of transmitted information or the amount of information common to x and y. It can be seen from Eq. 22 that T may be computed from any one of the three equivalent expressions. Specific examples of the necessary computational steps in these solutions may be found in Garner and Hake (1951) and in Attneave (1959).

Although Eq. 22 refers to the transmission of information in the simple case of two sources, the basic concepts may be extended to deal with as many dimensions as experimentally or computationally feasible (McGill, 1954; Attneave, 1959). In the three-variable case, for instance, it is possible to calculate both the amount of information which any two variables (considered as one source) transmit to the third, and the amount of information which is unique to each variable, i.e., not related to either of the other two sources, as well as the amount of transmitted information in the system as a whole. T for the entire system is given by:

$$\begin{aligned} T(x, y, z) = H(x) + H(y) &+ H(z) \\ &- H(x, y, z) \qquad (23) \end{aligned}$$

which has the same underlying rationale as that for Eq. 22.

Miller and Madow (1954) have investigated the problem of testing the significance of multivariate information transmission and have provided a test, which according to McGill (1954) applies to any particular T value computed. The test involves χ^2 and the relationship of χ^2 to T may be expressed as:

$$\chi^2 = 1.3863n\, T, \text{ approximately} \qquad (24)$$

where n is the number of observations on which T is based, and the degrees of freedom (*df*) for χ^2 are combined in exactly the same way as the information components with which they are associated in the computation of T. For details on combining *df*, see Attneave (1959). The null hypothesis which is tested by Eq. 24 is that the particular T, e.g., $T(x, y)$, is equal to zero. Tests of significance using χ^2 are also available for evaluating the various H's in the system (Attneave, 1959).

In the preceding discussion, no mention has been made of the fact that in the application of information measures to psychophysical data the true distributions of probabilities are unknown and are estimated from sample data. Consequently, any uncertainty statistic derived from the sample is a biased estimator and should be corrected for bias. On the average, the computed H will be less than the true H and the computed T will be larger than the true T. Miller and Madow (1954) have provided an approximate correction which is appropriate for both conditions:

$$H' \text{ (or } T') = H_{\text{obs}} \text{ (or } T_{\text{obs}}) - \frac{df}{1.3863n} \qquad (25)$$

in which H' and T' are unbiased estimates of the true values of H and T, respectively, H_{obs} and T_{obs} are the observed or computed estimates of H and T, respectively, n is the number of observations upon which the H_{obs} or T_{obs} is based, and *df* refers to the number of degrees of freedom corresponding to H_{obs} or T_{obs}. Equation 25 indicates simply that the corrected value of the sample uncertainty statistic may be obtained by adding to the sample statistic the quantity $K-1/1.3863n$, where K is the number of possible alternatives and n is the number of occurrences of the given event. The significance test for the transmitted information measure (Eq. 24) may be appropriately applied only after the correction for bias has been made.

Sequential Analysis. In the case of information transmission as presented in the preceding section, the assumption is made that the successive occurrences of events in a given situation are independent. This assumption is at best only a first approximation when dealing with behavioral processes. Consideration needs to be given, therefore, to those sequences of choices which exhibit internal dependencies, i.e., those sequences in which a given choice is dependent upon or affected by the immediately preceding choice or choices.

The communication model applicable to this case is one in which the organism is considered as the source which emits signals in the form of observable responses and the observing psychologist is considered as the destination or receiver whose task it is to analyze the chain of responses in terms of serial or sequential dependencies. To be able to perform a sequential analysis, the observing psychologist must record all the responses in a serial or temporal order. It is assumed that the stochastic process under consideration is ergodic; the probability laws which characterize the process remain constant for all portions of the sequence observed. The problem is to discover recurring patterns of response within a behavioral sequence and to determine how these patterns change as certain experimental variables are manipulated.

In the following development to differentiate the case of sequential or dependent selections of events from the earlier case of independent selections, the symbol U will be used instead of H to refer to the quantitative measure of uncertainty. The analysis of uncertainty U in the sequential case differs from that in the independent case in that for any given selection i, the uncertainty or information value will depend upon the conditional probabilities $p_i(j)$ rather than the unconditional probabilities $p(j)$. If a particular sequence of responses is considered as one state of a process, then there are several possible events (responses) which may follow. The certainty or uncertainty of the next response will depend upon the particular state of the system at that given moment. Thus, there is a different degree of uncertainty associated with each state. For a particular state, this uncertainty is expressed by:

$$U_i(y) = - \sum_{j=1}^{k} p_i(j) \log_2 p_i(j) \qquad (26)$$

where $U_i(y)$ is the amount of information associated with the selection made whenever the system is in the state of just having selected i, and the summation is over all possible j states that could occur.

Notice that Eq. 26 provides an expression for the uncertainty of any particular state i. This state may be the occurrence of a single event or the *occurrence of a specific sequence of events.* If Eq. 26 is solved for a single event (independence), the obtained value will be identical to that obtained by using Eq. 18, since the nonconditional probability $p_i(j) = p(j)$. With intersymbol (or response) dependencies, the uncertainty of a source may be reduced; the uncertainty as to which symbol will occur next will be less with knowledge of the symbol which has just occurred than without such knowledge. Similarly, the prediction of a symbol may be based on the preceding two, preceding three, preceding four, etc., symbols in the sequence. In general, $U(S)$ represents the average uncertainty per symbol associated with the source when the preceding S-1 events are known. As S increases without limit, $U(S)$ approaches U, the average uncertainty of the source, monotonically. Since $U(S)$ represents the uncertainty with which a response can be predicted when only the preceding S-1 responses are known, this value may decrease as S becomes larger; as the antecedent chain of symbols increases in length, knowledge is provided which may make the coming event more certain and thereby decrease $U(S)$.

For the S-th order approximation of U, the average uncertainty per event (sequence of symbols) is given by:

$$U(S) = - \sum_{ij}^{k} P_i p_i(j) \log_2 p_i(j) \qquad (27)$$

where P is a weighting factor assigned to each event or state of the system and equals the probability of occurrence of the i-th state. The rationale for weighting is similar to that involved in the development of Eq. 18 with independent events. The uncertainty of the whole process is, then, the weighted average of the uncertainties for each of the several states and represents the average uncertainty in the prediction of the next event when the preceding S-1 events are known.

The set of values of $U(S)$ for $S = 1, 2, 3 \ldots$, computed for a given source defines the uncertainty function of that source. If there are no intersymbol dependencies for a source, the uncertainty function will be essentially constant. If the source emits symbols with some regularity or pattern, the uncertainty function will decrease in magnitude at that value of S which corresponds to the length of the patterned sequence. The function will continue to decrease only so far as there are patterns of responses which become successively longer.

A hypothetical uncertainty function is shown in Fig. 7-6. The $U(S)$ estimates are plotted as a function of the order of approximation or estimation S, with the $U(S_0)$ value known a priori to be 1.00. The function drops sharply at $S = 2$ and remains essentially constant thereafter. This indicates an intersymbol dependency of length two; it does not indicate, however, the exact nature of this dependency, whether of the form RLRLRL . . . , RRLLRR . . . , RRRRLLLLRRRR, or any other form of digram repetition. Since the function fails to drop after $S = 2$, there is no evidence of patterned sequences greater than length two.

The uncertainty estimate $U(S)$ has been used by Miller and Frick (1949) in defining the index of behavioral stereotypy, $C(S)$:

$$C(S) = 1 - U(S)/U(S)_{\max} \qquad (28)$$

where $U(S)_{\max} = \log_2 n$ for any value of S, with n given by the number of different symbols the source can emit. This equivalence is derived from the notion that the maximum average uncertainty of the S-th order approximation is obtained when each symbol of the source is equally likely. For equiprobable events, $C(S)$ is equal to zero; for sequences which are completely patterned or stereotyped, $C(S)$ is equal to unity. Accordingly, $C(S)$ may vary between these two limits. For any value, v, of $C(S)$, v per cent of the symbols are completely determined and $(1 - v)\%$ are maximally uncertain. In other words, v per cent of the symbols are those which occur in a sequence but carry no information.

For specific examples of the methods of computation which are applicable to the analysis of sequences, see Miller and Frick (1949), Shannon (1951), Newman (1951), or Newman and Gerstman (1952). More recently, Edwards (1954) has suggested a procedure which may save some labor and has stressed that McGill's equation (1954) in which $N \log_2 N$ values are used to calculate uncertainties is more appropriate than solutions involving $p \log_2 p$ values, since it avoids the rapid accumulation of rounding errors arising from small values of p.

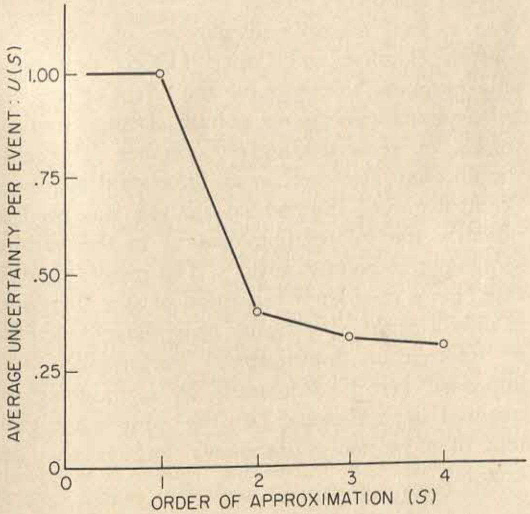


Fig. 7-6. A hypothetical uncertainty function showing $U(S)$ estimates as a function of the order of approximation (S). The *a priori* value of $U(S_0)$ is taken as 1.00.

SOME STUDIES ON INFORMATION PROCESSING IN MAN

Transmission. The application of the principles of information theory to situations in which man is regarded as a communication channel has led to considerable research in this area in recent years. In general, the trend has been to use informational analysis of absolute judgments obtained in specific experimental settings. The aim of these studies has been to estimate man's "channel capacity" or maximum information transmission under various conditions of stimulation. Since Miller (1956) and Alluisi (1957) have already presented reviews on this topic, only a few general comments will be made here.

Miller (1956) summarized a group of studies involving absolute judgments of points on a line (Hake and Garner, 1951), pitch (Pollack, 1952, 1953), loudness (Garner, 1953), hue (Eriksen, 1954), brightness (Eriksen, 1954), saltiness (Beebe-Centre et al., 1955), and size of squares (Eriksen and Hake, 1955). The maximum amount of transmitted information calculated in these studies ranged from 1.9 bits per signal for saltiness to 3.2 bits per signal for points on a line. This is equivalent, respectively, to perfect identification of about four to nine different stimuli of a given kind. The general conclusion from these studies seems to be that increasing the number of alternatives beyond the maximal number which can be perfectly identified does not increase the amount of information transmitted.

Several studies have been reported in which the absolute judgments were based on multidimensional, rather than unidimensional, cues. Halsey and Chapanis (1954), for example, found that the channel capacity for hue was about 3.1 bits per signal and for brightness, about 2.3 bits; when these two dimensions were simultaneously varied, however, channel capacity attained a value of only 3.6 bits, considerably less than the 5.4

bits expected if transmission were the sum of the two separate channel capacities. Pollack and Ficks (1954) working with six auditory dimensions and Klemmer and Frick (1953) working with eight spatial dimensions reached the same conclusion. The channel capacity for two or more dimensions is greater than any dimension considered alone, but it does not equal the sum of the separate capacities. In a study involving multidimensional cues related to absolute judgments of musical tonality, Corso (1957) found that the maximal amount of transmitted information in judging twenty-four keys was slightly more than one bit for a group of college musicians, although one subject with "perfect pitch" had perfect transmission (4.58 bits) in judging eight-tone diatonic scales.

Although transmitted information may be increased by increasing the range of stimuli, i.e., by increasing the separation between stimuli, the effect is rather small. Pollack (1952) found that in pitch judgments, a twenty-fold increase in the range of frequencies resulted in about a 10 per cent (.2 bit) increase in information transmission. Hartman (1954) and Alluisi and Sidorsky (1958) obtained data exhibiting similar effects.

In addition to channel capacity considered in terms of the average amount of information transmitted per signal, a number of studies have investigated the *rate* at which information is transmitted, e.g., the average amount of information transmitted per second. Oral reading of words approximating English text approaches a value of about 35 bits per second (Sumby and Pollack, 1954); the playing of random sequences of notes on a piano reaches about 22 bits per second and typing with 34 keys gives a maximum of about 15 bits per second (Quastler and Wulff, 1955). For silent reading, Quastler and Wulff estimate that the rate may reach 44 bits per second. It seems clear that the kind of stimulus, the kind of response, and their interrelations limit channel capacity. Such relations are referred to as *S-R* compatibility and have been shown to reflect the effects of practice (Deininger and Fitts, 1955).

References to numerous other studies on information transmission in man, including those on speed of coding, may be found in Quastler (1955), Broadbent (1958), and Attneave (1959).

Sequential Analysis. In a study to determine the effects of "meaningfulness" of verbal material on recall, Miller and Selfridge (1950) presented their subjects with samples of text which represented various orders of approximation to English. The orders of approximation or degrees of organization were derived from a sequential uncertainty technique and served to objectively define "meaningfulness." It was found that the number of words correctly recalled increased with the order of approximation of the material to English text.

Hake and Hyman (1953) studied the manner in which subjects perceived and responded to sequential dependencies built into a series of stimuli. Each series was derived from binary symbols with different probabilities of occurrence, and different conditional probability distributions characterized each series. The task of the subjects was to guess which symbol, prior to its presentation, would occur next in the series. By considering the subjects' previous guesses and the previous occurrences of symbols, conditional entropies were derived to measure the predictability of guesses. It was found that the highest predictability occurs when, for the two preceding trials, both the subjects' guesses and the actual occurrence of symbols are known. The same knowledge for only a single preceding trial, however, yields a prediction which is nearly as accurate; but, the prediction based on knowledge only of the occurrence of symbols in the two preceding trials is somewhat poorer. Thus, both the occurrence of symbols and the predictions made about these occurrences serve to affect a subject's guess.

As a final example in the use of sequential analysis, Senders and Cohen (1955) performed an experiment to determine the effect of sequential dependencies among pointer settings, used as stimuli, on scale reading performance. The same stimuli were also used in an unordered sequence (control group). Sequential analysis was used to quantify the information content in the various sequences of pointer settings. The results showed that "mere unordered repetition of stimuli results in only a slight, statistically insignificant, increase in information transmitted." Performance was improved (errors decreased) by introducing sequential dependencies, i.e., the information content of a sequence of pointer settings was decreased.

INFORMATION THEORY AND ITS RELATION TO GENERAL PROBLEMS OF BEHAVIOR

As a strictly deductive mathematical system, information theory provides a significant contribution to the understanding of situations centered around communication problems. The extent to which man behaves as an element in a communication system, however, is a matter of experimental fact, not logical conjecture. Nevertheless, the theory provides new measures of analysis and furnishes a framework within which new experiments may be conceptualized by the inquiring psychologist. The application of information concepts to behavioral situations demands a thorough understanding of the mathematical foundations underlying the development of the theory and is limited only by the experimental resourcefulness of the investigator.

Although most of the studies involving information theory in psychology have been concerned with channel capacity, amount of information transmitted, and similar concepts, some recent studies have tended to emphasize the effects of underloading or overloading the organism with input information. The work on sensory deprivation may be considered from the viewpoint that pathological functioning of the nervous system and abnormal behavior can result from the input underload of information. The studies by Heron, Doane, and Scott (1956), Riesen (1950), and others indicate that normal development and the continued maintenance of well-adjusted behavior depend upon the input of certain rates of sensory information. In contrast, Miller (1960), has reported preliminary studies involving the effects of information overload on different levels of behaving systems from the cell through the social institution. One of the aspects of the behaving system observed in these studies is the mechanism of defense or adjustment of the system to the stress of the informational input overload. It is suggested that the presence of defensive mechanisms at all levels of behaving systems, if experimentally established, may serve to "explain some of the psychopathology of everyday life and clinical practice." Shands (1959) has also directed attention to notions of information theory in relation to psychiatry.

Other applications of information theory related to general problems of behavior may be found in Battig (1959), who considered the question of multiple prediction; Shaw (1961), who investigated some factors influencing the use of information in small groups; Alluisi (1960), who studied the use of information measures in form perception; and Campbell (1958), who reviewed the literature to determine the bias tendencies of man as a link in communication systems.

Information theory offers the experimental, applied, and clinical psychologist a theoretical system and a set of measures which avoids the restrictions characteristic of the more traditional psychometric and statistical methods. The units of measurement may be used regardless of whether the variables are quantitatively or qualitatively described and do not demand that the variables be located along a single underlying continuum. The measures are not specific to the particular manipulations of a given study and thereby permit a ready comparison of the results derived from apparently diverse subject-matter areas. Furthermore, information measures provide a unique method of analysis for many problems in social and clinical psychology where sequences of responses, not individual responses, are required to explain behavior.

Two serious limitations in the application of uncertainty analysis to certain behavioral data must, nevertheless, be considered. These are: (*a*) the incompatibility of the assumption of a stationary time series and the effects of learning, and (*b*) the difficulty of obtaining adequate samples of behavior for estimating probabilities when the length of sequential dependencies increases in the observed chain of responses (Miller and Frick, 1949).

SENSORY FUNCTIONS AND UTILITIES: THE THEORY OF SIGNAL DETECTABILITY

While the consideration of man as a link in a communication system has aroused the interest of psychologists, the extension of this approach provides added stimulation when the processing of information is considered as part of a decision process. In a decision process, the informational content of a situation is available to the decision maker; the decision maker evaluates the situation according to a particular set of principles and, as a result of this evaluation, selects and executes a particular action. As in the analogous case of information transmission, noise effects or random processes may affect the sequence in any or all three of its components. Those psychologists interested in decision-making generally approach this problem by holding constant the informational aspects of stimulus input and limiting the number of possible responses, while varying the motivational factors in the "game." Those psychologists interested in the informational aspects of decision processes prescribe the "rules of the game" and vary the inputs to determine their effects on outputs. Although there are obvious advantages in studying each of these different stages in the decision process in experimental isolation, the combination of statistical decision theory and mathematical models into a single theory of behavior offers great promise, if carried out with care and an awareness of underlying assumptions. Such a combination is found in the theory of signal detectability.

THE SIGNAL-DETECTION EXPERIMENT

The theory of signal detectability may be considered as a special case of the theory of testing statistical hypotheses and statistical decision theory. Given a situation in which one must choose between two alternative statistical hypotheses on the basis of an observed event, the optimal behavior in that situation is specified by the theory of testing statistical hypotheses. If the observed events are arranged on a continuum, the theory specifies the optimal cut-off along that continuum in terms of (a) the a priori probabilities associated with the two hypotheses, (b) the amount of overlap of the statistical distributions constituting the two hypotheses, and (c) the values and costs which may be associated with the outcomes of the various decisions. According to the theory of signal detectability, the detection problem is basically a statistical decision.

The problem of signal detectability from an experimental viewpoint concerns the observation or detection of signals which are weak relative to their background of exposure. Detection, therefore, is a function of both signal intensity and the background in which the signal is presented. The modality of detection has no particular significance for the development of the theory. In the simplest experiment, a series of trials of fixed temporal duration is presented, in which for each trial either noise alone or signal plus noise occurs. The subject observes the events which occur during each trial, makes a decision based on this observation, and reports whether the trial consisted of noise alone or signal plus noise. Briefly, noise is always present, and the signal may or may not occur. The signal is usually presented at different intensity values in accordance with a predetermined experimental plan, and the observer knows that the signal, if present, will be selected from a specified ensemble of signals.

In any given observation interval there are four possible outcomes: the signal may or may not be present in the noise background and the observer may in either instance respond that it is or is not present. In reaching his decision, the observer must consider both the actual sensory input and the decision function to be maximized, e.g., the percentage of correct responses, the reduction in uncertainty as to whether a signal was or was not present, or the total expected value of the decisions. Other definitions of optimum strategy might also be used. If, for a given situation, the a priori probabilities of the various possible signal inputs are known, the decision function to be maximized is prescribed, and all the physical parameters of the signals and of the noise in the channel are specified, then it is possible to define for that situation an "ideal observer." The "ideal observer" is in effect a mathematical computation which indicates the maximal performance for that particular situation. By comparing the performance of the human observer with that of the "ideal observer," some insight is gained into the aspects of the situation which are actually being utilized by the human observer in reaching his decisions.

For those experiments in which the expected value of the decisions is to be maximized, the observer is paid a certain basic rate, e.g., one dollar per hour, during which he makes hundreds of observations. For each correct decision he receives a small amount, e.g., one tenth of a cent; for each incorrect decision he loses the same amount. The basic pay rate is sufficient for an extended game and, since the game may be made favorable to the observer by the appropriate selection of signals, the expected value becomes the desirable function to be optimized. Thus, it is possible to study the changes in the observer's decisions which are brought about by altering either the a priori probabilities of the signals or the values and costs of the decisions (pay-off matrix).

ESSENTIALS OF THE THEORY WITH THE EXPECTED VALUE CRITERION

As indicated in the description of the signal-detection experiment, the task of the subject is to make a decision based on his observation (x) of the events occurring in a fixed interval of time as to whether the interval contained only the background noise or included a predesignated signal. It is assumed that the observation may be represented as a continuous variable lying along a single dimension. It is further assumed that a given observation may arise either from noise alone or from signal plus noise, with specific probabilities. The distributions associated with these two conditions are usually designated as $f_{SN}(x)$ which represents the probability density function of x given the occurrence of signal plus noise and $f_N(x)$, which represents the probability density that x will result given the occurrence of noise alone. These density functions of x, or some monotonic transformation of the functions, are assumed to be normal distributions with equal (unit) variance (Peterson, Birdsall, and Fox, 1954). Given these density functions, the observation x is identified with or characterized by a particular likelihood ratio, $l(x)$, and the decision axis becomes the likelihood ratio. In this case

$$l(x) = \frac{f_{SN}(x)}{f_N(x)} \qquad (29)$$

in which the $l(x)$ expresses the likelihood (probability density) that the response of the sensory system (x) arose from SN (signal plus noise) relative to the likelihood that it arose from N.

According to the theory of signal detectability, the observer accomplishes his task of optimally mapping inputs into outputs by acting as though he computes the likelihood ratio associated with each sensory input and compares it with some number β. The rule for a decision is to report that the signal exists whenever $l(x) \geqq \beta$, otherwise to report that noise alone was present. Since the observation is defined in terms of likelihood ratio, the optimal criterion can always be specified by a given value β of $l(x)$. Birdsall (1954) has presented six ways of selecting β, depending on the particular purpose to be achieved by the detection process. Tanner (1960) has discussed the significance of the selection of β in terms of bringing the theory into agreement with the laboratory situation so that the theory might be used as an interpretive tool for psychophysical data.

A common procedure is to specify the optimal criterion β in terms of the maximization of the total expected value of a decision where the four possible outcomes of a decision in the signal-detection experiment have individual values associated with them. These four outcomes and their associated values (gain or loss) may be designated as: (a) $V_{N\cdot CA}$, the value of a correct negative report, i.e., saying that noise alone is present when there is no signal; (b) $V_{SN\cdot A}$, the value of correct positive report ("hit"), i.e., saying that the signal exists when the signal is present; (c) $K_{N\cdot A}$, the cost of an incorrect positive report ("false alarm"), i.e., saying the signal exists when noise alone is present; (d) $K_{SN\cdot CA}$, the cost of an incorrect negative report ("miss"), i.e., saying that noise alone exists when signal plus noise is present. Now, in statistical decision theory, the expected value of a decision (EV) is defined as the sum, over the potential outcomes of a decision, of the products of probability of outcome and the desirability of outcome. This gives, following generally Tanner, Birdsall, and Clark (1960),

$$EV = P(SN)V_{SN.A} + P(N)V_{N.CA} - P(SN)K_{SN.CA} - P(N)K_{N.A} \qquad (30)$$

where $P(SN)$ is the a priori probability that the signal exists, $P(N) = 1 - P(SN)$ is the a priori probability that noise alone exists, and the V and K terms have meanings as already defined. (The minus signs in Eq. 30 indicate costs or negative individual values for the last two terms.)

From Eq. 30 it follows directly that the condition for saying that signal plus noise is present is given by

$$P(SN)V_{SN.A} - P(N)K_{N.A} > P(N)V_{N.CA} - P(SN)K_{SN.CA} \qquad (31)$$

Equation 31 can be rewritten in the equivalent form:

$$\frac{P(SN)}{P(N)} > \frac{V_{N.CA} + K_{N.A}}{V_{SN.A} + K_{SN.CA}} \qquad (32)$$

It should be noted that Eq. 32 defines the condition for reporting the presence of a signal when only a priori probabilities form the basis for the decision. To take into account that decisions are based on *observations*, it is necessary to modify the left-hand member of the inequality by replacing the a priori probabilities with conditional probabilities $P_x(SN)$, the probability that the signal exists given the observation x, and $P_x(N)$, the probability that noise alone exists given the observation x. By imposing Bayes' theorem that

$$P_x(SN) = P(SN)P_{SN}(x)/P_{(x)}$$

$$\text{and} \quad P_x(N) = P(n)P_N(x)/Px$$

The left-hand member of Eq. 32 becomes

$$\frac{P_X(SN)}{P_X(N)} = \frac{P(SN)}{P(N)} \cdot \frac{P_{SN}(x)}{P_N(x)} \tag{33}$$

and the inequality (Eq. 32) may be rewritten as

$$P_{SN}(x)/P_N(x) > [P(N)/P(SN)](V_{N.CA} + K_{N.A}/V_{SN.A} + K_{SN.CA}) \tag{34}$$

Equation 34 indicates that whenever the likelihood ratio, $P_{SN}(x)/P_N(x)$, is greater than some value β, which is defined by the right-hand member of Eq. 34, the observer maximizing the expected value of his decisions must state that a signal is present in the background of noise. Observe also that the likelihood ratio in Eq. 34, $P_{SN}(x)/P_N(x)$, involves a development in which x is considered to be a discrete variable. According to an earlier stated assumption, x in the theory of signal detectability is a continuous variable with an infinite number of points. Thus, the likelihood ratio is best expressed in terms of probability density functions as in Eq. 29; the mathematical development, however, is similar in both cases.

The manner in which the concepts of the theory signal detectability developed to this point are applied to provide a statement of organismic capacity will now be described. Recall that there are only four possible outcomes to any given interval of observation. The probabilities of "hits" $[P_{SN}(A)]$ and "misses" will sum to one, as will the probabilities of "false alarms" $[P_N(A)]$ and correct negative reports. Hence, only $P_{SN}(A)$ and $P_N(A)$ are required to specify the observer's performance. Recall now that the two probability density functions were considered to lie on the same continuum, i.e., the decision axis, and that a criterion point β divided the continuum into two segments. All points to the right of β represent the criterion A (signal plus noise), while all points to the left of β represent the alternative hypothesis (noise alone). For a given criterion β, $P_{SN}(A) = \int_{x=\beta}^{\infty} f_{SN}(x)\,dx$ and $P_N(A) = \int_{x=\beta}^{\infty} f_N(x)\,dx$, i.e., the probabilities of "hits" and "false alarms" are given by the areas which lie to the right of β under the density functions of signal plus noise and noise alone, respectively. For a particular pair of distributions (signal plus noise and noise alone), $P_{SN}(A)$ and $P_N(A)$ are functions of the cut-off criterion β. These probabilities will change, however, depending upon the value of d', i.e., the difference between the means of the two distributions divided by their common standard deviation. For a given d', a graph can be constructed of $P_{SN}(A)$ versus $P_N(A)$ as the cut-off criterion β is moved along the decision axis. The curve connecting these points is called the receiver operating characteristic (ROC).

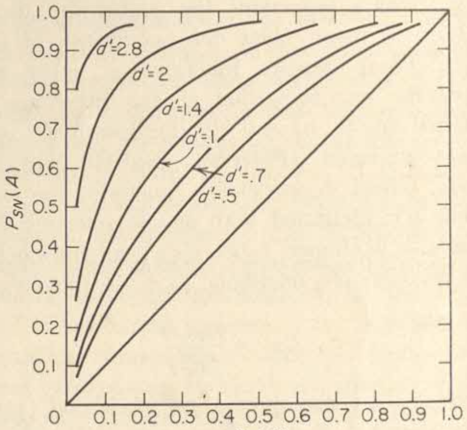


Fig. 7-7. Receiver operating characteristic (ROC) curves for the ideal observer, showing $P_{SN}(A)$ as a function of $P_N(A)$ with d' as the parameter. The curves are based on the assumptions that the probability density functions, $f_N(x)$ *and* $f_{SN}(x)$, are normal and of equal variance. Reproduced from Swets, Tanner, and Birdsall (1961, p. 309).

Figure 7-7 presents a set of ROC curves for the ideal observer for a series of experimental situations in which d' ranges from 0.5 to 2.8. No human observer or machine can produce data lying above a given curve, provided the expected value criterion is used to define optimal behavior. The diagonal line represents a criterion based on "random guessing." The optimal operating level is at the point of the ROC curve where the slope is equal to β. For the set of parameters characterizing a detection situation, this point specifies the appropriate "hit" probability and "false alarm" probability which will yield the maximal expected value for the experiment.

In a given experiment, the observer provides a pair of proportions as estimates of $P_{SN}(A)$ and $P_N(A)$ which are located on his ROC curve. The slope of the curve at this point yields the observer's value of the likelihood ratio at which he has established his criterion. Thus, the observer's criterion can be directly compared with that of the ideal observer. The cut-off criterion established by the observer is believed to reflect the effects of such variables as set, attitude, and motivation.

The second aspect of detection performance revealed by the analysis of signal detection theory is related to the observer's discriminatory ability. Given the two quantities $P_{SN}(A)$ and $P_N(A)$, a simple set of computations can be performed by referring to a published table of areas under the normal curve which will establish the distance of the criterion from the mean of the two density functions in x/σ units. For certain situations it has been shown that given an exactly specified waveform and a determinable starting time of the signal, $d'_{\text{optimum}} = (2E/N_0)^{1/2}$ where E is signal energy and N_0 is the power of white Gaussian noise in a one-cycle band (Peterson, Birdsall and Fox, 1954). Thus, the empirically determined index of detectability can be compared with ideal detectability. The square of the ratio of the real observer's index of detectability to that of the ideal observer specifies the real observer's efficiency η (Tanner and Birdsall, 1958).

The theory of signal detectability has been extended to include such problems as the recognition of patterns (Tanner, 1956), the intelligibility of aural and visual messages presented under difficult conditions of reception (Carterette and Cole, 1962), the detection of a noise signal presented in noise (Peterson, Birdsall, and Fox, 1954), the detection of speech in noise (Egan, 1957), and the detection of signals of uncertain frequencies (Creelman, 1960), but these extensions lie beyond the scope of the present summary.

SOME EXPERIMENTS RELATED TO THE THEORY OF SIGNAL DETECTABILITY

Since Swets, Tanner, and Birdsall (1961) have presented a comprehensive summary of a series of five experiments designed to test the adequacy of the theory of signal detectability, only the general nature and major findings of these studies will be presented in this section. Two experiments were performed to test the observer's ability to adopt the particular criterion which would maximize the expected value of a decision. In the first experiment, three observers judged visual signals at four levels of intensity, along with a "no-signal" presentation. The proportion of trials on which a signal of any intensity was presented, $P(SN)$, was either 0.80 or 0.40 in different sessions, with 300 total presentations per session. For some trials, no values were associated with the various decision outcomes; for other trials, various values were placed on the outcomes such that β ranged from 0.25 to 3.00. The results of the experiment showed that the obtained values of $P_N(A)$ varied in accordance with changes in the a priori probability of signal occurrence and that the criterion cut-off varied with changes in the optimal criterion, β. In the second experiment, only one value of signal intensity was employed and a greater number of observations was taken from which to estimate $P_{SN}(A)$ and $P_N(A)$. The $P(SN)$ remained constant at 0.50 and changes in the optimal criterion, β, were effected entirely by changing the values associated with the decision outcomes. The β values varied from 0.16 to 8.00. The results again were consistent with theoretical predictions; with the experimentally determined points providing a good fit to particular ROC curves.

A third experiment was performed to test the observer's ability to maximize the proportion of "hits" while at the same time placing a restriction on the proportion of false alarms. In this situation, no values and costs were involved. The a priori probability of signal occurrence was 0.72, with fourteen presentations of noise alone (0.28) in a block of fifty presentations. In each of four different experimental conditions, each extending over eighteen blocks of fifty presentations, the observers were instructed to adopt a cut-off criterion such that 0, 3, 6, or 9 "yes" responses would be made to the fourteen presentations of noise alone in each block of fifty. The correctness of responses was indicated only after a complete block of fifty presentations. The results indicated that the observers were able to adjust their criterion with considerable precision as required by the conditions of the experiment.

Two additional experiments were performed in which a different procedure was employed. In the first of these experiments, instead of establishing a criterion as a basis for responding "yes" and "no," the observer was asked to report after each observation interval his subjective probability that a signal was present during that interval. The response expressed a rating or confidence judgment of the observer. The observers were presented with a visual signal (0.78 ft-L) and the a priori probabilities $P(SN) = P(N) = 0.50$

for a total of 1200 observations. The task was to place each observation (signal plus noise or noise alone) in one of six categories of a posteriori probability. In the range between 0.2 and 1.0 there were four categories of equal size (0.2); the other two categories were given as 0.0 to .04 and .05 to .19. The results indicated that the six categories were used properly; as the probabilities which defined the categories increased, there was an increase in the proportion of total observations placed in each category which were actually observations of a signal. Furthermore, the ROC curves derived from these data were well described by the type of ROC curve predicted from the theory.

In the second of these experiments a temporal forced-choice method of response was employed. Given a trial in which a signal is presented in one of n temporal intervals, the observer is required to state the interval in which he believes the signal occurred. If the observer is following the optimal procedure in arriving at his decisions, the detectability of a signal of a given intensity should be the same under this procedure as that based on yes-no data. The signal consisted of a circular light with a diameter of 30 min of visual angle and a duration of .01 sec. The signal appeared on a large, uniformly illuminated background (10 ft-L). To predict performance in a forced-choice test, an extension of the decision model is required. With this extension, Tanner and Swets (1954) have shown that the estimates of d' for the situation described and for the expected-value experiment are highly consistent with each other. Swets (1959) has also shown that estimates of d' in auditory experiments are comparable for yes-no and forced-choice procedures, and from forced-choice procedures using from two to eight alternatives. Thus, by providing a dual approach, the theory of signal detectability permits the prediction of visual or auditory performance from one procedural situation to another.

COMMENTS ON THE ADVANTAGES OF THE THEORY OF SIGNAL DETECTABILITY

The theory of signal detectability has two major advantages over the classical psychophysical approach in dealing with organismic sensitivity. Conventionally, the measure of sensitivity has been the reciprocal of the absolute or differential threshold; the threshold approach, however, does not provide an analytical procedure for the independent specification of the criterion adopted by the subject in making his judgments. In the classical approach, while it recognized that the threshold value is affected by the stringency of the criterion, no effort is usually made to separate these two factors except, perhaps, for employing a guessing correction. The theory of signal detection avoids this difficulty by providing a quantitative measure of the cut-off criterion and, in addition, yields a separate measure of sensitivity.

The second advantage of the theory is that it provides the "ideal observer" as a standard of comparison. The availability of this "observer" permits the evaluation of the effectiveness of the behavior of the human observer and aids in the interpretation of experimental results. Since the theory of signal detectability is essentially one of testing statistical hypotheses, the theory offers a potentially useful tool for investigating personal and social processes in perception, as well as for studying the more classical problems of sensory discrimination.

SUMMARY

The organizational structure of the present chapter represents a wide departure from the usual expectation of a chapter dealing with sensory processes. The conventional approach in this area has been to review, in turn, each of the various sensory modalities delineating various theories of physiological mechanisms and presenting numerous experimental studies, the results of which bear directly on the particular theories under consideration, either favorably or unfavorably. In the present chapter, this approach has been discarded to permit the presentation of systematic developments which are sufficiently comprehensive to encompass psychological phenomena ranging across all modalities.

The initial portion of this chapter presented a brief sketch of the historical antecedents leading to sensory processes as an integral part of contemporary psychology and stressed the notions of variables, continua, and functional relations as applied to behavioral problems. Attention was directed to the conceptual distinction between sensation and perception, and primary emphasis was placed on the notion that a scientific psychology seeks to establish the characteristics of relationships among variables, one type of which is of the form $R = f(S)$. Problems related to the specification of *S* and *R* were pointed out, both in terms of their measurement characteristics and the need for specifying variables compatible with extant knowledge in other disciplines, such as neurophysiology.

After reviewing the general form of a psychophysical experiment, three theoretical explanations of sensory discrimination were presented: the classical theory, the theory of the neural quantum, and the theory of adaptation level (AL). The classical and neural quantum theories seek to establish invariant relationships involving the probability of response and single parameters of a particular stimulus or classes of stimuli, whereas AL theory, while dealing with the properties of stimuli, is also concerned with the behavioral effects of other independent empirical variables. These include background exposure factors and residuals from past experience. Although some studies have been reported which support the neural quantum theory, the classical theory still remains defensible for psychophysical threshold functions and certain classes of experiments on scaling. It would appear, however, that since AL theory is considerably more general than either the classical or quantum theory, significant progress might be made by applying the AL paradigm to psychophysical situations in which learning, motivational, cognitive, and other variables could be introduced. Such an approach, as well as that designated as the *S-R* model for sensory problems, would serve to minimize the artificial boundaries which have been fostered in contemporary psychology by the arbitrary attachment of classificatory titles to psychological problems, e.g., learning, perception, and motivation.

In treating the *S-R* model, emphasis was placed on the empirical variables of stimuli, instructions, and stimulus presentations; the behavioristic viewpoint was adopted with publicly observable events, not experiential events or terminology, forming the basic groundwork of psychophysics. The applicability of the *S-R* model was shown with respect to vision, audition, and gustation by reference to specific studies.

Information theory was developed in terms of two cases: transmission and sequential analysis, with a large number of studies cited in each case for human performance. If man is considered to perform the functions of a communication system, he must obey the laws which govern all such systems with respect to certain factors, such as the amount or rate of information which can be processed under a given set of conditions. The concepts and measures of the theory were presented in detail, and it was shown that the theory provides a quantitative technique for specifying the stimulus material and measuring the performance of the subjects. This applies regardless of the type of experiment under consideration and, accordingly, permits intermodality and inter-subject-matter comparisons. Although the transmission case has been relatively widely studied by psychologists, including underloading and overloading the stimulus environment, sequential analysis remains essentially unexplored. It is suggested that this technique might be profitably employed to study such situations as perceptual-motor behavior, industrial manipulations and operations, group structure, conflict behavior, and psychological tests. Information theory avoids the restrictions characteristic of the traditional psychometric and statistical methods; consequently, it can provide a desirable integrative link in the study of behavioral problems.

In the final section of this chapter, the theory of signal detectability was presented, in which sensory functions and utilities were combined into a single formulation. The essentials of the theory were given, together with a description of a series of experiments which tested and supported the adequacy of the theory. In contrast with the classical theory, the theory of signal detectability provides two quantitative measures: one specifies the criterion adopted by the observer in making his judgments, and the other denotes the observer's sensitivity in signal detection. In addition, the observer's performance can be compared with a mathematical ideal. Although the theory offers a potentially useful tool for investigating personal and social processes in perception, most research to date has been conducted on the evaluation of the theory in terms of classical problems of sensory discrimination.

In general, it may be concluded that the study of sensory processes in contemporary psychology is continuing to develop in its mathematical tradition, but with a broadened outlook. In addition to probability concepts, current formulations include values, past experiences, sequential dependencies, and other factors which provide the sensory psychologist with appropriate tools for studying discriminative behavior as a multivariate problem.

REFERENCES

ALLUISI, E. A. Conditions affecting the amount of information in absolute judgments. *Psychol. Rev.*, 1957, **64**, 97-103.

———. On the use of information measures in studies of form perception. *Percept. mot. Skills*, 1960, **11**, 195-203.

———, AND SIDORSKY, R. C. The empirical validity of equal discriminability scaling. *J. exp. Psychol.*, 1958, **55**, 86-95.

ATTNEAVE, F. *Applications of Information Theory to Psychology*. New York: Holt, Rinehart, and Winston, Inc., 1959.

———. Perception and related areas, in S. Koch, ed., *Psychology: A Study of a Science*, Vol. 4, 619-659. New York: McGraw-Hill Book Company, Inc., 1962.

BARLOW, H. B. Comment on neural quanta, in W. A. Rosenblith, ed., *Sensory Communication*. New York: John Wiley & Sons, Inc., 1961, 786-790.

BATTIG, A. W. An application of information theory to a problem of multiple prediction. *Psychol. Newsletter*, 1959, **10**, 148-150.

BEEBE-CENTRE, J. G., ROGERS, M. S., AND O'CONNELL, D. N. Transmission of information about sucrose and saline solutions through the sense of taste. *J. Psychol.*, 1955, **39**, 157-160.

BÉKÉSY, G. V. Über das Fechner'sche Gesetz und seine Bedeutung für die Theorie der Akustischen Beobachtungsfehler und die Theorie des Hörens. *Ann. Physik*, 1930, **7**, 329-359.

BERKSON, J. Application of the logistic function to bioassay. *J. Amer. Stat. Assoc.*, 1944, **39**, 357-365.

BIRDSALL, T. G. The theory of signal detectability, in H. Quastler, ed., *Information Theory in Psychology*. Glencoe, Ill.: Free Press, 1955.

BISHOP, G. H. Natural history of the nerve impulse. *Physiol. Rev.*, 1956, **36**, 376-399.

BLACKWELL, H. R. Evaluation of the neural quantum theory in vision. *Amer. J. Psychol.*, 1953, **66**, 397-408.

BORING, E. G. A chart of the psychometric function. *Amer. J. Psychol.*, 1917, **28**, 465-470.

BOUMAN, M. A., AND WALRAVEN, P. L. Some color naming experiments for red and green monochromatic lights. *J. opt. Soc. Amer.*, 1957, **47**, 834-839.

BROADBENT, D. E. *Perception and Communication*. London: Pergamon Press, 1958.

CAMPBELL, D. T. Systematic error on the part of human links in communication systems. *Information and Control*, 1958, **1**, 334-369.

CARNAP, R. *Der Logische Aufbau der Welt*. Berlin: Schlachtensee, Weltkleis Verlag, 1928.

CARTERETTE, E. C., AND COLE, M. Comparison of the receiver-operating characteristics for messages received by ear and by eye. *J. acoust. Soc. Amer.*, 1962, **34**, 172-178.

COLLIER, G., AND SISKEL, M., JR. Performance as a joint function of amount of reinforcement and inter-reinforcement interval. *J. exp. Psychol.*, 1959, **57**, 115-120.

CORSO, J. F. The neural quantum theory of sensory discrimination. *Psychol. Bull.*, 1956, **53**, 371-393.

———. Absolute judgments of musical tonality. *J. acoust. Soc. Amer.*, 1957, **29**, 138-144.

———. The quantal hypothesis and the threshold of audibility. *Amer. J. Psychol.*, 1961, **74**, 191-204.

CREELMAN, C. D. Detection of signals of uncertain frequency. *J. acoust. Soc. Amer.*, 1960, **32**, 805-810.

DEININGER, R. L., AND FITTS, P. M. Stimulus-response compatibility, information theory, and perceptual-motor performance. In H. Quastler (ed.), *Information Theory in Psychology*. Glencoe, Ill.: Free Press, 1955.

EDWARDS, W. Methods for computing uncertainties. *Amer. J. Psychol.*, 1954, **67**, 164-170.

EGAN, J. P. Message repetition, operating characteristics and confusion matrices in speech communication. *AFCRC-TR*-57-50, Bloomington, Ind., Indiana University, 1957.

EKMAN, G. Weber's law and related functions. *J. Psychol.*, 1959, **47**, 343-352.

ERIKSEN, C. W. Multi-dimensional stimulus differences and accuracy of discrimination. *USAF: WADC Tech. Report* 54-165, 1954.

———, AND HAKE, H. W. Absolute judgments as a function of the stimulus range and the number of stimulus and response categories. *J. exp. Psychol.*, 1955, **49**, 323-332.

FECHNER, G. T. *Elemente der Psychophysik*. Leipzig: Breitkopf und Hartel, 1860.

FLYNN, B. Pitch discrimination. *Arch. Psychol.*, 1943, **280**, 3-41.

FRICK, F. C. Information theory, in S. Koch, ed., *Psychology: A Study of a Science*, Vol. 2, 611-636. New York: McGraw-Hill Book Company, Inc., 1959.

FULLERTON, G. S., AND CATTELL, J. M. On the perception of small differences. *Univ. Penn. Philos. Series*, No. 2, 1892.

GARNER, W. R. An informational analysis of absolute judgments of loudness. *J. exp. Psychol.*, 1953, **46**, 373-380.

———. Context effects and the validity of loudness scales. *J. exp. Psychol.*, 1954, **48**, 218-244.

———, AND HAKE, H. W. The amount of information in absolute judgments. *Psychol. Rev.*, 1951, **58**, 446-459.

GIBSON, J. J. The concept of stimulus in psychology. *Amer. Psychologist*, 1960, **15**, 694-703.

GRAHAM, C. H. Behavior, perception and the psychophysical methods. *Psychol. Rev.*, 1950, **57**, 108-120.

———. Behavior and the psychophysical methods: an analysis of some recent experiments. *Psychol. Rev.*, 1952, **59**, 62-70.

———. Sensation and perception in an objective psychology. *Psychol. Rev.*, 1958, **65**, 65-76.

GUILFORD, J. P. *Psychometric Methods.* (2d ed.). New York: McGraw-Hill Book Company, Inc., 1954.

HAKE, H. W., AND GARNER, W. R. The effect of presenting various numbers of discrete stages on scale reading accuracy. *J. exp. Psychol.*, 1951, **42**, 358-366.

———, AND HYMAN, R. Perception of the statistical structure of a random series of binary symbols. *J. exp. Psychol.*, 1953, **45**, 64-74.

HALSEY, R. H. AND CHAPANIS, A. Chromaticity-confusion contours in a complex receiving situation. *J. opt. Soc. Amer.*, 1954, **44**, 442-454.

HARTMAN, E. B. The influence of practice and pitch-distance between tones on the absolute identification of pitch. *Amer. J. Psychol.*, 1954, **67**, 1-14.

HEBB, D. O. *A Textbook of Psychology.* Philadelphia, Pa.: W. B. Saunders Company, 1958.

HELSON, H. Adaptation-level as frame of reference for prediction of psychophysical data. *Amer. J. Psychol.*, 1947, **60**, 1-29.

———. Adaptation-level as a basis for a quantitative theory of frames of reference. *Psychol. Rev.*, 1948, **55**, 297-313.

———. Adaptation Level Theory, in S. Koch, ed., *Psychology: A Study of a Science*, Vol. 1, 565-621. New York: McGraw-Hill Book Company, Inc., 1959.

———, DWORKIN, R. S., AND MICHELS, W. C. Quantitative denotations of common terms as a function of background. *Amer. J. Psychol.*, 1956, **69**, 194-208.

HERON, W., DOANE, B. K. AND SCOTT, T. H. Visual disturbances after prolonged perceptual isolation. *Canad. J. Psychol.*, 1956, **10**, 13-18.

HIGH, W. S., GLORIG, A., AND NIXON, J. Estimating the reliability of auditory threshold measurements. *J. aud. Res.*, 1961, **1**, 247-262.

JAMES, W. *The Principles of Psychology.* New York: Henry Holt and Company, Inc., 1890.

KLEMMER, E. T., AND FRICK, F. C. Assimilation of information from dot and matrix patterns. *J. exp. Psychol.*, 1953, **45**, 15-19.

LEWIS, D., AND BURKE, C. J. The use and the misuse of the chi-square test. *Psychol. Bull.*, 1949, **46**, 433-489.

MACCORQUODALE, K., AND MEEHL, P. E. On a distinction between hypothetical constructs and intervening variables. *Psychol. Rev.*, 1948, **55**, 95-107.

MCGILL, W. J. Multivariate information transmission. *Psychometrika*, 1954, **19**, 97-116.

MEIER, G. W., FOSHEE, D. P., WITTRIG, J. J., PEELAR, D. F., AND HUFF, F. W. Helson's residual factor versus innate S-R relations. *Psychol. Rep.*, 1960, **6**, 61-62.

MICHELS, W. C. An interpretation of the bril scale of subjective brightness. *J. opt. Soc. Amer.*, 1954, **44**, 70-74.

———, AND DOSER, B. T. Rating scale method for comparative loudness measurements. *J. acoust. Soc. Amer.*, 1955, **27**, 1173-1180.

———, AND HELSON, H. A reformulation of the Fechner law in terms of adaptation-level applied to rating-scale data. *Amer. J. Psychol.*, 1949, **62**, 355-368.

———, AND ———. A reconciliation of the veg scale with Fechner's law. *Amer. J. Psychol.*, 1954, **67**, 677-683.

MILLER, G. A. The magic number seven, plus or minus two: some limits on our capacity for processing information. *Psychol. Rev.*, 1956, **63**, 81-97.

———, AND FRICK, F. C. Statistical behavioristics and sequences of responses. *Psychol. Rev.*, 1949, **56**, 311-324.

———, AND GARNER, W. R. Effect of random presentation on the psychometric function: implications for a quantal theory of discrimination. *Amer. J. Psychol.*, 1944, **57**, 451-467.

———, AND MADOW, W. G. On the maximum likelihood estimate of the Shannon-Wiener measure of information. *Air Force Cambridge Research Center: Tech. Report*, 54-75, Aug. 1954.

———, AND SELFRIDGE, J. A. Verbal context and the recall of meaningful material. *Amer. J. Psychol.*, 1950, **63**, 176-185.

MILLER, J. G. Information input overload and psychopathology. *Amer. J. Psychiatry*, 1960, **116**, 695-704.

MORIKIYO, Y. Time-order error in the successive comparison of tones—an examination of adaptation-level theory. *Jap. J. Psychol.*, 1959, **30**, 198-207.

MÜLLER, G. E. *Zur Grundlegung der Psychophysik.* Berlin: Th. Grieben, 1878.

NEWMAN, E. B. Computational methods useful in analyzing series of binary data. *Amer. J. Psychol.*, 1951, **64**, 252-262.

———, AND GERSTMAN, L. S. A new method for analyzing printed English. *J. exp. Psychol.*, 1952, **44**, 114-125.

PARDUCCI, A., CALFEE, R. C., MARSHALL, L. M., AND DAVIDSON, L. P. Context effects in judgment: adaptation level as a function of the mean, midpoint, and median of the stimuli. *J. exp. Psychol.*, 1960, **60**, 65-77.

———, AND MARSHALL, L. M. Supplementary report: the effects of the mean, midpoint, and median upon adaptation level in judgment. *J. exp. Psychol.*, 1961*a*, **61**, 261-262.

———, AND ———. Control-effects in judgments of length. *Amer. J. Psychol.*, 1961*b*, **74**, 576-583.

PETERSON, W. W., BIRDSALL, T. G., AND FOX, W. C. The theory of signal detectability. *Trans. Professional Group on Information Theory*, PGIT-4, 1954, 171-212.

PFAFFMANN, C. Sensory processes and their relation to behavior: studies on the sense of taste as a model *S-R* system. In S.Koch, ed., *Psychology: A Study of a Science*, Vol. 4, 380-416. New York: McGraw-Hill Book Company, Inc., 1962.

PHILIP, B. R. The frame of reference concept. *Canad. J. Psychol.*, 1949, **3**, 73-79.

POLLACK, I. The information of elementary auditory displays. *J. acoust. Soc. Amer.*, 1952, **24**, 745-749.

———. The information of elementary auditory displays, II. *J. acoust. Soc. Amer.*, 1953, **25**, 765-769.

———, AND FICKS, L. Information of elementary multidimensional displays. *J. acoust. Soc. Amer.*, 1954, **26**, 155-158.

QUASTLER, H. (ed.). *Information Theory in Psychology*. Glencoe, Ill.: Free Press, 1955.

———, AND WULFF, V. J. Human performance in information transmission. *Univ. Illinois Control Systems Lab. Rep.*, No. 62, 1955.

RIESEN, A. H. The development of visual perception in man and chimpanzee. *Science*, 1947, **106**, 107-108.

RIESZ, R. R. Differential intensity sensitivity of the ear for pure tones. *Phys. Rev.*, 1928, **31**, 867-875.

ROSENBAUM, M., AND BLAKE, R. R. Volunteering as a function of field structure. *J. abnorm. soc. Psychol.*, 1955, **50**, 193-196.

SENDERS, V. L. AND COHEN, J. The effects of sequential dependencies on instrument reading performance. In H. Quastler, ed., *Information Theory in Psychology*. Glencoe, Ill.: Free Press, 1955.

SHANDS, H. C. Adaptation and information in psychiatry. *J. nerv. men. Disease*, 1959, **128**, 204-213.

SHANNON, C. E. Prediction and entropy in printed English. *Bell syst. tech. J.*, 1951, **30**, 50-64.

SHAW, M. E. Random versus systematic distribution of information in communication nets. *J. Personality*. 1956, **25**, 59-69.

SMITH, M., AND DUFFY, M. Evidence for a dual reinforcing effect of sugar. *J. comp. physiol. Psychol.*, 1957, **50**, 242-247.

SPENCE, K. W. The postulates and methods of 'Behaviorism.' *Psychol. Rev.*, 1948, **55**, 67-78.

STEVENS, S. S. Sensation and psychological measurement. Chap. 11 in E. G. Boring, H. S. Langfeld, and H. P. Weld., eds., *Foundations of Psychology*. New York: John Wiley & Sons, Inc., 1948.

———. Review of L. L. Thurstone, *The Measurement of Values. Contemp. Psychol.*, 1959, **4**, 388-389.

———. The psychophysics of sensory function. Chap. 1 in W. A. Rosenblith, ed., *Sensory Communication*. New York: John Wiley & Sons, Inc., 1961.

———, MORGAN, C. T., AND VOLKMANN, J. Theory of the neural quantum in the discrimination of loudness and pitch. *Amer. J. Psychol.*, 1941, **54**, 315-335.

SUMBY, W. H., AND POLLACK, I. I. Short-time processing of information. *HFORL Rept.*, TR-54-6, Jan. 1954.

SWETS, J. A. Indices of signal detectability obtained with various psychophysical procedures. *J. acoust. Soc. Amer.*, 1959, **31**, 511-513.

———, TANNER, W. P., JR., AND BIRDSALL, T. G. Decision processes in perception. *Psychol. Rev.*, 1961, **68**, 301-340.

TANNER, W. P., JR. Theory of recognition. *J. acoust. Soc. Amer.*, 1956, **28**, 882-888.

———. Theory of signal detectability as an interpretive tool for psychophysical data. *J. acoust. Soc. Amer.*, 1960, **32**, 1140-1147.

———, AND BIRDSALL, T. G. Definitions of d' and η as psychophysical measures. *J. acoust. Soc. Amer.*, 1958, **30**, 922-928.

———, ———, AND CLARKE, F. R. The concept of the ideal observer in psychophysics. *Tech. Rept. No. 98, Electronic Defense Group*, The University of Michigan, Ann Arbor, 1960.

———, AND SWETS, J. A. A decision-making theory of visual detection. *Psychol. Rev.*, 1954, **61**, 401-409.

THURSTONE, L. L. A law of comparative judgment. *Psychol. Rev.*, 1927, **34**, 273-286.

———. *The Measurement of Values*. Chicago: University of Chicago Press, 1959.

TORGERSON, W. S. *Theory and Methods of Scaling*. New York: John Wiley & Sons, Inc., 1958.

TURCHIOE, R. M. The relation of adjacent inhibitory stimuli to the central tendency effect. *J. gen. Psychol.*, 1948, **39**, 3-14.

WEISS, W. AND TEN EYCK, P. H. Effects on judgments of changes in the response scale. *Psychol. Rep.*, 1960, 7, 487-495.

———, AND ———. Effect of shock on size judgments. *Psychol. Rep.*, 1961, 9, 135-144.

ZENER, K., AND GAFFRON, M. Perceptual experience: an analysis of its relations to the external world through internal processings, in S. Koch, ed., *Psychology: A Study of a Science*, Vol. 4, 515-618. New York: McGraw-Hill Book Company, Inc., 1962.

CHAPTER 8

Perception[1]

HARRY HELSON
Kansas State University

Since the time of Locke (1690) it has been believed that perception has a double aspect, in which deliverances of the senses (sensations, dimensions, attributes) combine with central components (images, past experiences, cognitions) to yield percepts giving information about the properties and relations of objects. Perception has thus been regarded as having both outer and inner sources, the former responsible for our knowledge of the external world, the latter for individual differences in reactions to objects. Stemming directly from Locke is the notion that the organism (or mind, as he put it) is passive in its reception of sensory data, perceiving whatever comes to it. Sensory qualities are simple and irreducible; some, like colors, sounds, odors, tastes, and pressure, come through one sense only, and others, like space, extension, figure, rest, and motion, come through two or more senses. According to Locke the qualities that resemble objects in the external world, solidity, extension, figure, and mobility are mediated by more than one sense and are called "primary qualities," while the others have no reality, their qualities arising merely from the "powers (of objects) to produce various sensations in us." The latter are called "secondary qualities" of which color, sound, and taste are examples. Although the mind at birth is a *tabula rasa* it does have the power of compounding passively received simple ideas. The four types of active, compounding operations are distinction, comparison, abstraction, and combination.

Locke began with the intention of showing that all knowledge is derived from experience, or the simple deliverances of the senses, but he wound up with complicated compounding mechanisms requiring a number of mental activities to account for knowledge and understanding. Locke's theories of mind and perception were the progenitors of later atomistic accounts given by James Mill and others. Complex ideas, according to Locke, merely combine simple ideas, e.g., substance + hardness + weight + ductility + fusibility = idea of lead! Pure empiricism is usually found linked with genetic-atomistic approaches to problems, and the *reductio ad absurdum* of such approaches is found in Locke's derivation of the meaning of infinity, which supposedly is built up by thinking of something large, then something larger, then something still larger and larger, until finally one arrives at the notion of infinity!

While Lockian empiricism furnished a partial antidote to seventeenth- and eighteenth-century rationalism, which postulated innate ethical, religious, and mathematical truths, it led to Berkeley's subjective idealism, wherein the external

---

[1] Preparation of this chapter was supported in part by the Office of Naval Research under Contract Nonr-3634 (01) with Kansas State University.

world was reduced to complexes of ideas having order only because of "divine intelligence." Carrying the radical empiricist argument to its logical conclusion led finally to Hume's uncaused "impressions" or "self-bundle of perceptions in movement" which Huxley characterized as follows:

> For any demonstration that can be given to the contrary effect, the 'collection of presentations which makes up our consciousness' [according to Hume] may be an orderly phantasmagoria generated by the Ego, unfolding its successive scenes on the background of the abyss of nothingness; as a firework which is but cunningly arranged combustibles, grows from a spark into a corruscation, and from a corruscation into figures, and words, and cascades of devouring fire, and then vanishes into the darkness of the night (Huxley, 1901).

Neither the British empiricists, the British associationists, nor the pre-Gestalt psychologists who leaned heavily on Lockian principles, were able to give a satisfactory account of the meaning, order, and patterning found in perceptual processes. The doctrine of compounding, which at first sight seems to offer the most economical conceptual frame of reference for handling the complicated facts of sensation, perception, and mental life in general, proves to be the least economical in the long run.

In contrast with the gross analyses of the philosophers and their psychological successors, the associationists, stands the analytical, introspective school founded by Wundt in Germany. Talk of objects on the one hand and ideas, impressions, and their associations on the other, was replaced in dealing with perceptual problems by careful correlations between controlled stimulation and, where possible, specific endorgans, and sensory processes. While Bishop Berkeley recognized the problem of perceiving depth and distance from flat retinal images, and Bain stressed the importance of the muscle sense or kinesthesis (cf. Boring, 1942), it was the structural school of psychology which made the detailed study of stimulus-sensory correlations the main program of an experimentally oriented young science. Psychology as science began with the measurement and analysis of perceptual processes, whether we date its beginnings in the work of Weber (1824), Fechner (1869), or the founding of the psychology laboratory at Leipzig by Wundt (1879).

Because receptors are tiny endorgans or nerve fibers, the adequate stimuli for sensations were defined in terms of energies emanating from objects and capable of exciting specific endorgans connected by afferent nerves to the central nervous system. Thus light rays stimulating the rods and cones are the adequate stimuli for everything perceived visually; mechanical stimuli resulting in deformation of the skin produce contact, light pressure, deep pressure, and pain; and sound waves impinging on the ear drum eventually stimulate the basilar membrane and the fibers of the organ of Corti to arouse the auditory nerves and their central loci to make us aware of sounds. According to this first, older view, stimuli are narrowly and strictly defined: only the energies involved in stimulation are of moment, not the information they convey or their organization; receptors act more or less as independent units, no matter how closely they are packed together and regardless of branching and interlacing of afferent neurons; and sensory processes are real only to the extent that they stand up under pointlike stimulation and analytical introspection. Sensations on this basis reduce to their attributes—hue, brightness, and saturation in vision; pitch, volume, and loudness in audition; and so on in the different sense modalities.

Analytical introspection brought order and simplification into Hume's unordered "self bundle of perceptions" which seemed, as Huxley pointed out, to arise out of nowhere and to disappear into nothingness. Order was explained by reference to the attributes of sensations, which in turn were referable to dimensions or combinations of dimensions in the stimulus energies; and simplification was achieved by sticking to the "palpable" products of introspective analysis. Since only sensations and images were observed in the act of analyzing perceptions, meaning was said to arise from juxtaposition of inherently meaningless components. Perceptual objects as such had no place in introspective psychology, since they could be described in terms of their attributes which alone possessed "psychological" reality. Objects such as chairs and roses, and properties such as neatness, grace, and beauty were regarded as logical or technological constructs, not as immediate data of experience with which scientific psychology should deal (Titchener, 1909).

Brief as this historical review of the "atomistic" approach to perception has been, the reader may ask: Why spend any time at all discussing out-worn and outmoded concepts and theories, since the best way to forget them is to let them die of inanition by not mentioning them. There are several reasons for resurrecting older, erroneous views. First, the logic of old approaches may appear in new guises and may not be recognized as such unless the student has been alerted. Today we find the logic of compounding in learning theory and cognition, where it is not so easily detected as in perception. Second, essentially Lockian notions are still found in some approaches to sensory processes and perception, e.g., in the assumption that some sensory presentations denote 'real' stimulus attributes and others are judgmental or inferential in nature although the latter are as predictable, vivid, and stimulus-bound as the former except that they do not correlate with pointlike stimulus energies, e.g., colored shadows, and anchor effects in perception and judgment. Third and finally, since many students and laymen have access only to the philosophical and older psychological accounts of perception, it is necessary to mention them in order that they may be evaluated in the light of present-day advances.

SOME PHYSIOLOGICAL CONSIDERATIONS

The concept of sensory process as well as that of perception has been broadened as knowledge has accumulated concerning its role in cognitive, affective, and emotional behavior. Effects of sensory deprivation, new knowledge concerning physiological interrelations of sensory, motor, and attentive (central) states, effects of feedback, learning, and needs, and the greater potentialities for stimulation in the environment have shown that perception is more than a complex of sensations and images. First it should be noted that the classic division of sensation and perception, with the latter arising out of the former through associative or compounding mechanisms, is now almost universally abandoned. It was made untenable by the phenomenological approach of Katz, the Gestalt psychologists, J. J. Gibson, and others to problems of color, space perception, and apparent movement. Thus Graham and Ratoosh (1962) point out that the use of the word "sensory" as different from the word "perceptual" can be justified only as a matter of convenience, not of content: "operationally and with respect to experimental procedures and outcomes the presumed differences between sensory discrimination and perceptual discrimination is meaningless" (p. 503). Since much of Graham's earlier work was concerned with what used to be called sensation as opposed to perception, this statement takes on added significance. There is, however, an essential difference between phenomenological and psychophysical procedures, which was pointed out by Graham (1958): the former involve relatively unrestricted stimuli and responses, while the latter usually involve a high degree of restriction in both. We shall return to this point later in our discussion of phenomenology.

The notion of simple, sensory deliverances of receptors fitted the physiological concept of the reflex arc, according to which sensory processes were regarded as the culmination of afferent impulses initiated by stimulation of sense organs followed by transmission of neural impulses to the brain and culminating in a response. What occurred in the central areas, when sensations were transformed into perceptions, was supposedly determined by afferent impulses and the associated cognitive-imaginal processes aroused by them. This view did not take into account central pooling, the influence of feedback from the organism's own activities, the gating action of the central nervous system on incoming impulses (Hernández-Peón, 1964), efferent control of receptor mechanisms (Lindsley, 1956), nor the conative and motivating involvements of sensory processes.

The more recent neurophysiological findings are epitomized in a summary of his work by Hernández-Peón. Through direct recording, he found that potentials from the cochlear nucleus, which represents the first synapse in the auditory pathway where impulses coming from the auditory nerve enter the brain, were reduced when mice were brought into the visual field of a cat. When the mice were removed, the auditory potentials recovered their normal amplitude. He furthermore reported that impulses as low down as the spinal cord (in the spinothalamic tract) were also blocked when the cat was attending to a rat or to the odor of fish. Since even electric shocks applied to the skin were blocked, it is evident that presumably intense, disagreeable stimulation may be inhibited when attention is directed to other stimuli. Generally, when attention is focused upon a stimulus of any given sensory modality, transmission at the first synapse of other sensory pathways is partially blocked. There is also selective blocking within the same sensory modality for objects outside the focus of attention, e.g., exclusion of the curtain and stage contiguous to the screen as we watch a movie. Concurrently with inhibition there is also facilitation of signals generated by stimuli in the focus of attention according to Hernández-Peón. The simple reflex arc concept has also been shaken by demonstration of efferent control of receptor processes in practically all sense modalities (Edelberg, 1961). Lowered thresholds can result from tonic bombardment along efferent fibers. Lindsley (1956) pointed out that a new principle of behavioral control has emerged from work showing that "all sense modalities have some means of centrifugal control either at the level of the receptor itself, through reflex loops, at the first or second synapse, or more centrally located stations along efferent pathways" (p. 335). Efferent impulses may alter the excitability of receptors and thereby exercise a gating effect on afferent input.

Subcortical sensory blocking has also been demonstrated in man not only during attention to sensory input but also when attention is focused on ideas or central processes. Blocking, according to Hernández-Peón, was also achieved in human subjects by verbal suggestion: with electrodes implanted in the optic radiations, potentials evoked by flashes of light could be reduced or increased by suggesting to Ss that they would receive more or less intense flashes, and hypnotically induced anesthesia may result in selective decrease of excitability at the *spinal* level. Finally, there are also complex relations between arousal (vigilance) and sleep systems which may be either facilitating or inhibiting. Facilitation of some pathways or brain systems is accompanied by inhibition of others, even in sleep.

That sensory stimulation has a far wider significance than mere registration of impressions is shown by a review of literature dealing with effects of sensory deprivation (Miller, 1960). It is now known that sensory stimulation is necessary during early infancy for proper development of the visual system as a result of observations of chimpanzees raised in darkness (Riesen's study quoted by Miller), and such basic activities as urination and defecation in rats and color changes in kittens depend upon or are speeded up through appropriate types of stimulation during early life. Henry Head has pointed out somewhere in his writings that skin deprived of its normal afferent nerve supply becomes inflamed and otherwise abnormal showing that afferent nerves have nutrient as well as sensory functions. While perceptual disturbances have been stressed in studies of sensory deprivation, there are also cognitive-emotional disturbances. Suedfeld, Grissom, and Vernon (1964) point out that the failure in some studies to find conclusive evidence of cognitive decrement in problem solving, learning, and reasoning tasks was due to the nature of the tasks which were short, structured, and definite. Using unstructured, variety-requiring, TAT-like tasks in which Ss were required to tell a story about material given them, these investigators found after twenty-four hours of sensory deprivation a highly significant loss in number of words and lowered speech rate. Socially isolated but not sensorially isolated Ss gave significantly greater numbers of words, but there was some loss in speech rate after twenty-four hours.

PERCEPTION, AFFECT, AND MOTIVATION

That perception is more or less tinged with affect is generally recognized, yet psychologists have been slow to include affect as a dimension of perception. In their "discrepancy-from-adaptation-level" theory of affect, McClelland and Clark (1953) have incorporated affectivity along with intensity and other dimensions as a dimension of sensory processes. Their theory is based on the assumption that stimuli to which the organism is adapted are neutral in the sense that they are neither too loud nor too soft, too bright nor too dim, too sweet nor lacking in sweetness, etc. They then add the assumption that small departures from level are pleasant and larger departures are increasingly unpleasant. Haber has tested this theory in the field of temperature (1958). He found for adaptation to temperatures in the vicinity of 33°C the maximum number of preferences for warm and cool came within ±1°, with some Ss preferring departures as much as 3° from adaptation level. Support for the McClelland-Clark theory also comes from a study by Conners (1964), utilizing a set of seven randomly constructed visual figures to one of which Ss adapted by copying it each time before looking at all the members of the set. The members of the set were exposed two at a time and Ss were told to let their gaze be drawn toward one or the other member of the pairs being exposed. Both the fixation times and the verbal reports of pleasantness showed that, in the case of one of the two adapted figures, there was a distinct perference for the figure next to it, while in the case of the other adapted figure the results were confirming but not so clear-cut. Considering the tendency for visual stimuli of this sort to be neutral and the difficulty of scaling randomly constructed figures on a similarity continuum, the results must be considered highly suggestive and, in general, confirmatory of the McClelland-Clark theory in a new area.

Wherever there is affect we can presume there is also motivation. The fact that tastes and odors are so heavily affectively loaded leads us to expect them to be strong motivators also, and this point has been stressed by Pfaffmann (1961; 1962). Using taste as an example. Pfaffmann has been one of the few psychologists to emphasize the motivating properties of sensory stimulation. He points out that stimuli elicit other functions than the commonly stressed discriminatory ones in classical sensory studies. This view has important consequences for the sensory control of behavior. Among them is his suggestion that the classical manifold of four tastes—salt, sour, bitter, and sweet—may be reduced to two behavioral classes: acceptance and rejection. However, some substances may be accepted at low but rejected at high concentrations, e.g., NaCl and sugar. No preference was shown by rats for acid and quinine solutions which start at about 50 per cent at low concentrations and decline rapidly with higher concentrations. Sugar and salt show roughly inverted U-shaped curves of preference as a function of log mol concentration. Sweet seems to be the only substance that has a high preference rating with very strong concentrations. This leads to the well-known generalization regarding the pleasantness of tastes: sweet is predominantly pleasant, bitter is predominantly unpleasant, and salt and sour are intermediate. Direct confirmation of central effects from reinforcement have been found by Hernández-Peón: unrewarded stimuli such as repeated fish odors result in decreased activity in the olfactory bulb, but if the cat is given fish to eat (positive reinforcement), the activity in the bulb increases (Hernández-Peón, 1964).

Affective values of colors and tones have been determined and reveal systematic relationships contrary to the popular belief that affective preference (aesthetics) is a purely personal matter. Guilford and his co-workers (Guilford and Smith, 1959) have mapped isohedonic contours for the hues as a function of their saturation and "brightness," and from these maps the domains of pleasant, unpleasant, and neutral colors can be read. Earlier, Guilford (1954) mapped the domains of pleasant, neutral, and unpleasant pure tones as a function of loudness and pitch (intensity and frequency). Studies are needed in other sense modalities to furnish information for a taxonomy of their affective values as a function of dimensional variables.

We thus find that recent neurophysiological studies have shown the classical afferent-central-efferent one-way conduction is an oversimplified view of what goes on in the nervous system in the simplest sensorimotor behavior. There is now evidence for central gating of incoming impulses and efferent control of receptor thresholds. Furthermore, afferent input has a wider significance than mere registration of sensory contents. Sensory deprivation studies, results of deafferenting the skin, and appreciation of the motivating properties of stimulation have served to place sensory input and perception in a wider neurophysiological and behavioral context. Finally, the affective and motivating properties of sensory qualities are now recognized by workers in sensory physiology and neurophysiology.

THE PHENOMENOLOGICAL APPROACH

While there were criticisms of Wundtian introspection as the sole method of studying psychological data in the latter part of the nineteenth and early in the twentieth century (cf. Helson, 1925), it was not until the phenomenology of Husserl (1913) was modified to serve as a tool in psychological investigations by Katz (1911), Wertheimer (1912), and Rubin (1921) that the approach to perception changed from dimensional analysis to the description of molar properties of objects and temporal events. New dimensions and modes of color appearance, pattern and form perception, illusory movement, and figure-ground phenomena were studied in experimental laboratories by these and other workers. Even such tertiary qualities as beauty, grace, friendliness, and other "physiognomic characters" (Koffka, 1935) were held to be as immediately perceived as dimensional attributes of perception. Learning and judgment were relegated to a very minor, if any, role in determining what is perceived under various conditions. Every experience was valid in its own right and without regard to its reality status. Thus movement perceived from rapid succession of stationary stimuli (phi phenomenon) was held to be as real psychologically as perception of actually moving objects. Phenomenology thus broadened the concept of what is immediately given in perception to include properties of complex stimuli and temporally extended stimuli such as melodies and trains of thought. This point of view has not been without effect on all areas of psychology (cf. Snygg and Combs, 1949) and is still a vigorous movement as is shown by the fact a whole session was devoted to "the phenomenological approach in psychology" at the International Congress of Psychology held in Bonn, in 1960 (see, for example, Herrmann, 1961).

Impetus to the phenomenological approach was given by the great physicist, Ernst Mach (1886), and the philosopher-psychologist, von Ehrenfels (1890). The former maintained that all science, physics included, studies only sensations in various domains. But more important for psychology was his demonstration that sensory contents appear in "space" and "time" forms which are over and above their qualitative contents, hence they are formal in character. Thus red and green squares possess the same formal space form, and different melodies having the same rhythm possess the same time form. Ehrenfels regarded these space and time forms as suprasummative properties, not derivable from similar properties and activities of their parts, and Köhler, following him, argued: ". . . it cannot be assumed that sensations of color and tone, and meanings of single words, are to be considered as 'parts' of space-forms, melodies, and higher thought processes; since the exact impression of a visual figure, or the specific character of a musical motif, and the meaning of an intelligible proposition, contain more than a sum of patches of color, tone sensations, and individual word meanings" (1924, p. ix). It was thus that effects of organization in space and time were brought into psychology as a major problem in their own right.

The study of visual qualities was also extended beyond the boundaries of attributive analysis, which yielded too few dimensions with which to encompass visual experience. Katz (1911) showed that the achromatic colors were not adequately described purely as a brightness series but, like the chromatic colors, were also characterized by pronouncedness and insistence. The list of attributes of color was enlarged from the classical three, hue, brightness, and saturation, and modes of color appearance were described by Katz, which possess varying numbers of attributes as listed in Table 8-1. Katz's work opened new vistas for visual science and has also provided the vocabulary for communicating with artists, scientists, and laymen about visual phenomena that were previously only known intuitively.

Similar advances were made by Wertheimer (1923; English translation, 1958) with respect to perception of form in his principles of visual organization. Gestalten tend to be as symmetrical, balanced, complete, and simple as possible. These tendencies can be summarized in the law of *Prägnanz* according to which gestalten tend to be as "good" as prevailing conditions allow. This law accounts for both accentuation or sharpening and leveling of configurational properties in brief exposures and over time in memory. Still another law is that of "good continuation" according to which forms are perceived in such a way as to avoid sharp breaks, angles, or abrupt changes in direction. Factors making for grouping of parts or elements (or subunits) are nearness, similarity, and common fate. Wertheimer also stressed the stepwise character of many of our perceptions, e.g., in the tendency to perceive angles in broad schemata or categories, as either acute, obtuse, or right. Thus an angle of 93° is seen as a deviation from a right angle or as a "bad" right angle. Examples of these generalizations originally published by Wertheimer (1923; 1958) can now be found in most psychology textbooks (e.g., Woodworth and Schlosberg, 1954) and in books by psychologists writing on the psychology of art (Arnheim, 1954; Gulamerian, 1963). By 1933 the writer culled 114 "laws" from the Gestalt literature (Helson, 1933) concerning organization, patterning, and behavior of configurations under various conditions which Boring (1942) reduced to fourteen generalizations.

Two Experimental Tests of Configurational Laws. Wertheimer's generalizations concerning the behavior of forms were based upon patterns that were observed and compared under ordinary conditions of viewing. Would these configurational laws stand up under different conditions, say under tachistoscopic exposure, where what is perceived results from the "inner dynamics" of the forms themselves? An experiment was accordingly performed by Fehrer (1935), in which 128 figures were exposed at intervals ranging from 40 to 680 σ. Eight types of figures were employed: simple geometrical, dot figures, simple symmetrical and asymmetrical outline figures, complex asymmetrical, simple and complex "broken" figures, and figures composed of parts that were complete units in themselves. Difficulty scores were computed for each of the 128 figures based upon the number of times they had to be presented for correct reproduction weighted by the time of exposure. Considering only the ten easiest and the ten most difficult figures, it was found that all but one of the best figures were symmetrical, geometrical, closed figures, while six of the worst were dot figures, seven were asymmetrical, and seven were open-line or dot figures. In general, difficulty increased with complexity. The types of errors made in reproducing the figures indicate how they tended to be perceived. There were eleven types of errors with frequencies of each in per cents as follows: (1) leveling, 21.9; (2) more complicated than original, 18.4; (3) accentuation, 18.3; (4) greater symmetry introduced, 16.0; (5) qualitatively different figure, 8.2; (6) simplification, 7.6; (7) greater asymmetry than original, 6.7; (8) incomplete, 4.5; (9) nothing seen, 1.4; (10) formless or chaotic, 0.6; and (11) only position of 'something' indicated, 0.1.

From Fehrer's results it is seen that the laws of gestalt were amply confirmed according to two different criteria: ease or spontaneity in perception and by the ways in which the stimuli changed when the times were too short for them to be perceived correctly. One new and wholly unexpected finding at variance with the law of simplicity was the large number of cases (18.4 per cent) in which perceived figures were *more complicated* than the original figures. The tendency toward

TABLE 8-1

The attributes or dimensions of color classified with respect to the modes of appearance

	Modes of Appearance				
Attributes	1 Illuminant (glow)	2 Illumination (space filling)	3 Surface (object)	4 Volume (tridimensional)	5 Film (aperture)
1. Hue	x	x	x	x	x
2. Brightness	x	x	x	x	x
3. Saturation	x	x	x	x	x
4. Lightness			x		
5. Duration	x	x	x	x	x
6. Size	x	?	x	x	(x)
7. Shape	x	?	x	x	(x)
8. Location	x	(x)	x	x	not in depth
9. Texture	x	?	x	x	x
10. Gloss (lustre)			x	x	
11. Transparency	(x)	x	x		
12. Fluctuation (flicker, sparkle, glitter)	x	x	x	x	
13. Insistence	x	x	x	x	(x)
14. Pronouncedness	x	?	x	x	
15. Clearness[1]				x	
16. Affective quality	x	x	x	x	x

Adapted from *The Science of Color*, Committee on Colorimetry, Optical Society of America. New York: Thomas Y. Crowell Co., 1953 p. 151.

Parentheses indicate the attribute, in this writer's opinion, is indefinite for the mode in question and the question symbol stands for doubt as to whether or not the attribute is found in the mode.

[1] This attribute, first pointed out by Evans (1959) is regarded by him as "degrees of whiteness" (p. 66) in line with his example: by progressively diluting a glass of milk with water it finally becomes clear. The liquid does not go through any stage that can be called "gray." But the same may be done with black ink, and so I would treat this as a case of "turbid-clear" dimension just as transparency is the endpoint of an "opaque-transparent" dimension.

simplification under conditions of brief exposure seems intuitively sound on the premise that there isn't enough time for everything to be seen; but complication is difficult to understand when it does not contribute to symmetry, completeness, or other configurational properties. Thus a semicircle composed of seven dots was reproduced correctly but with nine dots more closely spaced together, while a parallelogram with gaps in two corners was reproduced with gaps in three corners.

In the second study by Mowatt (1940), the configurational principles were tested in reverse, so to speak, by presenting figures to naive Ss with instructions to change them in any way to make them "better" figures. Mowatt verified Fehrer's results, including the tendency toward complication. In this study forty figures were employed, of which one-half were symmetrical in one or more axes, one-half were asymmetrical, eight were dot figures, and the remaining thirty-two were line figures. One-half the line figures were closed or continuous, and the other half were composed of unconnected lines or one line with one or more changes in direction. The figures least frequently modified in any way and their number in per cents were: isosceles triangle, 72; circle, 70; hexagon, 62; rectangle, 62; and square, 60. The types of figures changed by all Ss (except one who did not change any of the forty figures) were the open, asymmetrical, and irregular figures. Asymmetrical figures were changed more than symmetrical and open more than closed. The changes were in the direction of greater simplification, symmetry, closure, good continuation, and complication or greater differentiation. In 94 per cent of the cases something was added to the figures and in 3 per cent was something subtracted. Only 3 per cent of the figures were left unchanged. While accentuation and leveling appeared, they were not counted because of the difficulty in making exact determinations concerning these changes.

The results of the Fehrer and Mowatt studies make it clear why configurational principles find expression in pictorial art, where many of the techniques have developed by trial and error and by nothing more than intuition as to what is good or bad to the eye. The fact that these principles are found operative in tachistoscopic exposures as well as in *Dauerbeobachtung* seems to point to their locus in basic physiological mechanisms as claimed by the Gestalt psychologists.

Still other types of forms have been described, notably by Gibson (1951), who singles out the following ten with illustrations of each:

1. *Solid form:* The margin or contour formed by two solid objects.
2. *Surface form:* Flat physical surfaces and their edges at various degrees of slant.
3. *Outline form:* The tracings of objects by means of pencil or pen on a surface.
4. *Pictorial form:* Photographs, paintings, and drawings.
5. *Plan form:* A plan view of the edges of a surface as in engineering drawings.
6. *Perspective form:* Outlines indicating a perspective projection or view of an object.
7. *Plane geometrical form:* An imaginary closed line on an imaginary plane, although in actual representations composed of finite lines in given planes.
8. *Solid geometrical form:* Geometrical forms in three dimensions.
9. *Projected form:* A form in one: one correspondence with another form, e.g., an object and its shadow.
10. *Nonsense form:* Tracings on a surface which do not represent recognizable objects.

To Gibson's list we would add:

11. *Dot and broken line forms:* Forms made up of dots, short lines, and combinations of both, such as were employed in the study by Fehrer referred to above.
12. *Open forms:* Incomplete drawings such as triangle with apex missing or surface forms with parts taken out such as a pie with a piece cut out.
13. *Broken areas:* Perceived as objects as in the Street Gestalt completion figures.

Object recognition depends largely upon form, especially in animals devoid of color vision, and the orientation of the form may determine what kind of object is perceived. Tinbergen (1951), using an ambiguous figure, which resembled a hawk from the left and a goose from the right, found that several species of birds exhibited fear reactions when the figure was moved to the left, accenting the hawklike appearance, but showed no such reactions when it was moved to the right, emphasizing the gooselike appearance. Although these birds were reared in isolation, their fear reactions to hawklike appearance were immediate.

Direction and orientation, as well as other figural properties, influence our actions. Bühler (1951-52) has described how bees and ants use polarized light to determine the course angle necessary to return to hive or anthill. We should add that it is possible that their behavior is guided instead by the position of the sun and earth landmarks. The pointing of the compass needle instructs the navigator of a ship which direction he must follow; and when entering a harbor, he must shift from instrumental indicators to landmarks, buoys, and other fixed objects to find his way (Bühler, 1954). Landmarks have a directive quality and a location quality which indicate "there is where I want to go." As odor may lead an animal to food, the names, directions, angles, and intersections of streets furnish a coordinate system that orient humans to various goals, near or far.

We should, however, point out that the directional qualities of landmarks, signs, and pointing devices are not inherent in them as such, but that they depend rather upon the contexts in which they appear. Up is away from the surface of the earth, whether at the north or south pole; right is on the side of the body away from the heart; and north is variously taken with respect to the magnetic pole or to the pole of the earth, where *Polaris* is directly overhead. Even the visual vertical and horizontal, favored by our predominantly upright position and by the responses of the tactile-kinesthetic systems to gravitational force, are influenced by contextual stimuli that can be made to distort these directions as perceived. Only recently have the background and contextual stimuli responsible for the establishment of spatial frames of reference been investigated, e.g., by the Ames group (cf. Ittelson, 1952).

We are accustomed to think of directive stimuli as either spatial or cognitive, but often internal agents may be responsible for making it possible for stimuli to exert directive effects they would not otherwise have. Nissen (1951) gives the following example, instructive in this respect. "When carbon dioxide is added to the water of their aquarium, Daphnia ordinarily rise to the surface, but if the aquarium is lighted from below, the addition of carbon dioxide results in movement to the bottom of the tank. Without addition of carbon dioxide there is no orientation to light. The carbon dioxide supplies the sensitizing factor, light the directive component" (p. 356).

Although objects are oriented with respect to their environments, they are also internally differentiated so that directions may be laid down within the framework of the objects themselves. Both animate and inanimate objects, including man's artifacts, are structured internally in accordance with functional considerations. Tables, chairs, and beds stand on their legs, and the orifices of containers are at the top. What an impression of disorder prevails when such objects are overturned, and how difficult it is to learn to use shakers that discharge salt or pepper from their bases! The writer was astounded the first time he saw a snake of the species called "sidewinder" progressing sidewise by first forming loops at right angles to its length, then elongating its body, forming new loops, and moving perpendicular to the long axis of its body. It is often difficult to determine whether some modern cars are approaching or receding, because front and back look so much alike in the distance. We repeatedly perceive stimuli whose internal structures bear fixed relations to other objects. These relations impress themselves upon us and influence our responses later also by their residual effects. When object structures and functions are not in accord with our expectancies, we are disturbed, and our reactions to them may suffer. Our spatial world is a synthetic structure built up by the pooled effects of present stimuli more or less modified by residuals operative from previous experience.

A more explicit attempt to combine the psychophysical with the phenomenological approach has been made by Gibson in a number of notable publications, in which he has shown that many perceived object properties can be traced to stimulus conditions if a broader view of what is the stimulus is taken. Contrary to the prevalent view that light carries very little information and that space perception is a matter of learning and experience, Gibson points out that geometrical relations are given or specified by the light reaching the eye (1960, p. 25). Starting from the "optic array," he describes how multiply-reflected light in a clear medium (air) from opaque surfaces having imperfect smoothness gives rise to different intensities and compositions in different directions when taken in small angular units. This yields an "optical texture." While the use made of the information in ambient light may depend upon the anatomy and physiology of the eye, stage of growth of the individual, and practice and attention, the point is that the information is there in the light to be perceived. The following types of information as well as their importance for behavior are said by Gibson to be present in the light stimulus:

1. Presence or absence of texture. When absent in the downward direction it means loss of support and falling as shown by the visual cliff experiments of Walk, Eleanor Gibson, and Tighe (1957), wherein rats on an elevated wooden runway would not descend to an *optically far* surface having a denser and smaller texture than the *optically near* surface with its coarser texture;

2. Pattern or form as well as various types of texture such as grainy, pebbled, mottled, aligned, irregular, sinuous;

3. Closed contours yielding impression of solid, detached objects;

4. Gradients of density of the same texture yielding impressions of distance. Finer textures appear more distant in accordance with the law of visual angle;

5. Projective transformations of both contours and textures with movement of the organism providing guidance and control of locomotion;

6. Invariance of optical arrays under a variety of conditions, e.g., from tri- to bidimensional projections and with changes in perspective due to locomotion of the observer. This last property may yield the ultimate solution to the problem of constancy and the stability of the phenomenal environment with changes in the retinal image.

Gibson has added to the usual spatial directions of up–down, forward–backward, and near–far, the dimensions of slant and tilt, which he derives from differential texture gradients in the visual field (1950*a*; 1950*b*). Gibson and Cornsweet (1952) distinguish between geographical and optical slant: a desert plane is geographically flat, but an individual looking into the distance sees an optical slant that increases from himself to the horizon. This slant is accompanied by a gradient of texture that becomes finer and finer as the eye travels from near to far. Retinal density gradient is important for optical slant and is built up, according to these writers, by successive retinal images and postural-gravitational stimuli as well. Whereas slant concerns rising or falling level from floor to ceiling or from ground to sky (down–up) and from wall to wall (right–left), tilt depends upon a clockwise or counterclockwise rotation of a surface with respect to either the up–down axis of the retina or the gravitational vertical. Measured differences found by Gibson and Cornsweet between optical and geographical slant indicate that they are functionally and phenomenologically different.

The effect of slant on depth perception was measured by Dusek, Teichner, and Kobrick (1955). Using a modified Howard-Dolman apparatus, Ss made equality settings between a standard and a variable by bringing the variable toward the standard in the third dimension. This procedure was repeated with the table at 0°, 1.7°, and 2.9° slope on the hypothesis that angular relationships between the observer and the base-surround would influence depth acuity. As slope and eye level increased, sensitivity also increased. The authors concluded that the slope of the terrain is important in discriminating distances out of doors. We find thus that phenomenological taxonomies provide new directions in which to look for explanations of behavioral data that otherwise could not be accounted for.

The concept of texture, like that of form, is not unidimensional. Hunter (1935) has described the texture of surfaces with a view toward measuring gloss as smooth, wavy, matt, blotter, pimpled or pitted, orange peel, brush-marked, checked, scratched (when surfaces are otherwise smooth), and yarn, which is a finer type of brush-marked. Except for types of texture mentioned in connection with their effects on gloss measurements, the varieties of texture remain to be evaluated in psychophysical terms. Theoretically, problems of texture raise questions concerning the manner in which the retinal mosaic of receptors is able to deliver the information on which differences of texture must ultimately rest. Practically, the control of textural qualities may require knowledge of the psychophysics of textures, as the psychophysics of color has proved to be of great importance in colorimetry.

Carrying the analysis to the tactile field, Gibson finds that many of the spatial properties of vision are found also in tactile-kinesthetic perception. Differences between the qualities and information yielded by active, searching touch and passive touch have been described by Gibson (1962). He finds an analogy between the former and ocular scanning. Active touch mediates the shape of objects which is over and above what is given by passively received contact and kinesthesis. Active touch involves joints and tendons in addition to the usual tactile components. In active touch the perceived qualities are referred to the object rather than to the skin or to the self. In this respect touch is more like vision, where impressions are projected outward. Accompanying this outward reference is perception of stability as the hand moves over the stimulus. In spite of changing tactile sensations as the hand moves over objects, the form of the object remains invariant, not the form of the hand or skin. New qualities are perceived in active touch such as rough–smooth, soft–hard, and various kinds of plasticity, elasticity, and viscosity—the latter first reported by Katz (1925). Gibson also adds new "solid geometry" qualities of curvature, planarity, slant with respect to gravity, parallelity of surfaces, edges, angles, and corners as types of information yielded by active touch. And like vision, forms are transposable tactually when moved across the skin, and, as pointed out by Deutsch (1962), under rotation, with changes in size, and in right-left (mirror image) reversals.

The phenomenological approach also yields new sources of stimulation in touch. Thus Gibson (1962) distinguishes between brief deformations of the skin which give pressure, push, pat, slap, prick, and tap and prolonged deformations without displacement which give vibration, stretching, kneading, and pinching; and prolonged deformations with displacement which give scratching, scraping, rubbing, sliding, brushing, and rolling. We find that both the perceptual and stimulus domains of touch are greatly extended as a result of the phenomenological approach, just as it has been in color vision.

The question as to whether or not phenomenal properties are constitutive, i.e., whether or not perceived objects and relations are governed wholly by anatomical (peripheral or central) mechanisms or to some extent by their phenomenal properties, comes up again and again (cf. Helson, 1964). Two studies by Rock and his coworkers show that perceived location rather than place of retinal excitation is the primary determinant of stroboscopic movement and visual grouping. In the study by Rock and Brosgole (1964) a test was made to determine if the Gestalt law of grouping by proximity is based on retinal or perceived proximity of elements. Dots separated by 4 in horizontally and 3 in vertically were perceived as columns because of the wider horizontal separations. The dots were then turned out of the frontal-parallel plane about their vertical axis and the reports of Ss changed from columns to rows at about 45° for monocular and at 55° for binocular viewing. Since the horizontal separation would equal the vertical at an angle of 41.4° the dots were perceived in columns after the horizontal distances were less than the vertical distances. From this and similar results in another experiment they conclude that phenomenal grouping is not based (entirely?) on retinal location. To test for phenomenal constancy, Ss made estimates of the vertical and horizontal distances in inches. Constant estimates prevailed for binocular viewing almost up to the most extreme degrees of tilt, which ranged from 50° to 60°. There was, however, much less constancy in monocular viewing. These writers assert that localizing autochthonous Gestalt factors in the stimulus configurations is a weakness in the configurationists' position.

In a second experiment, Rock and Ebenholtz (1962) tested the Gestalt explanation of apparent movement from stationary stimuli in terms of central physiological interactions between loci of excitations yielded by the two stimuli. If apparent movement can be perceived when only a single retinal area is excited or when there is no change in one of the areas, this would argue against strict anatomical determination of apparent movement. It is usually assumed, they point out, that the minimum condition for apparent movement is that *different* spots on the retina be excited. Can movement be perceived when there is phenomenal but not retinal difference? To answer this question several experiments were performed.

In the first experiment Ss moved their eyes from one slit to another to view spots of light in different places, but in doing so the same foveal elements were excited. Under these conditions apparent movement was seen. Six of ten Ss saw one object moving; the other four Ss saw two lights flashing on and off in their respective different positions. In a second part to this experiment different regions of retinal stimulation were phenomenally localized in the same place. By having S move his eyes so that a flashing light hit the fovea and then the periphery, different retinal areas were stimulated but the light was seen in the same position. None of the Ss who saw movement in the first part of this experiment reported it here. There was no perceived movement here because the flash was seen in the same place in spite of exciting different retinal loci.

In a second experiment, conducted in three parts, a long luminous line moved back and forth while a short line flashed on and off in a given position. The short line was seen to move in a semicircular path about a taller luminous line in the third dimension, the latter moving back and forth. This effect parallels Duncker's induced movement, where a stationary object in a frame of reference that moves appears to move also, although there is no stimulation of different retinal points. When this experiment was repeated with the dimensions of the moving and stationary lines reversed so the short line only moved, no movement of the longer line was perceived. The third part of this experiment with phenomenal location of the stationary line uncertain and in conflict with observed motion of another line yielded ambiguous results. From all these observations it appears that change in phenomenal location must be given unequivocally for motion to be perceived, whether or not different retinal areas are stimulated.

From the discussion up to this point it might be inferred from the results showing that phenominal properties of objects influence other perceived attributes that they do so in all cases. There is much evidence to the contrary, e.g., different geometric figures have not been found to have significantly different absolute thresholds (Helson and Fehrer, 1932). But since there are many cases in which certain phenomenal properties may act against assumed anatomical determinants of perception, as in the perception of depth from parallel lines which appear to diverge in stereoscopic presentations of the Zöllner figure (Squires, 1956), it cannot be assumed that phenomenological properties are or are not constitutive without experimental proof that such is the case.

The Information Approach to Form Perception. An entirely new approach to the quantification of dimensions of forms was made possible by the work of Shannon (1948) known as information-theory, which has served, among other things, to show that the concept of form is by no means simple and unanalyzable. Michels and Zusne (1965) point out that quantification has now passed beyond the limits of information theory, and there is a plethora of physical form measures. Information theory is based essentially on the extent to which ignorance is replaced by knowledge: the less likely the occurrence of an event or the greater the uncertainty or variance of an event, the greater is the information it imparts when it occurs. In this way of looking at things information is conveyed by surprise, change, improbability. Thus if we look at a solid black square on a white ground, information is conveyed only at the contours where white changes to black. In this figure, the corners are the places where the greatest amount of information is conveyed. The 'filling' inside the square merely repeats itself, if we take it point by point, and hence is redundant and conveys no information. Measurement of information is in 'bits,' a bit being defined in terms of reduction of possible alternatives by one-half. Thus in throwing a coin, since there are two sides, the appearance of heads conveys one bit of information because the two possibilities have been reduced to one. To predict what will happen when eight alternatives are possible, three bits of information are necessary. In short, the number of bits is merely the power to which the number 2 must be raised to equal the number of alternatives, or, conversely, it is the logarithm of the number to the base 2 (cf. Miller, 1953; 1956).

What we have just said concerning information theory and form perception is not entirely accurate, for it has become clear that, as Garner has emphasized: "The concept of redundancy can be dealt with in an exact manner only when we are dealing with *sets of stimuli* (italics ours), not with single stimuli, since redundancy is a measure (in uncertainty terms) of the extent to which an actual number of stimuli is smaller than the maximum number which could exist given the same number of variables with a given uncertainty . . . Uncertainty measurement deals with sets of stimuli, and probability distributions of these sets" (1962, p. 182). It is true, however, that a structured pattern is (more or less) redundant, and a meaningless pattern is one that is random or nonredundant. The difficulty, as Garner points out, is in specifying or determining known amounts of redundancy in given figures. Strictly speaking, then, we should apply information theory to a stimulus only as a member of a class of stimuli defined in some way.

Having glanced briefly at the informational approach, let us return to the properties of forms that have been investigated quantitatively. Michels and Zusne (1965) list information content, linearity of contour, area, rotation, elongation, dispersion defined as perimeter squared divided by area, symmetry, sizes and relations among angles, curvature, area overlap, ratios of angles, and various relations of parts within figures and contours. Among these many possibilities of variation and measurement of forms Attneave (1957) and Arnoult (1960) have shown that 87 per cent of the variance in responses to forms could be accounted for by the number of independent sides, angular variability, symmetry, and the ratio of perimeter squared to area. So far as the study of forms is concerned, information theory has fostered the use of nonsense forms constructed by choosing points according to tables of random numbers. The result has been that there is often a seeming contradiction between informational and response criteria of what constitutes good, recognizable, or other behaviorally determined criteria of forms, e.g., low threshold in short exposures, visibility under reduced illumination, discriminability, correct reproduction, etc. Thus according to Michels and Zusne increase in information usually leads to increased difficulty in performance, but Garner claims that more redundant figures are more complex and hence easier to discriminate. In short, while increased information might seem to be good in the sense of providing cues for easier recognition and discriminability, this may not be the case when increased complexity makes for difficulty in correctly perceiving, remembering, and reproducing forms (cf. Fehrer's study discussed above). Probably the resolution of these apparent contradictions lies in the fact that amount of information (complexity) and goodness of response measures are not monotonically related, i.e., up to a certain point increased information or complexity may aid and thereafter it is deleterious to ease of perception. In support of this conclusion is the fact that the hardest figures for color-blind individuals to discriminate in tests of the Ishihara type are the rounded ones; those with sharp breaks and corners are the easiest. Results of a study by Gaito (1959) bear directly on this point also. Exposure thresholds for four figures were determined. Using a straight line, arc of a circle, angle with apex upwards, and an incomplete box (long horizontal line with short verticals at the ends), Gaito found that line and arc of circle had the lowest thresholds and angle and "box" the highest. These results are hard to interpret wholly on an information theory basis because the line yields information at two places (the ends), the arc at three places (ends and middle of the top), the angle at three places (ends and apex), and the "box" at four places (where the short verticals join the horizontal and at their ends). According to Gaito an information theory interpretation is only partially correct in that it must be supplemented by consideration of critical detail, e.g., length of the two short vertical lines of the "box" figure.

Another difficulty with information theory is that it does not take into account all stimulus relations, e.g., effects of rotation on figures, changes in size, and conditions of observation, nor does it make any reference to properties of the reacting organism or the individual differences among observers, e.g., decreased visual acuity of the color-blind. Thus the change in a square to a diamond appearance, when it is rotated so that it stands on a corner, is not accounted for by information theory, and the new figures perceived in tachistoscopic exposures and under reduced illumination require a knowledge of how forms are perceived under various conditions in order to predict responses. These shortcomings of information theory in dealing with perceived forms are not meant to convey the impression that this theory has no place in studying perception, judgment, and learning. A large literature shows that it has contributed much to all these areas (cf. Miller, 1956, who cites many sensory studies elucidated by information theory), but it has not made unnecessary such behavioral studies as those of Fehrer, Mowatt, the Gestalt psychologists, and psychophysical studies of judgment and sensory processes.

ADAPTATION, CONTRAST, ASSIMILATION, AND CONSTANCY

considered

(Explanations of perceptual phenomena have been couched in terms of four mechanisms or processes conceived by earlier theorists to be more or less independent of each other. They are adaptation, contrast, assimilation, and constancy. Adaptation was traditionally conceived as a leveling process both in the sense of reducing differences between observed properties of stimuli and in the sense of a reduction in effectiveness of stimulation.) A striking example of adaptation can be observed as follows: if a half-white, half-black field is fixated for a sufficient length of time the two sides of the field approach each other in lightness until, with complete adaptation, the two halves of the field are perceived as the same medium gray. Similarly with complete adaptation to odoriferous, gustatory, tactile, and temperature stimuli there is disappearance of sensory quality. Complete neutralization, however, occurs only with weak or intermediate intensities of stimulation. Contrast has effects opposite to those of adaptation in that differences between stimuli are enhanced. Thus the chromaticness of yellow is increased when juxtaposed to blue, and a sound of medium intensity is louder following a softer sound. The third process, assimilation, refers to reversal of classical contrast and appears in spatial as well as qualitative aspects of perception. Thus Müller-Lyer ascribed the illusion that bears his name to a "confluxion" (streaming together) effect in opposition to contrast (repelling) as pointed out by Ladd and Woodworth (1915) and the lightening of chromatic areas by white lines and their darkening by black lines in the von Bezold figures has been ascribed to "mixture" or assimilation of adjacent stimuli (Helson and Rohles, 1959). Sensory blends within and across modalities, the latter called "complications" by Wundt, can also be included among assimilation effects.

The fourth aspect of perception which has seemed to require a separate underlying mechanism is the phenomenon of constancy, which appears in the tendency of objects to hold their color, size, shape and other properties under changing conditions of stimulation. Constancy is the behavioral analogue of physiological homeostasis wherein mechanisms act to maintain stable values of fundamental bodily processes such as the pH of the blood and internal body temperature. In many cases constancy refers to the perception of certain *constant physical* properties of objects, e.g., reflectance, size, and shape. Constancy, then, represents an isomorphism between certain properties of objects and their perceived properties when *local* or proximal stimulation varies. By local we refer to the receptors directly affected by object stimulation divorced from the influence of contextual stimuli.

Before proceeding to show that adaptation, contrast, assimilation, and constancy are not the results of independent mechanisms, let us first take a closer look at phenomena of adaptation and its interrelations with the others.

The concept of adaptation in its narrowest sense has been restricted to the loss in sensitivity of peripheral sensory mechanisms (receptors) after relatively long exposures to stimulation. This view, like all definitions, is defensible if we are willing to overlook other important aspects of adaptive processes. Before the stationary level of sensitivity is reached in durative adaptation, there are rapid, transient changes in sensitivity as shown in the steep initial parts of curves of dark adaptation for both the foveal and the peripheral areas of the retina. Everyday experience attests the rapidity of the initial stages of adaptation: on going into daylight from dark the initial blinding effect is over in a few seconds; on plunging into water the shock of the cold medium disappears almost instantly; and adaptation to intense sounds, tastes, and odors, while not as rapid as that to light and cold, is nevertheless significant long before steady-state adaptation has been reached. Except under contrived experimental conditions the adaptation of receptor systems changes from moment to moment, with every change in the environment and every change in bodily activities. It is doubtful if steady-state adaptation occurs during normal, waking conditions. The adaptation level fluctuates continually as sounds rise and fall, as the eyes turn from one part of the visual field to another, as foods are tasted in succession, and as the temperature of the environing air changes or limbs are moved. Understanding the role of transient changes in adaptation can lead to greater insights regarding the nature of perceptual processes.

In addition to the fast, transient aspect of adaptation the classical view also neglects the sensitizing effects of adaptation. Adaptation to dark makes us more sensitive to light; adaptation to warmth makes us more sensitive to cold. Such effects have been attributed to "contrast" mechanisms, but they cannot be divorced from adaptive mechanisms, if indeed, they are at all separate. While the temporal aspect of adaptation has been stressed in sensory physiology, phenomena of "simultaneous" contrast prove that adaptive mechanisms may act so fast as to appear to be nontemporal in character (cf. Walls, 1960). We must therefore include in our view of adaptation transient as well as durative states, increased sensitivity as well as loss in sensitivity, and simultaneous as well as temporal phenomena of adaptation.

There is still another aspect of adaptation which has been neglected in studies of sensory adaptation, viz., central adaptation. It is very probable that there is not one but many types of central adaptation in view of the large variety of functions served by the brain. Both habituation and withdrawal symptoms in narcotics addiction must be attributed to central mechanisms. Even some sensory adaptations in the narrower, classical sense argue strongly for central as well as peripheral involvement. In this connection Aftanas and Zubek (1963) found that absence of tactual stimulation on an area of the skin for a week resulted in increased tactual acuity, while application of a constant light pressure resulted in decreased acuity. Since the effects lasted several days following the experimental treatment, it appears that the phenomena were at least partly central in character. Stronger evidence for central adaptation was found by Marion Aftanas, and Zubek in a later study (1964), which involved interlimb transfer of effects of adaptation. An area 4 cm in diameter on the forearm was covered by a plastic cup to ensure absence of stimulation with ten men Ss. A second group of ten men Ss had constant pressure on the same area, and a control group of ten men Ss merely had a plastic open ring bandaged to this area. Each treatment lasted one week. Two-point threshold and tactual fusion were determined before and immediately after each of the conditions and also one and two days later. The same tests were also made on homologous and nonhomologous areas of the contralateral arm. Fusion was tested by determining the critical frequency of percussion (CFP)—number of air vibrations—necessary to perceive fusion.

It was found that absence of stimulation was followed by increased CFP on the seventh day on both the affected arm and to a lesser extent on the contralateral area. Higher CFP was found on the day following the end of the experimental conditions on the affected area. No change was found in the control group or in the nonhomologous area of the experimental group. Similar results were obtained with two-point threshold measurements: absence of stimulation resulted in a lower threshold (both CFP and two-points) and constant stimulation produced a decrease in tactual acuity (increased two-point threshold), while the control group showed no change. There was some transfer to the contralateral, homologous area (increased sensitivity) following the constant stimulus condition. There was no transfer to the nonhomologous area of the contralateral arm and none in the case of the two-point threshold on the homologous area. In this experiment there were no perseverative effects on the first and second days following the stimulus treatments as contrasted with the first experiment, in which effects persisted to some extent for three days. Hence the transfer effects for both increased and decreased sensitivity must be attributed to central as well as to peripheral adaptation. The authors explain the transfer effects in terms of level of background activity in somatic areas I and II of the right hemisphere and somatic area I of the left hemisphere: neural impulses enter these regions after isolation against a reduced background of activity, whereas they enter these regions against a background of heightened activity after constant stimulation with resultant effects on discrimination. Whether or not the brain mechanisms responsible for these effects are correctly described, the phenomena point to central as well as peripheral adaptations, when conditions are made sufficiently compelling to induce durative modifications in the central processes.

It is apparent that the concept of adaptation must be extended beyond the classical definition, if transient as well as durative portions of adaptation curves are taken into consideration, if simultaneous and successive sensitizing (contrast) and desensitizing effects of adaptation are considered, and finally, if central as well as peripheral phenomena are taken into account. The concept of adaptation thus embraces a wider range of phenomena and also furnishes a unitary frame of reference to relate phenomena that were formerly regarded as separate.

There are cogent reasons for regarding adaptation, contrast, assimilation, and constancy as manifestations of a single underlying mechanism. First is the fact that they are all mediated by the same receptor systems. Thus the rods and cones and their appurtenant neural elements are certainly involved in all four types of phenomena. It is hard to believe that the eye, the ear, the skin senses, taste, and smell mechanisms act one way when adaptation is in question, and in other ways when contrast, constancy, or assimilation are in question. While inhibition may be responsible for contrast and facilitation or summation for assimilation, as we shall argue later, this does not mean that new or different mechanisms are involved in contrast as opposed to assimilation or that adaptation and contrast are *toto caelo* physiologically different. Second, it is not true that these characteristics of

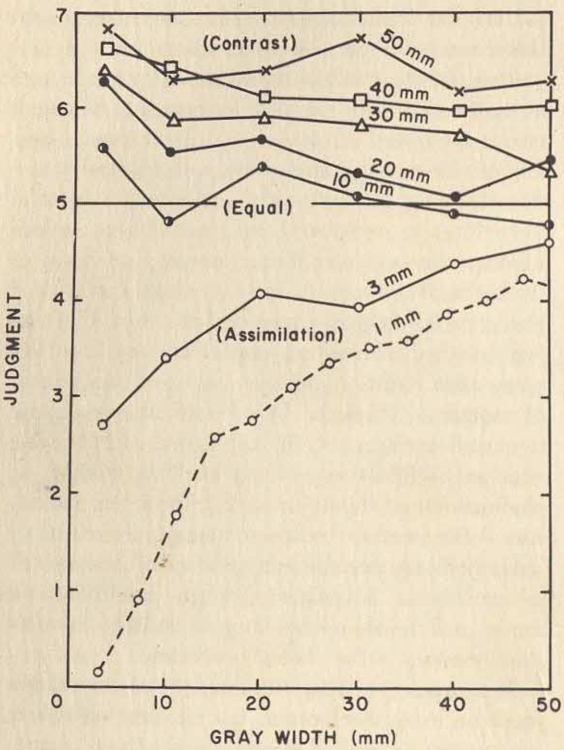


Fig. 8-1. Contrast and assimilation as a function of gray width for each width of line. The broken curve for 1-mm line width is from a study by Helson and Rohles (1959). The parameter of the curves is line width. (From Helson and Joy, 1962. With the permission of *Psychologische Beiträge.*)

perception are independent. As pointed out above, adaptation to one stimulus or quality makes us both simultaneously and successively more sensitive to the complementary quality or stimulus, hence adaptation and contrast are inextricably related. Titchener (1909) long ago stressed the interrelations of adaptation and contrast in the various sense modalities: "In smell, the effect of adaptation is to increase our sensitivity for certain qualities, and to reduce or destroy it for others. . . . A sour applied to one side of the tongue brings out . . . the taste of a subliminal sweet applied to the other side" (pp. 136-7). In the same vein Gibson (1937) has enunciated the principle of adaptation with negative aftereffect (contrast, in our terms), and the principle of color conversion (Helson, 1938) expresses the fact that adaptation and contrast occur together and are the results of a single visual mechanism.

What of constancy? Does it require the assumption of new or different mechanisms? Work by Bornemeier (quoted by Helson, 1943) showed that lightness constancy depends upon the state of adaptation of the eye. Constancy of color, size, and shape disappears if perspective and surroundings are obliterated by viewing objects through long, black tubes, or if stimuli are seen as filling apertures in texturally homogeneous, neutral surrounds.

Color contrast and assimilation (reversal of classical contrast) have been shown to lie on a single quantitative continuum in a series of studies by the writer and his co-workers (Helson and Rohles, 1959; Helson and Joy, 1962; Helson, 1963; Steger, 1964). We have already referred to the lightening (darkening) of chromatic areas overlaid by white (black) lines as examples of assimilation. Reference to Fig. 8-1 shows that the lightening effect of white lines or the darkening effect of black lines on a gray surface is inversely related to the width of the lines and of the intervening gray areas between the lines. The transition from assimilation to contrast occurs with lines about 10 mm wide; wider lines exert contrast effects. Contrast and assimilation are thus seen to be complementary and to lie on a single continuum divided by a neutral zone, wherein there is neither contrast nor assimilation. However, when background reflectance is near line reflectance, e.g., white lines on near-white background, or black lines on near-black background, there is no contrast, only *varying degrees of assimilation with all widths of lines and intervening areas* as seen from Figs. 8-2 and 8-3.

A simple physiological model, based on generally accepted principles, seems to account for both contrast and assimilation as follows: we assume that small differentials in stimulation summate and large differentials result in inhibition of weaker by stronger neural impulses. On this basis assimilation occurs when neural impulses facilitate, and contrast occurs when they inhibit each other in neighboring areas. This model explains the absence of contrast, when the lines and backgrounds have nearly the same reflectances, and also the stronger assimilation effects found with narrower lines and smaller intervening background areas. To explain the predominance of contrast effect with wide stripes or intervening areas, we need only assume that within certain limits area acts like luminance, i.e., increase in area has the same effect as increase in luminance. In line with this assumption, assimilation should be replaced by contrast when the width of the lines increases relative to that of the intervening areas, a fact verified in Fig. 8-4. Reading vertically, it is seen that assimilation decreases and contrast increases as the intervening gray area increases for any given width of line. One final deduction from our physiological model has been verified experimentally: if large differentials in reflectance produce contrast and small differentials produce assimilation, there should be some intermediate differential that produces neither contrast nor assimilation and this is exactly what is found with lines in the neighborhood of 10 mm on gray background as seen in Fig. 8-4.

Having seen that adaptation, contrast, assimilation, and constancy are interrelated we have achieved an economy in thinking about these four phenomena. A fifth phenomenon, figural aftereffects (FAE), must also be regarded as related to the better-known phenomena of adaptation and contrast rather than as a phenomenon *sui generis.* FAE appears when figures are fixated and then compared with identical stimuli falling on neighboring retinal areas. Thus if a tilted line is fixated, a straight line appears tilted in the opposite direction (Gibson); if a square falling on the left of the fixation point is fixated for 20-40 sec, it is perceived to be smaller than an identical figure exposed on the right of the fixation point. In general, stimuli in the vicinity of previously fixated figures are repelled from the adapted region. Thus a figure exposed inside the contours of a fixated figure appears smaller, while if exposed outside the fixated figure it appears larger, than identical figures on unexposed retinal areas (Köhler and Wallach, 1944).

The main figural aftereffects are so similar to older, better-known adaptation and contrast effects that it is hard to see why they have been regarded as a special class of phenomena. The repulsion of figures from adapted areas is analogous to the effects of anchors and backgrounds in phenomena of adaptation and contrast (cf. Helson and Nash, 1960). Perhaps the most closely related phenomenon to FAE is the rotating spiral, wherein a black spiral figure appears to expand during rotation and to contract when the disc stops moving. While it has been claimed that certain aspects of FAE are different from adaptation and contrast phenomena, no satisfactory theory has yet been devised to account for them as special phenomena. Viewed as cases of adaptation

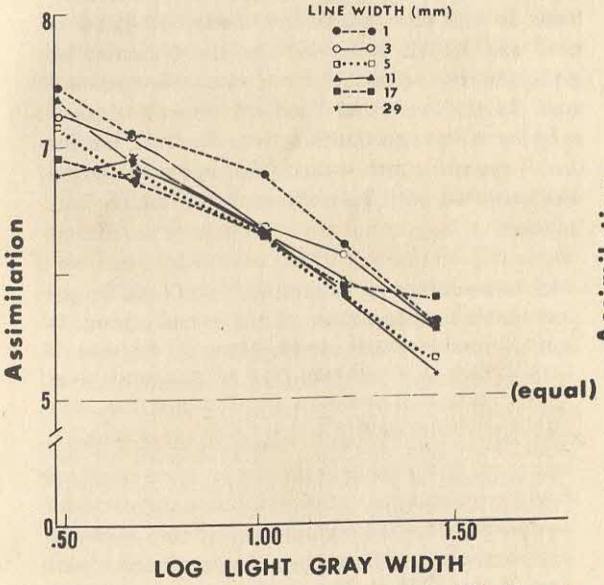


Fig. 8-2. Assimilation as a function of white line separations. The light gray background has 80 per cent reflectance. Only assimilation is found with all line widths, when the reflectances of lines and background are nearly the same. (From Helson, 1963.)

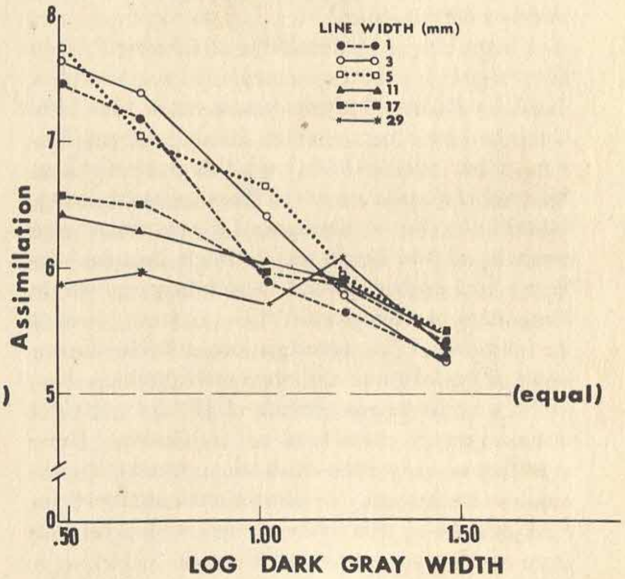


Fig. 8-3. Assimilation as a function of black line separations with background of 14 per cent reflectance. Only assimilation is found with all line widths, when the reflectances of lines and background are nearly the same. (From Helson, 1963.)

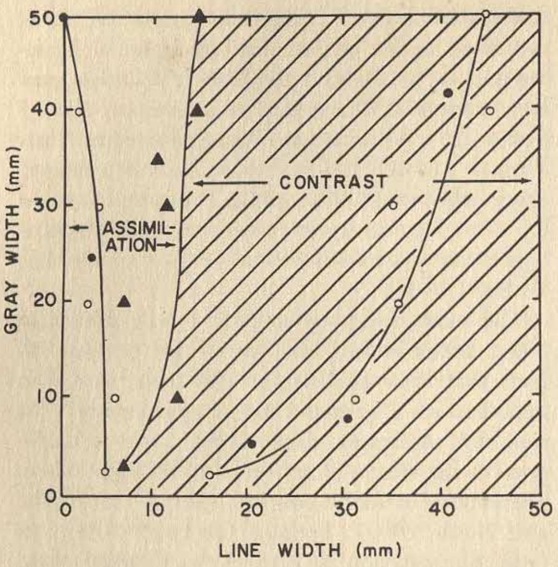


Fig. 8-4. Domains of assimilation and contrast. Each point in the graph represents the combination of line widths and separations that yield contrast or assimilation. The triangles represent conditions that yield neither contrast nor assimilation. (From Helson and Joy, 1962. With the permission of *Psychologische Beiträge.*)

with negative aftereffect FAE is brought within the much wider context of more general, better understood principles.

A beginning in the direction of relating FAE to adaptation-contrast experimentally was made in 1952 by Elaine Graham, whose work was later confirmed by others cited in the delayed publication of her results (1961). In this study Graham found that displacements of lines following adaptation to the inspection figure (IF) increased as a function of the brightness contrast between the figure and its background. Absolute variations in luminance of background with contrast constant do not affect FAE. Investigations of other dimensions of test figures and their backgrounds, e.g., width or area versus contour of IF, did not yield univocal results as pointed out by Graham. However there can be little doubt that systematic, parametric studies of the dimensional attributes of FAE will bring this phenomenon within the domain of adaptation-contrast effects.

More recently, Deutsch (1964) has argued that FAE is produced by the mechanism responsible for simultaneous and successive contrast, and he cites neurophysiological evidence of inhibitory interactions of adjacent cells to account for contrast and thereby for FAE. However, Deutsch's assumption of interaction of *adjacent* areas is open to the same objections as the Köhler-Wallach electrotonic theory, which cannot account for FAE when heteromodal phenomena are in question. Jaffe (1956) found that if the fingers are run along two raised diverging strips of aluminum and then along parallel strips, the latter appear to converge. If Ss view a strip of white paper on black background of the same width as the standard, parallel strips, there is no effect of vision on kinesthetic FAE, but if the visual stimulus is wider or narrower than the tactile standard, the latter shows a contrast effect. Hence concurrent visual stimulation can induce significant kinesthetic FAE. Jaffe maintained that the electrotonic model, couched in terms of specific brain areas, could not account for heteromodal interactions, because these would require a greater spread of currents in the brain than seems plausible. A similar argument against the electrotonic model has also been advanced by Moylan (1964), who found that if KFAE was tested not by using the same fingers and hand for the I and T conditions giving contrast, but one hand for adaptation (I) and the other hand for testing (T) on parallel bars, a T-bar narrower than the I-bar was judged wider rather than narrower, hence reversed contrast. In the case where a nonadapted hand is used and KFAE is found, the effect cannot be explained by reference to a cortically satiated area. Moylan refers to "general contrast theory" as better able to account for these findings, but his use of general contrast theory is vague. He seems unacquainted with Jaffe's formulation which is as follows:

> In terms of . . . 'adaptation level' one might assume that the action of the visual stimuli is not limited to visual [areas] alone. These stimuli contribute to a common pool of past and present effects of stimulation and thereby may produce alterations in all modalities . . . adaptation level does provide a potential basis for the explanation of intermodal influences in perception. This broader application is possible since a specific physiological mechanism has not been invoked that is limited to a single sensory process (Jaffe, 1956, p. 75).

The adaptation-level paradigm allows for pooling or interaction not only between adjacent sensory elements but also for interactions between cortical areas presumably as far apart as those of the two hands in Moylan's variation of KFAE.

ATTENTION, FIGURE GROUND, AND DISTINCTIVENESS

Almost every system of psychology has had to grapple with the fact that among the stimuli bombarding the organism through every avenue of sense, some stand out in perception and elicit overt or covert responses while the rest serve only as background or filling material. In general, transient stimuli become focal, while steady-state stimuli tend to be relegated to marginal status unless very intense. While changes in the environment provide many of the phasic stimuli to which we attend, most of the variation in stimulation comes from our own activities as we move about, turn our heads and eyes, make contact with various objects, listen to various sounds, sniff at odors, and so on. Similar considerations apply to emotional and cognitive states, which are influenced by what we are perceiving from moment to moment. The role of attention, largely forgotten in later learning theories, was recognized at the beginning of the century by Thorndike (1898), who noted that at the beginning of confinement in puzzle boxes animals reacted to any and every part of the enclosure, and only after some learning occurred did the animal shorten the time it took to obtain release from the box by concentrating its efforts (focalizing its responses) on the means of release. It is now recognized that a large part of the time and effort spent in learning is devoted to finding out *what* must be responded to in the learning situation.

In Wundt's system *Klarheit* and *Deutlichkeit* were used to characterize contents of consciousness and, influenced by these terms, Titchener first maintained that sensory processes are clear only when in the center of attention, the rest being unclear. Later clearness gave way to attensity as an attribute of sensation to make the list of attributes five in all—attensity, quality, intensity, extensity, and protensity (duration). The doctrine of clearness or attensity was designed to render a descriptive account of attention in keeping with Titchener's position (following Wundt) that science only describes and does not use concepts denoting activities or meanings (cf. Boring, 1942). Originally there were only two levels, clear and unclear, but later it was granted that there may be several grades of clearness between the clear and unclear.

The Gestalt psychologists objected to the clearness account of attention because it essentially involved the constancy hypothesis: the perceptual object is implicitly assumed to be the same, only changing its clearness value with changes in attention (cf. Helson, 1925, 356ff.). Thus the famous brain–babies figure which is first perceived as a convoluted, side view of a cerebral hemisphere, is later seen as a mass of intertwining babies. Titchener (1909) maintained that when the brain was seen the babies were unclear and when the babies were seen, the brain was unclear. Other illusions were accounted for in the same way. The configurationists, on the other hand, maintained that there were two different phenomenal objects—brain in the one case and babies in the other. The description in terms of clearness, they felt, did not square with what was actually perceived. In place of changes of clearness, the configurationists stress the emergence of new structures, new patterns with changes in set, attitude, or with changes in attention.

The work of Rubin (1921) on figure-ground phenomena fitted so well with conclusions reached by the configurationists that they incorporated it into their system of psychology. Figure and ground are the two fields into which all perceptions divide. Qualities emerge from a general level, are figured, and predominate in perception, while ground sets off the figured, differentiated parts of the field. Illusions of reversible perspective show how new phenomenal objects arise as one part of the field is figure, the other ground; when the field reverses so that what was figure becomes ground and what was ground becomes figure, new objects are perceived which may not have existed at all (in perception) before. Figure and ground differ not only in phenomenological properties, e.g., figure is harder, more thing-like, better formed, and more solid than is ground, but they also differ functionally: the limen for color induction is higher in the figure than in the ground and once a part of the field is perceived as figure, it tends to be remembered and perceived again as figure. Since the simplest quality is perceived against a ground all experience is figural in character (Koffka, 1925).

The introspective and configurationists' accounts of attention are alike in being purely descriptive. Not until recently (Nachmias, 1958; Murdock, 1960; Helson, 1964) has the concept of attention as clearness or vividness been reformulated in a quantitative way. Replacing clearness and vividness the term "distinctiveness" has been employed by Nachmias and by Murdock as difference from level and this proves to be capable of quantitative definition and treatment. Whereas Nachmias and the present writer define distinctiveness in terms of difference from level, stimuli coinciding with level being least distinctive, Murdock uses a somewhat similar definition by defining distinctiveness as the sum of the differences in logs of any items of a group from all the others. The per cent difference of any item would then be its total difference divided by the sum of the total differences of all items in the group. This definition yields maximal distinctiveness at the ends of a serially ordered set of items and minimal distinctiveness in the middle. The D-scale is not symmetrical even when all the items have equal intensities or are simply rank ordered (as in the case of a set of nonsense syllables) because of the log function, items at the beginning of a series get higher distinctiveness ratings than do items equally far from the center at the ends of the series.

The concept of distinctiveness has the advantage of being able to account for the distinctiveness of weak stimuli as well as strong stimuli, since it depends upon distance from level which is an average function of all the stimuli. Thus Nachmias found that low-frequency words along with high-frequency words according to the Thorndike-Lorge word count were remembered better than words of average frequency, even though they were less familiar. The concept of distinctiveness can also be used to interpret contrast effects in reinforcement, thus bringing perception back into learning theory where it has been neglected for many years.

A THEORY OF PERCEPTION

A theory of perception must take into account the fundamental fact that perception, no less than other types of response, is an adjustive process contributing to the adaptation of the organism to its environment. The adjustive character of perceptual processes manifests itself in a number of ways. For example, in any viewing situation some stimuli are very bright, some very dim, and others are medium or neutral; similarly some sounds are loud, some soft, and others are medium. Moreover, a stimulus that is very bright in one situation may be dim in another, and sounds that are soft in one situation may be heard as loud in another. Often changes in perceived intensity or magnitude are accompanied by changes in quality: a bright neutral light in a red surround turns to blue-green if its luminance is sufficiently decreased, and a positive afterimage on a black background turns to its complementary on a white background. Thus both magnitude and quality of sensory processes depend upon the level of adaptation which prevails from moment to moment. To be operational the concept of adaptation level must be strictly defined in quantitative terms if possible. The results of many experiments, including determinations of actually reported medium or neutral judgments of stimuli, point toward a weighted log mean as the best and most general quantitative approximation to adaptation level (Helson, 1964), although other definitions may be preferable in certain cases. Stimuli above AL have a given quality, stimuli below have the complementary quality, and stimuli near AL are neutral or of uncertain quality. In defining AL as a weighted mean we also assume that it is the pooled effect of all stimuli, inner as well as outer, acting on the organism.

It is evident that AL serves as the origin or zero of organic function. Since the value of AL is always positive, bipolar qualities and responses arise as positive or negative gradients from level. Afterimages, contrast, and adaptation effects can thus be positively accounted for without recourse to negative sensations, or higher level judgmental, illusory, or imaginative processes. Since the concept of pooling is central to AL theory, we shall consider a number of critical problems in the next section which is devoted to a discussion of various phenomena and consequences of the pooling model.

PHENOMENA AND CONSEQUENCES OF THE POOLING MECHANISM

The organization, patterning, and complexity so characteristic of perceptual processes arise from interactions of the simplest, basic dimensions and extend to the most complex formations. The dependence of absolute and differential thresholds on interactions of space, time, and intensity of stimulation have long been formalized in such laws as the law of photographic reciprocity ($It = k$, where the symbols stand respectively for intensity, time, and a constant) and Ricco's law ($AI = k$, where A stands for area and the other symbols have the same meanings as before). That these laws depend upon pooling processes is almost self-evident, for they express the fact that intensity or area may function for time or vice versa. There are many other such cases, e.g., the Tau effect (Helson and King, 1931), in which perceived spatial extents between three points stimulated successively on the skin depend upon the time intervals, and the kappa effect (Abe, 1935; Cohen, Hansel, and Sylvester, 1953), in which perceived temporal intervals between points stimulated successively depend upon the distances. In all these cases pooling occurs among basic dimensions of perception. The pooling manifested at absolute thresholds and in perception of temporal and spatial intervals must be ascribed to the neurophysiological mechanisms underlying sensory processes.

Pooling occurs also over more extended temporal intervals as shown by Bevan and Darby (1955) in their study of the reciprocity between frequency and magnitude of stimulation with lifted weights. Noting the reciprocity between frequency and magnitude implied by the weighted log mean definition of adaptation level (Helson, 1964), these workers determined the number of presentations required by a wide range of anchors with a constant core of series stimuli to yield a given value of adaptation level (AL), i.e., the value of stimulus judged to be medium. They found very good agreement between theory and experimental data when anchors were varied in weight and frequency of presentation over a wide range. Thus an anchor of 314.4 g presented 200 times for each presentation of the series stimuli yielded the same AL as an anchor of 1,010 g presented 1.16 times for each presentation of the same core stimuli. These results are significant in showing that it is possible to have the same AL under varying conditions of stimulation because of the pooling of frequency and magnitude of successively presented stimuli.

Pooling is also found among spatial attributes of stimulation. Often spatial properties of objects appear to be so stable as to be static and independent of their surroundings. However, constancies and illusions of form, size, and position have been shown to depend upon interactions with background or contextual stimuli which pool with focal stimuli to influence what is perceived. Thus Nellis (1958) found that shape constancy was highest when ellipses were viewed against normally oriented backgrounds, i.e., at right angles to the line of regard. Constancy decreased as *background* was turned more and more out of the frontal-parallel plane. Hermans (1954) showed that as less and less of the field surrounding an object is visible by reducing vision from binocular to monocular to pinhole viewing apparent size of the object was reduced. With reduction of the visual field there are other changes that affect perceived size, e.g., changes in accommodation and convergence, in the amount of light entering the eye, and in size of the pupil. Reduction in apparent size of objects amounting to 33 per cent results when accommodation is maximal (near fixation) as compared with minimal accommodation (far fixation) as found by the writer (Helson, 1935). Similarly apparent size was shown to vary with changes in luminance by comparing objects in free vision with the same objects viewed through pinholes which excluded all light except that from the focal stimulus.

A test in reverse can be made of the influence of surroundings on apparent size as follows: according to Emmert's law the size of the afterimage is proportional to the distance at which it is projected. In other words, nearer fixation results in smaller, farther fixation in larger, perceived size of the afterimage in free viewing. But if the afterimage is viewed through a tube having a diameter only slightly larger than the image (Helson, 1936), the majority of the reports of perceived size are either equal or only slightly larger when projection distances are 2, 5, and 6 times the original distance of fixation of the stimulus. Emmert's law did not apply, under these conditions, to the *perceived size* of the afterimage but the *measured size* did obey this law. This striking departure of apparent from measured size shows that the perceived size depends largely on the field conditions of viewing. The failure of the afterimage to increase in perceived size with increasing distance of the plane of projection in tubular vision shows that for Emmert's law to hold the image must be perceived in normal perspective relations. That normal perspective was not present appears from the protocols of the Ss who reported that the afterimage was always at the end of the tube, not at the actual distance of projection. It is also probable that accommodation was not relaxed in tube viewing, and this may also have contributed to the failure of the image to increase in apparent size with increasing distance of projection since, as pointed out above, objects appear smaller with near fixation than with far fixation. It is thus seen that normal perspective, the presence of other objects, and relaxation of accommodation are necessary for Emmert's law to be validated in perception.

Cross-modal pooling also occurs though it is usually less intimate than intramodal interactions except for the phenomenon known as synesthesia, where sensory processes in one modality fuse with those of another. The fusion is so complete that Ss use such expressions as "colored hearing" to describe their experiences. The pattern in Fig. 8-5 shows how one synesthetic individual, a young man in his twenties, perceived a portion of an orchestral composition.[2] He reported, in connection with this picture of his experience: "I made this snapshot while listening very attentively to an orchestra . . . while there was constant action in what I 'saw,' it was in this place (represented by the figure) that it slowed up enough for me to visualize it clearly and to remember the motifs at play. After making a pencil sketch of the arrangement of the various parts of the picture I later transcribed it in wash drawing to give it the texture it seemed to have. In this particular I have failed for I was unable to give it any luminosity or transparency that seemed to be part of it." He then went on to say: "On analyzing it you will

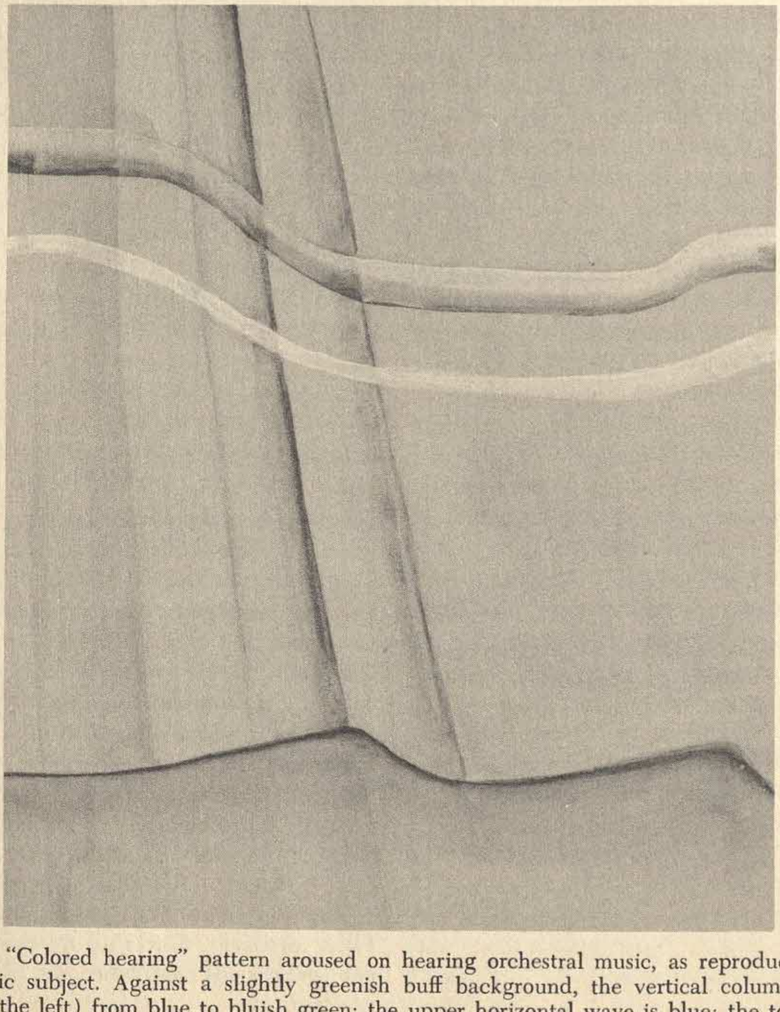


Fig. 8-5. "Colored hearing" pattern aroused on hearing orchestral music, as reproduced by a synesthetic subject. Against a slightly greenish buff background, the vertical columns range (toward the left) from blue to bluish green; the upper horizontal wave is blue; the top of the heavy lower wave is darker blue.

---

[2] I am indebted to Mrs. Ruth Hilton Burgert for this description and the figure which was furnished by her brother who was the synesthetic observer.

notice what I believe to be the melody and the counter-melody (the two horizontal 'waves' in the upper part of the figure) . . . The dominating volume of heavy instruments seem to form a background and there, too, you find them in harmony or parallel (the vertical columns). The heavy 'wave' at the base must be the rhythm . . . I was surprised to discover purely from analysis that there were three separate parts to what I saw: the melody, the associated melody, and then the background or base notes. Lastly there was a distinctly removed thing (the rhythm). Action was portrayed by the melody and the rhythm. A lack of action seems to be portrayed in the background."

In a study of the influence of heteromodal anchors, Bevan and Pritchard (1964) found that *visual* anchor stimuli caused a downward shift in judgments of the *loudness* of tones when the former were sufficiently intense. But visual stimuli, matched in intensity with the tonal stimuli, *functioned* as weaker anchors, raising judgments of loudness instead of lowering them. Such effects may be due to inherent differences in intensity among the different sense modalities, failure of Ss to make good heteromodal matches, or there may be some loss in effective intensity when heteromodal comparisons are made. That there are inherent temporal differences among the sense modalities has been shown in judgments of auditory and visual durations by Behar and Bevan (1961) and by Goldstone and Goldfarb (1964). These workers found that when auditory and visual stimuli are of equal duration, the former are judged longer than the latter.

Some of the most important heteromodal interactions are found in spatial perception. Roelofs (1960) has made out a very strong case for the influence of kinesthetic input from eye movements on depth perception and localization in the visual field as shown in the following quotation:

> The fundamental basis of optic localization is to be found in the adjustment impulses, both for the eyes and for the head. It is the tensions between the various adjustment impulses which constitute the physiological correlate of optic localization. Limiting ourselves to the impulses to ocular movements, we find that it is the tension between impulses to lateroversion which informs us as to what object is localized more to the left and what more to the right; it is the tension between impulses to sursumversion and those to deorsumversion that shows us which is higher and which is lower; it is the tension between impulses to convergence and those to divergence which teaches us which is nearer and which farther away. In the case of exocentral or relative localization we encounter tensions between adjustment impulses arising from various objects in the environment. In the case of egocentral or absolute localization we are confronted with tensions between the reflex gaze tonus and the adjustment impulses provided by the objects seen (1960, p. 214ff.).

Although we may grant that input from the muscles of the eyes and head are correlated with perception of relative position, distance, and depth of visual stimuli, the question how tactile-kinesthetic impulses are translated into visual data remains. While the ultimate solution must eventually be sought in neurophysiological terms, for purposes of further behavioral investigations it may be necessary to abandon the classical belief in a self-contained, self-determined visual space. Except for a primitive visual expanse as suggested by the reports of individuals who have acquired sight for the first time in adult life, there is probably no purely visual perception of depth, distance, direction, or position.

Pooling is also exemplified in masking, inhibition, or suppression of data in one sense modality by those from another as in the case of suppression of pain by sound (Gardner, Licklider, and Weisz, 1960). These writers found that certain types of pain may be reduced or abolished by intense acoustic stimulation. They report that in 90 per cent of 5000 "operations" (dental procedures?) sound stimulation was the only analgesic required. The greater the pain, the higher the intensity required to reduce the pain. In explanation of this effect they point out that parts of the auditory and pain systems come together in several regions of the reticular formation and lower thalamus. Even reduction of pain through relaxation, decreased anxiety, and diversion of attention can, they believe, be explained by assuming that acoustic stimulation reduces the gain of pain relays upon which branches of the auditory nerve impinge.

So far the discussion of pooling has been concerned with interactions of sensory data, but pooling is not confined to interaction of impulses from peripheral systems only. Pooling also occurs between cognitive and affective systems on the one hand and sensory systems on the other. The influence of needs and other inner factors on perception was demonstrated by the New Look psychologists experimentally (cf. the review of this movement by the writer, 1953). Effects of instructions on what is perceived argue for cognitive participation in perceptual processes, as shown in a study by Bevan, Maier, and Helson (1963). Four groups of seventy-five Ss were asked to judge the number of beans in either a small or a large glass container under "figure" or "ground" instructions. Under figure instructions Ss were told to regard the beans and their containers "as a single organic unit set off from their surroundings," while under ground instructions Ss were told to "remember they are separate. The jar is a container . . . and can be thought of as part of the context or background, just as in this picture of the (Rubin) goblet . . ." (p. 465). There were three jars containing 60, 230, and 410 beans. It was hypothesized that, if the instructions succeeded in making the jars focal with their contents, then the largest and smallest estimates should be with the large jar as figure and the small jar as figure, respectively, while the estimates with large jar as ground and small jar as ground should fall in between the other two in that order. The results plotted in Fig. 8-6 show that these hypotheses were verified. An unexpected finding was that all the estimates were below the actual number of beans under all conditions. That jar size exerted a greater influence under the figural instructions than under ground instructions supports the assumption that focal stimuli have greater weight in perception and judgment than do background and residual stimuli.

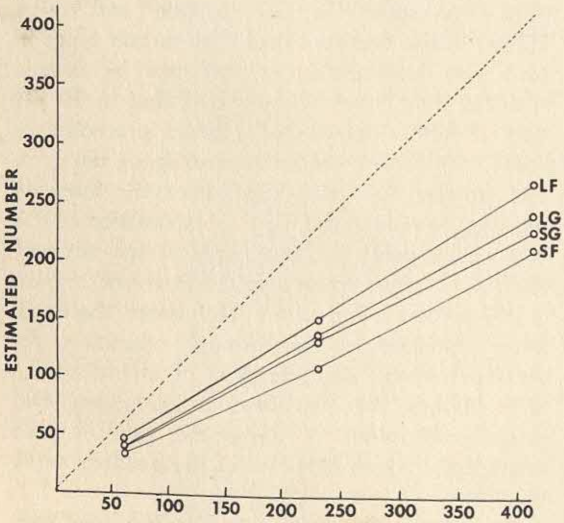


Fig. 8-6. Direct estimates of number as a function of jar-size and the way in which the jars are perceived. L and S indicate large and small jars, F and G indicate figure and ground instructions. The dotted line represents perfect estimates. (From Bevan, Maier, and Helson, 1963. With the permission of the *American Journal of Psychology.*)

We have so far considered interactions of focal-on-focal and contextual-on-focal stimuli. Can it be shown *experimentally* that residual stimuli from previous experience can affect perceived attributes of stimulation? Among the most important and difficult conditions to control in psychophysical and sensory investigations are those in which preceding stimuli influence succeeding ones. In the experiment referred to above by Behar and Bevan (1961) on judgments of duration, the results for intramodal (visual) anchors clearly show residual effects of preceding stimuli. Durations of 1, 2, 3, 4, and 5 sec were judged with anchors of 0.2 and 9.0 sec duration. The order of anchor presentations was counterbalanced, some receiving the control condition first, others the short anchor first, and the rest the long anchor first, and so on. In Fig. 8-7 the results are plotted across all anchors (left panel) and for the condition given first (right panel). It is seen that each anchor is more effective when presented first. An analysis of variance based on extraction of sums of squares for Ss X conditions and a sequence X conditions interaction yielded highly significant Fs showing that there was a perseveration of one condition into the next.

Many psychologists have used "past experience" as an indiscriminate principle in explaining many perceptual phenomena, including individual differences in perception. That this principle cannot be safely invoked for all cases, especially where stimulus conditions are so compelling that residuals play little or no part in determining what is perceived, has been amply demonstrated by the Gestalt psychologists (Wertheimer, 1923; 1958). One of the latest studies to come to the writer's attention shows that residuals may be of importance with respect to one attribute or task in a given stimulus configuration and not to another. Robinson, Brown, and Hayes (1964) exposed critical letter patterns, geometrical figures, and representational patterns in pairs which were either in the same or in opposite orientations. One group of Ss had merely to report "same" or "different" and another group had to identify the two patterns which might be the same or different, e.g.,

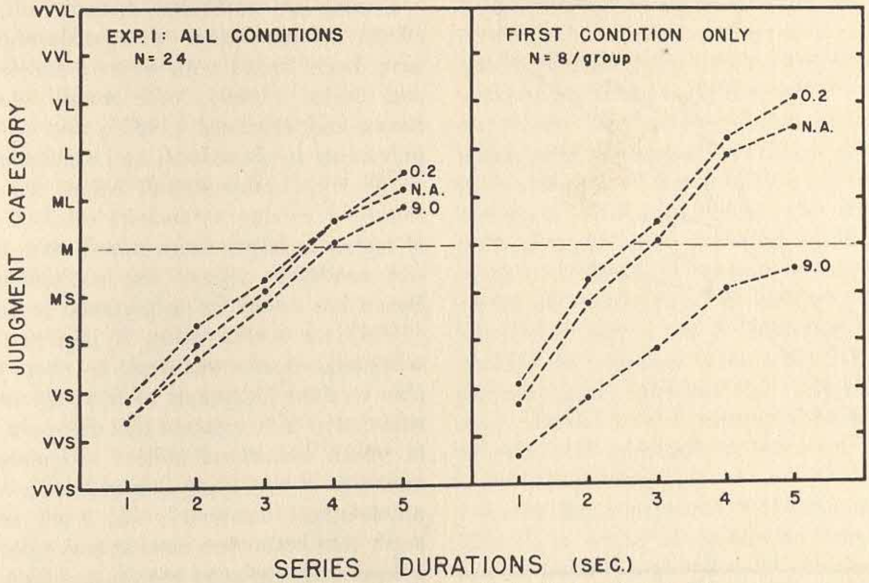


Fig. 8-7. Intramodal anchor effects in judgments of visual durations with anchors above and below the series intervals. The left panel confounds order effects; the right panel shows results for each condition presented first. (From Behar and Bevan, 1961. With the permission of the *American Journal of Psychology.*)

square and triangle, or sailboat and package of cigarettes. Time thresholds were determined by using tachistoscopic exposures in ascending order. Thresholds were higher for novel than for familiar objects so far as identification went, but not so far as same-different judgments were concerned. This was also true for letters differing in frequency of occurrence in English: identification thresholds were higher for the less frequent letters but the same-different thresholds were not significantly different. Thus the type of response or task demanded of Ss may determine whether or not residuals are of importance in perception.

It is frequently asserted that the manner of perceiving influences later behavior. It is well known that anchors exert predominant effects in perception. Do such anchor effects persist over time, i.e., do they persist as residuals to influence later perceptions? A study by Steger and the author (reported in Helson, 1964*a*) furnishes evidence for the potency of previous anchor effects in later judgments of size, as shown in the following experiments. In the first experiment four groups of subjects judged a series of black squares on white background during the impression or learning series. The squares ranged from 1.0 to 3.82 in. on a side, and they were judged in terms of a rating scale from very, very small to very, very large. The initial conditions for the four groups were as follows: (*a*) without anchor stimulus; (*b*) with an anchor at the geometric center of the series (1.96 in.); (*c*) with a 9.0-in. anchor; and (*d*) with a .30-in. anchor. The average size judgments are shown in Fig. 8-8 and are in line with expectations from known anchor effects: The squares were judged smallest with the largest anchor, largest with the smallest anchor, and intermediate with the anchor at the center of the series and with the no anchor condition. One week later subjects were called back to the laboratory and asked to identify the smallest, middle, and largest stimuli in the series from a much more extended series that ranged from .10 in. to 14.56 in. on a side presented in ascending and descending orders. As shown in Fig. 8-9, the residual effects of the anchors in the impression trials are evident after one week: The 9.0-in. anchor group picked the smallest set of three; the .30-in. group picked the largest set of three; while the two control groups picked sets intermediate in size between those of the other two groups.

The first experiment was replicated and extended in a second experiment with four new groups of subjects to determine the relative potency of anchors as residuals, i.e., anchors given during the impression phase, versus anchors pres-

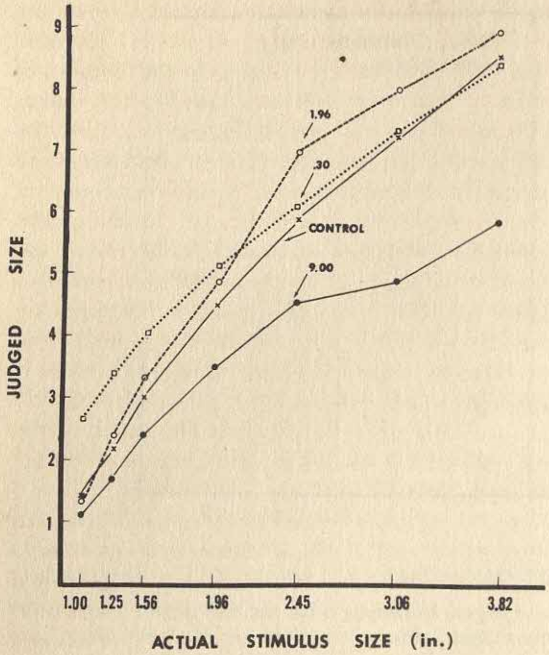


Fig. 8-8. Judged sizes of stimuli in the first (impression) phase showing expected anchor effects with small (0.30 in), intermediate (1.96 in), and large (9.0 in) anchors and under control condition in which only series stimuli were judged. (From Helson, 1964*a*. With the permission of the American Psychological Association.)

ent during the recall phase. Two control groups were not given anchors during the impression phase but were exposed to the .30- and 9.0-in anchors during the recall phase. One experimental group was given the 9.0-in. anchor during the impression phase and the .30-in. anchor during the recall phase; conversely, the other experimental group was given the .30-in. anchor in the impression session and the 9.0-in. anchor in the recall session. The results again show expected effects of anchors as seen in Fig. 8-10 where the stimuli recalled with the 9.0-in. anchor present were smaller (curves coded V and VIII in Fig. 8-10) than stimuli recalled with the .30-in. anchor (curves coded VI and VII in Fig. 8-10). Comparison of the curves for Groups VI and VII show that the .30-in. anchor during recall was more effective if it followed 9.0-in. anchor during the impression phase one week earlier than if no anchor was present during the impression phase. However the 9.0-in. anchor in the recall phase following .30-in. anchor in the impression phase did not yield significantly lower recall choices than under the control condition (Groups V and VIII).

Pooling of subliminal stimuli with resultant effects on perception of suprathreshold stimuli have been found with electric shock by Black and Bevan (1960), with sound intensities by Bevan and Pritchard (1963), and in visual size judgments by Boardman and Goldstone (1962). In all these cases stimuli below the awareness threshold serving as anchors resulted in reports of higher or larger series stimuli than in the control condition without the subliminal anchors. Bevan has noted the importance of such results (1964) for several areas of inquiry: (1) since subliminal shocks too weak to elicit GSR were able to raise judgments of intensity of electrical stimulation, it is apparent that there are "situations in which behavioral indices are more sensitive measures of mediating events than are the usual physiological measures"; (2) such results may imply that brain-stem mechanisms associated with arousal and vigilance are distinct from those related to anxiety and fear; (3) the absolute threshold cannot always be applied as a criterion in the identification of stimuli relevant for pooling nor as the limiting value for all sensory dimensions; this suggests, in turn, that sensory scales are not absolute but relative since the origin of the scale

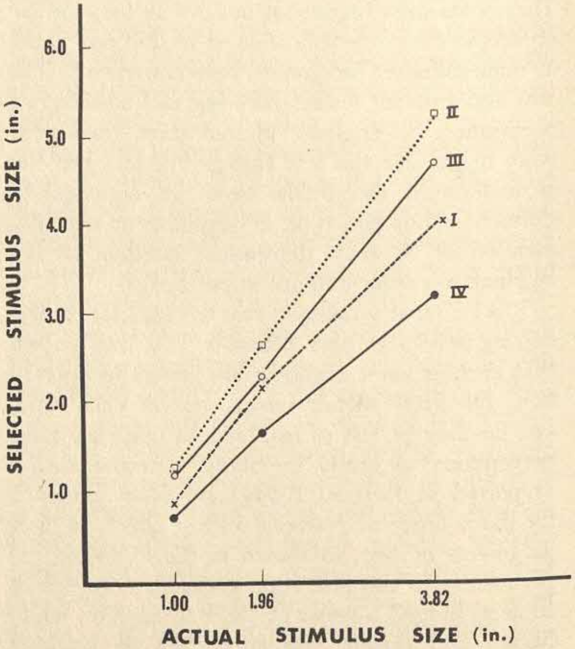


Fig. 8-9. Smallest, middle, and largest of the series stimuli recalled one week after initial presentation. (From Helson, 1964*a*. With the permission of the American Psychological Association.)

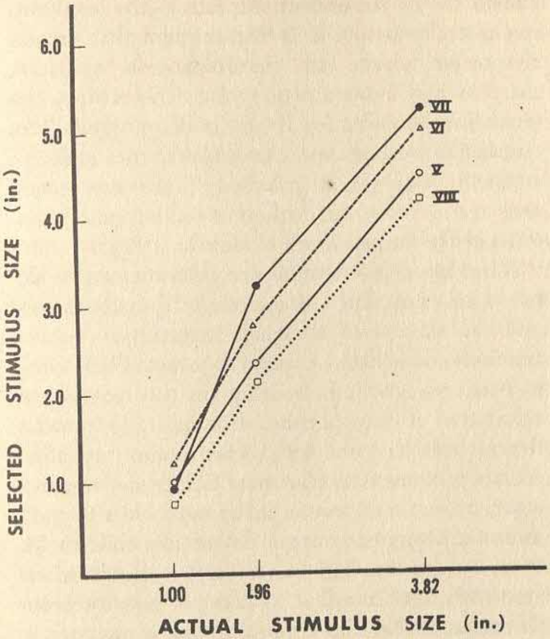


Fig. 8-10. Smallest, middle, and largest of the series stimuli recalled one week after initial presentation with large (small) anchor if small (large) anchor was employed during the impression phase. (From Helson, 1964*a*. With the permission of the American Psychological Association.)

is not a true zero; and (4) views of the way in which "unconscious" mechanisms influence behavior may need revision in so far as their role is similar rather than different from, as in psychoanalytic theory, supraliminal stimuli. In connection with the last point Bevan says: ". . . at least one device by which unconscious variables influence behavior is through their role in structuring the behavioral environment within which the behavior they influence occurs. Here their role is similar to that of supraliminal variables and the behavior, far from being irrational, (as in Freudian theory) is not only rational but appropriate to the circumstances as the behaving individual perceives them" (1964, p. 97).

The Problem of Relevance. In discussing pooling both within and across sense modalities it is obvious that not everything pools to form level and that there are limits to the pooling process. In general there is greater interaction among elements in the same modality than in different modalities, and distance in space and time is known to reduce interaction. In proposing the pooling of focal, background, and residual stimuli as the mechanism by which adaptation levels are established, interaction of these classes of stimuli was limited to those belonging to the "same universe of discourse" (Helson, 1964*b*). Later demonstrations of cross-modal interactions and the influence of subliminal stimuli on perception and judgment showed that pooling was even more general than I had supposed it to be originally. The pooling model may be used to determine the limits or absence of pooling, just as the normal curve of distribution is used in determining whether or not a die is loaded or normal. The problem of relevance is not a simple one, since it concerns many questions that may be asked. We shall consider two experiments illustrative of the experimental approach to problems of relevance in the field of vision.

In the first experiment, by Bevan and Pritchard (1963), a series of rectangles ranging from a square 5 × 5 cm to 4.4 × 5.6 cm was presented to Ss with instructions to report on departures from squareness using an anchor 4.0 × 6.0 cm. The results from several experimental groups given the rectangles in vertical, horizontal, and intermediate orientations all plotted below the control group without anchor, i.e., the rectangles were judged to be more square with anchor than without. When a circle having the same diameter as the side of the square and an ellipse with axes of 4.0 × 6.0 cm which were the same as the sides of the rectangle anchor were used, no anchor effect was found in judgments of the rectangles. Hence it was not the proportionality of the axes of the anchors, but their phenomenal likeness to the series (judged) stimuli that determined whether or not they were effective as anchors. The question then arose whether or not a stimulus must be not only the same shape but also the same size and color as the series stimuli in order to be effective (relevant) as anchor, and so rectangles one-fourth and four times the area of the original anchor and one having the same area but differing in color (gray instead of black) were introduced. Neither the small nor the large nor the differently colored stimuli exerted statistically significant anchor effects. Relevance thus is in large measure determined by dimensional similarity to the stimuli being judged.

Since several dimensions are usually present in psychophysical and perceptual situations, and since it may not always be easy or possible to vary one while holding the others constant, Turner and Bevan (1962) varied two of the three dimensions of size, shape, and color of visual stimuli, holding the third constant. Ss were required to judge all three dimensions, but anchors were introduced in only two of the three at any time. In other words, one group of Ss was given a series of rectangles with anchors in the shape and size dimensions but not in the color (gray) dimension, a second group was given anchors in the color and shape but not in the size dimension, and a third group was given anchors in the color and size but not in the shape dimension. Each group could thus provide control data for the dimension that was uninfluenced by anchors. The results clearly show that when anchors are present in two dimensions, these dimensions show typical anchor effects, while the third dimension, uninfluenced by anchors, show only the usual series types of judgments. Moreover, when high anchors (above series stimuli as in the case of size and shape) and low anchors (below series stimuli as in the case of color) are simultaneously presented, the response curves should be farthest from the control curves at the upper end in the former case and at the lower end in the latter case, and this was actually found. It is thus evident that several adaptation levels are simultaneously operative more or less independently of one another when multidimensional stimuli are being judged. This conclusion has also received support in a study of aesthetic responses to paintings that were judged with respect to several different dimensions (Mouton and Helson, reported in Helson, 1964).

Whether or not stimuli are relevant can be determined from their effects on adaptation levels, and the degree of relevance can thus be quantitatively specified. A paradoxical anchor effect in lifted weights has bearing on the question of relevance, or the question of when and to what degree stimuli pool with other stimuli to affect AL. It is known that anchors below the mean of a series of stimuli cause an upward shift in judgments of weights, hence a downward shift in AL. It is also known that AL for series stimuli alone, i.e., with anchor equal to zero, is usually below the mean of the series stimuli. Now if anchors are made lighter and lighter, the value of AL should decrease, but at some value there should be an

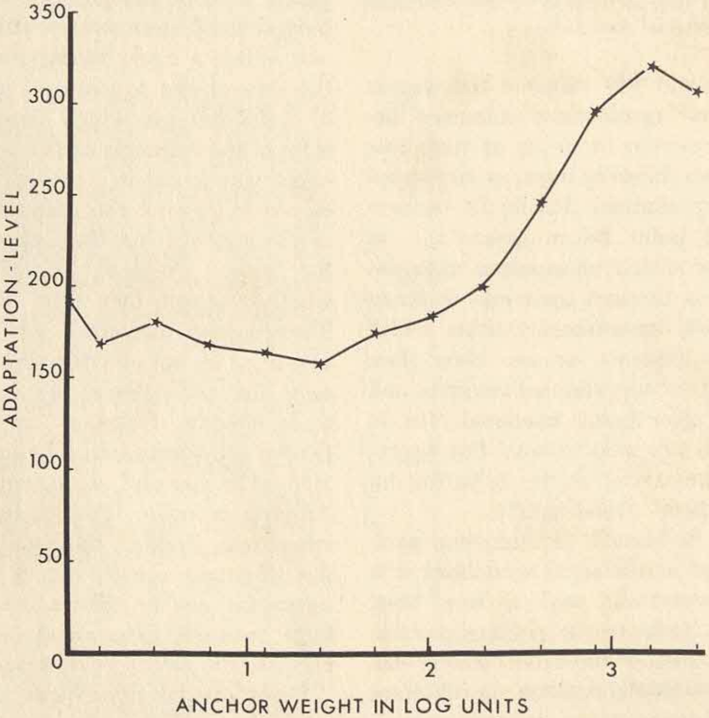


Fig. 8-11. Locus of ALs as a function of extremely light to extremely heavy anchors. Below 25 g anchors have less and less effect, as the ALs approach the AL of the series or no anchor condition. (From an unpublished study by Helson and Masters.)

inflection point and still lighter anchors should have less and less effect on AL, as their value approaches zero because AL is reverting to the series AL. This deduction was tested in an unpublished experiment (Helson and Masters). Weights of 100, 150, 200, 250, and 300 g were employed as series stimuli, and weights of 200, 100, 50, 25, 12.5, 6.25, 3.12, and 1.56 g were employed as anchors in a randomized design. AL with no anchor was found to be 193 g, and lighter anchors resulted in lower values of AL until AL was minimal (159 g) with an anchor of 25 g (cf. Fig. 8-11). Below 25 g the anchors exerted less and less influence, so that with an anchor of 1.56 g the value of AL was practically the same as the series AL (171 g). The paradox of lighter anchors raising the value of AL after a certain point disappears when we remember that the series AL is higher than AL with anchors below the mean of the series. The experimental data fully supported the deductions from theory, and indeed the theory made the experiment unnecessary except that the exact value of the inflection point had to be determined empirically. This result also tells us that, if subliminal stimuli that lower AL are carried sufficiently far below absolute threshold, their effect on AL will be less and less until the series AL is approached. Studies are now under way to investigate inflection points with subliminal anchors using sound, electric shock, and visual stimuli.

Are there inflection points in the effectiveness of anchors *above* series stimuli? This question was also investigated in the study just discussed. In the case of lifted weights it is reasonable to assume, when anchors become so heavy vis-a-vis series stimuli as to require different sets of muscles and stances in lifting, that they will exert less effect on AL. Reference to Fig. 8-11 shows that the highest AL of 323 g was reached with an anchor of 1600 g. But with the heaviest anchor of 2879 g AL dropped to 309 g. Whether 1600 g is a true inflection point at the upper end of the scale and the drop in AL with 2879-g anchors is real remains to be determined experimentally. In the case of light and sound it is doubtful if there are inflection points with anchors above the series stimuli, unless there are different modes of seeing and hearing with extremely intense stimuli than with stimuli of moderate intensity.

We thus see that whether or not stimuli are relevant depends upon the extent to which they pool with other stimuli to affect ALs. In order to know the value of AL operative in any given situation it is necessary that Ss be exposed sufficiently to stimuli in order to become more or less adapted to them. AL effects appear most clearly when different sets of conditions can be compared. An experiment by Helson and Bevan (1964) bears on these points. Ss were required to estimate the areas of central black areas relative to their white margins with materials simulating printed pages, since it is known that amount of printed material on a page is usually greatly overestimated, as pointed out by Paterson and Tinker (1938). In our study total field size (black plus white areas) did not prove to be a significant variable when each group judged with only a single field size. But when Ss judged constant black central areas against different overall field areas, errors of overestimation were found to be inversely related to total field area, the largest errors occurring with the smallest backgrounds. Errors of overestimation were largest for all field areas when the central black area was 53 per cent of the total area, next when it was 81 per cent, and least when it was 21 per cent of the total area as seen

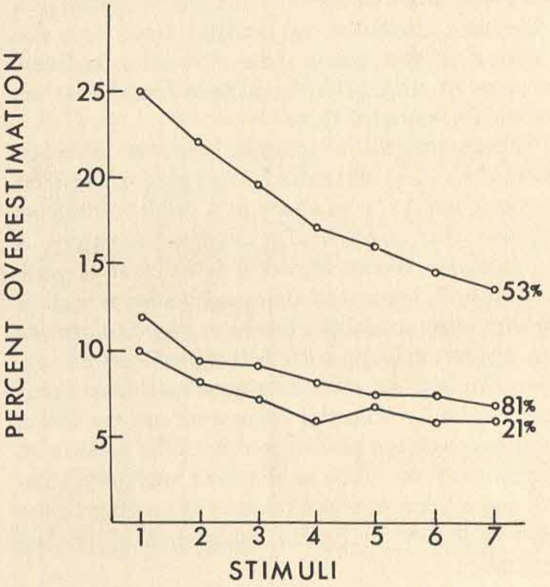


Fig. 8-12. Error of overestimation with judged area held constant, while size of background area is varied. (Background area was increased from Stimulus 1 to Stimulus 7.) The parameters of the curves indicate relative size of the areas judged. (From Helson and Bevan, 1964. With the permission of the American Psychological Association.)

in Fig. 8-12. From these findings we conclude that in order for a variable to make itself manifest it must vary within the experience of each S. In other words, if the role of many background or contextual factors is to be identified, experimental designs must be employed in which stimulus variables are manipulated *within* rather than *across* groups (Helson and Bevan, 1964, pp. 340-341). If adaptation effects are very pronounced they can be demonstrated across groups, but it is unsafe to conclude that level is not affected by a variable unless a within-groups design has shown this to be the case.

SUMMARY

We conclude this chapter by noting that perception now embraces phenomena ranging from simple sensory processes to complex, patterned formations having cognitive and affective components. Modern advances in neurophysiology have shown that perception involves more than central elaboration of afferent impulses terminating in the brain; rather, it is the product of afferent-central-efferent interactions, in which centrifugal as well as centripetal impulses determine what is focal and what is background or entirely excluded from notice. On the phenomenological side, new attributes and qualities have been discovered in the various sense modalities, and new sources of stimulation have been found that account for many of them.

Phenomena of adaptation, contrast, assimilation, constancy, and satiation or figural aftereffects were shown to be products of a single underlying process that is essentially adaptive in nature. A quantitative theory involving interaction or pooling in both space and time and across as well as within sense modalities has been found to provide an operational approach to problems of perception. Among the more important problems being investigated within this framework are the extent and limits of the pooling process. The solution to many problems, such as effects of anchoring stimuli and what is or is not relevant in judging classes of stimuli lies in further exploration of pooling processes.

REFERENCES

ABE, A. Experimental study on the co-relation between time and space. *Tohoku Psychologica Folia,* 1935, **3**, 53-68.

AFTANAS, M., AND ZUBEK, J. P. Long-term aftereffects following isolation of a circumscribed area of the skin. *Percept. mot. Skills,* 1963, **17**, 867-870.

———, AND ———. Interlimb transfer of changes in tactual acuity following occlusion of a circumscribed area of the skin. *Percept. mot. Skills,* 1964, **18**, 437-442.

ARNHEIM, R. *Art and Visual Perception.* Berkeley: University of California Press, 1954.

ARNOULT, M. D. Prediction of perceptual responses from structural characteristics of the stimulus. *Percept. mot. Skills,* 1960, **11**, 261-268.

ATTNEAVE, F. Physical determinants of judged complexity of shapes. *J. exp. Psychol.,* 1957, **53**, 221-227.

———, AND ARNOULT, M. D. The quantitative study of shape and pattern perception. *Psychol. Bull.,* 1956, **53**, 452-471.

BEHAR, I., AND BEVAN, W. The perceived duration of auditory and visual intervals: cross-modal comparison and interaction. *Amer. J. Psychol.,* 1961, **74**, 17-26.

BEVAN, W. Subliminal stimulation: a pervasive problem for psychology. *Psychol. Bull.,* 1964, **61**, 81-99.

———, AND DARBY, C. L. Patterns of experience and the constancy of an indifference point for perceived weight. *Amer. J. Psychol.,* 1955, **68**, 575-584.

———, MAIER, R. A., AND HELSON, H. The influence of context upon the estimation of number. *Amer. J. Psychol.,* 1963, **76**, 464-469.

———, AND PRITCHARD, JOAN F. The effect of subliminal tones upon the judgment of loudness. *J. exp. Psychol.,* 1963*a*, **66**, 23-29.

———, AND ———. The anchor effect and the problem of relevance in the judgment of shape. *J. gen. Psychol.,* 1963b, **69**, 147-161.

———, AND ———. The effect of visual intensities upon the judgment of loudness. *Amer. J. Psychol.,* 1964, **77**, 93-98.

———, AND TURNER, E. D. Assimilation and contrast in the estimation of number. *J. exp. Psychol.,* 1964, **67**, 458-462.

BLACK, R. W., AND BEVAN, W. The effect of subliminal shock upon judged intensity of weak shock. *Amer. J. Psychol.,* 1960, **73**, 262-267.

BOARDMAN, W. K., AND GOLDSTONE, S. Effects of subliminal anchors upon judgments of size. *Percept. mot. Skills,* 1962, **14**, 475-482.

BORING, E. G. *The Physical Dimensions of Consciousness.* New York: Appleton-Century-Crofts, Inc., 1933.

———. *Sensation and Perception in the History of Experimental Psychology.* New York: Appleton-Century-Crofts, Inc., 1942.

Bühler, K. The skywise and neighborwise navigation of ants and bees. *Acta Psychologica*, 1951-52, **8**, 225-263.

———. The essentials of contact navigation. *Acta Psychologica*, 1954, **10**, 278-316.

Cohen, J., Hansel, C. E. M., and Sylvester, J. D. A new phenomenon in time judgment. *Nature*, 1953, **172**, 901-903.

Conners, C. K. Visual and approach motives as a function of discrepancy from expectancy level. *Percept. mot. Skills*, 1964, **18**, 457-464.

Deutsch, J. A. A system for shape recognition. *Psychol. Rev.*, 1962, **69**, 492-500.

———. Neurophysiological contrast phenomena and figural aftereffects. *Psychol. Rev.*, 1964, **71**, 19-26.

Dusek, E. R., Teichner, W. H., and Kobrick, J. L. The effects of the angular relationships between the observer and the base-surround on relative depth-discrimination. *Amer. J. Psychol.*, 1955, **68**, 438-443.

Edelberg, R. The relationship between the galvanic skin response, vasoconstriction, and tactile sensitivity. *J. exp. Psychol.*, 1961, **62**, 187-195.

Ehrenfels, C. von. Über Gestaltqualitäten. *Vierteljahrsschrift f. wiss. Philosophie*, 1890, **14**, 249-292.

Evans, R. M. *Eye, Film, and Camera in Color Photography*. New York: John Wiley & Sons, Inc., 1959.

Fehrer, E. V. An investigation of the learning of visually perceived forms. *Amer. J. Psychol.*, 1935, **47**, 187-221.

Gaito, J. Visual discrimination of straight and curved lines. *Amer. J. Psychol.*, 1959, **72**, 236-242.

Gardner, W. J., Licklider, J. C. R., and Weisz, A. Z. Suppression of pain by sound. *Science*, 1960, **132**, 32-33.

Garner, W. R. *Uncertainty and Structure as Psychological Concepts*. New York: John Wiley & Sons, Inc., 1962.

Gibson, J. J. Adaptation with negative aftereffect. *Psychol. Rev.*, 1937, **44**, 222-244.

———. What is a form? Psychol. Rev., 1951, **58**, 403-412.

———. The information contained in light. *Acta Psychologica*, 1960, **17**, 23-30.

———. Observations on active touch. *Psychol. Rev.*, 1962, **69**, 477-491.

Gibson, J. J., and Cornsweet, J. The perceived slant of visual surface—optical and geographical. *J. exp. Psychol.*, 1952, **44**, 11-15.

Goldstone, S., and Goldfarb, Joyce L. Direct comparison of auditory and visual durations. *J. exp. Psychol.*, 1964*a*, **67**, 483-485.

———, and ———. Auditory and visual time judgment. *J. gen. Psychol.*, 1964*b*, **70**, 369-387.

Graham, C. H. Sensation and perception in an objective psychology. *Psychol. Rev.*, 1958, **65**, 65-76.

———, and Ratoosh, P. Notes on some interrelations of sensory psychology, perception, and behavior. In *Psychology: A Study of a Science, S. Koch (ed.)*. New York: McGraw-Hill Book Company, Inc., 1962.

Graham, Elaine H. Figural after-effects as functions of contrast, area, and luminance of the inspection-figure. *Psychologia*, 1961, **4**, 201-208.

Guilford, J. P. System in the relationship of affective value to frequency and intensity of auditory stimuli. *Amer. J. Psychol.*, 1954, **67**, 691-695.

———, and Smith, Patricia C. A system of color preferences. *Amer. J. Psychol.*, 1959, **72**, 487-502.

Gulamerian, N. B. *The Language of a Work of Art*. New York: Utrecht Linens, 1963.

Haber, R. N. Discrepancy from adaptation level as a source of affect. *J. exp. Psychol.*, 1958, **56**, 370-375.

Helson, H. The psychology of Gestalt. *Amer. J. Psychol.*, 1925, **36**, 342-370.

———. Demonstration of pupillary, accommodative, and consensual reflexes, through changes in apparent size of a pinhole. *J. gen. Psychol.*, 1935, **13**, 186-188.

———. Size-constancy of the projected afterimage. *Amer. J. Psychol.*, 1936, **48**, 638-642.

———. Fundamental problems in color vision. 1. The principle governing changes in hue, saturation, and lightness of non-selective samples in chromatic illumination. *J. exp. Psychol.*, 1938, **23**, 439-476.

———. Some factors and implications of color constancy. *J. opt. Soc. Amer.*, 1943, **33**, 555-567.

———. *Perception and Personality—A Critique of Recent Experimental Literature*. Report No. 1, Project No. 21-0202-0007, USAF, School of Aviation Medicine, Randolph Field, Texas, 1953, 1-55.

———. Studies of anomalous contrast and assimilation. *J. opt. Soc. Amer.*, 1963, **53**, 179-184.

———. Current trends and issues in adaptation-level theory. *Amer. Psychologist*, 1964*a*, **19**, 26-38.

———. *Adaptation-Level Theory: An Experimental and Systematic Approach to Behavior*. New York: Harper & Row, Publishers, 1964*b*.

———. Constancy. In *International Encyclopedia of the Social Sciences*, (in press).

———, and Bevan W. An investigation of variables in judgments of relative area. *J. exp. Psychol.*, 1964, **6**, 335-341.

———, AND FEHRER, E. V. The role of form in perception. *Amer. J. Psychol.*, 1932, **44**, 79-102.

———, AND JOY, V. Domains of lightness assimilation and contrast effects in vision. *Psychol. Beitr.*, **1962**, 6, 405-415.

———, AND KING, S. M. The Tau effect—an example of psychological relativity. *J. exp. Psychol.*, 1931, **14**, 202-217.

———, AND NASH, MYRTLE C. Anchor, contrast, and paradoxical distance effects. *J. exp. Psychol.*, 1960, **59**, 113-121.

———, AND ROHLES, F. H., JR. A quantitative study of reversal of classical lightness-contrast. *Amer. J. Psychol.*, 1959, **72**, 530-538.

HERMANS, T. G. The relationship of convergence and elevation changes to judgments of size. *J. exp. Psychol.*, 1954, **48**, 204-208.

HERNÁNDEZ-PEÓN, R. Psychiatric implications of neurophysiological research. *Bull. Menninger Clinic*, 1964, **28**, 165-185.

HERRMANN, T. Die phänomenologische als einer kritische Methode, *Acta Psychologica*, 1961, **19**, 522-527.

HUNTER, R. S. A new gloss comparator and a study of different types of gloss effects. Washington, D.C.: Circular of the National Paint, Varnish, and Lacquer Association, Inc., 1935, No. 493, 268-280.

HUSSERL, E. G. Ideen zu einer reinen Phänomenologie und phänomenologischen Philosophie. *Jahrb. f. Philosophie und phänomenologische Forschung*, 1913, **1**, 1 ff.

HUXLEY, T. H. *Hume with Helps to the Study of Berkeley*. London (England): MacMillan & Co., Ltd., 1901.

ITTELSON, W. H. *The Ames Demonstrations in Perception*. Princeton, N.J.: Princeton University Press, 1952.

JAFFE, R. The influence of visual stimulation on kinesthetic figural aftereffects. *Amer. J. Psychol.*, 1956, **69**, 70-75.

KATZ, D. Die Erscheinungsweisen der Farben und ihre Beeinflussung durch die individuelle Erfahrung. *Z. f. Psychol.*, 1911, Erg. Bd. 7.

———. *Der Aufbau der Tastwelt.* Leipzig: Barth, 1925.

KOFFKA, K. Perception: an introduction to the gestalt-theorie. *Psychol. Bull.*, 1922, **19**, 531-585.

———. *Principles of Gestalt Psychology.* New York: Harcourt, Brace, and Company, 1935.

KÖHLER, W. *Die physischen Gestalten in Ruhe und im stationären Zustand*, Braunschweig: Vieweg & Sohn, 1924.

———, AND WALLACH, H. Figural after-effects: an investigation of visual processes. *Proc. Amer. Philos. Soc.*, 1944, **88**, 269-357.

LADD, G. T., AND WOODWORTH, R. S. *Elements of Physiological Psychology*. New York: Charles Scribner's Sons, 1911.

LINDSLEY, D. B. Physiological psychology. *Annu. Rev. Psychol.*, 1956, **7**, 323-348.

LOCKE, J. *An Essay Concerning Human Understanding*. London: G. Bell & Sons, 1913 (originally published in 1690).

MACH. E. *Analysis of Sensations*. Chicago: Open Court, 1914 (German ed. Jena: 1886).

MCCLELLAND, D. C., ATKINSON, J. W., CLARK, R. A., AND LOWELL, E. L. *The Achievement Motive*. New York: Appleton-Century-Crofts, Inc., 1953.

MICHELS, K. M., AND ZUSNE, L. The metrics of visual form. *Psychol. Bull.*, 1965, **63**, 74-87.

MILLER, G. A. What is information measurement? *Amer. Psychologist*, 1953, **8**, 3-11.

———. The magical number seven, plus or minus two: some limits on our capacity for processing information. *Psychol. Rev.*, 1956, **63**, 81-97.

MILLER, J. G. Information input overload and psychopathology. *Amer. J. Psychiat.*, 1960, **116**, 695-704.

MOWATT, MARIAN H. Configurational properties considered "good" by naive subjects. *Amer. J. Psychol.*, 1940, **53**, 46-69.

MOYLAN, J. J. Kinesthetic figural aftereffects: satiation or contrast. *J. exp. Psychol.*, 1964, **67**, 83-90.

MURDOCK, B. B. The distinctiveness of stimuli. *Psychol. Rev.*, 1960, **67**, 16-31.

NACHMIAS, J. The effect of stimulus-heterogeneity on free recall. *Amer. J. Psychol.*, 1958, **71**, 578-582.

NELLIS, BARBARA S. Effects of object- and background-tilt on perception of form. Ph.D. dissertation, University of Texas Library, Austin, 1958.

NISSEN, H. W. Phylogenetic comparison. In *Handbook of Experimental Psychology*, S. S. Stevens (ed.). New York: John Wiley & Sons, Inc., 1951.

PATERSON, D. G., AND TINKER, M. A. The part-whole proportion illusion in printing. *J. appl. Psychol.*, 1938, **22**, 421-425.

PFAFFMANN, C. The sensory and motivating properties of the sense of taste. *Nebraska Symposium on Motivation*, Lincoln, Neb.: University of Nebraska Press, 1961.

———. Sensory processes and their relation to behavior: studies on the sense of taste as a model S-R system. *Psychology: A Study of a Science*, S. Koch (ed.). New York: McGraw-Hill Book Company, Inc., 1962, 380-416.

ROBINSON, J. S., BROWN, L. T., AND HAYES, W. H.

Test of effects of past experience on perception. *Percept. mot. Skills*, 1964, **18**, 953-956.

ROCK, I., AND BROSGOLE, L. Grouping based on phenomenal proximity. *J. exp. Psychol.*, 1964, **67**, 531-538.

———, AND EBENHOLTZ, S. Stroboscopic movement based on change of phenomenal rather than retinal location. *Amer. J. Psychol.*, 1962, **75**, 193-207.

ROELOFS, C. O. Convergence and depth perception. *Acta Psychologica*, 1960, **17**, 193-217.

RUBIN, E. *Visuell wahrgenommene Figuren.* Copenhagen, 1921.

SHANNON, C. E. A mathematical theory of communication. *Bell Syst. tech. J.* 1948, **27**, 379-423; 623-656.

SNYGG, D., AND COMBS, A. W. *Individual Behavior.* New York: Harper & Brothers, 1949.

SQUIRES, P. S. Stereopsis produced without horizontally disparate stimulus loci. *J. exp. Psychol.*, 1956, **52**, 199-203.

STEGER, J. A. A dimensional study of lightness assimilation and contrast as a function of differential stimuli. Unpublished doctoral dissertation, Kansas State University Library, 1964.

SUEDFELD, P., GRISSOM, R. J., AND VERNON, J. The effects of sensory deprivation and social isolation on the performance of an unstructured cognitive task. *Amer. J. Psychol.*, 1964, **77**, 111-115.

THORNDIKE, E. L. Animal intelligence. *Psychol. Rev. Monogr. Suppl.*, 1898, **2**, Whole No. 8.

TITCHENER, E. B. *A Textbook of Psychology.* New York: The MacMillan Co., 1909.

TINBERGEN, N. *The Study of Instinct.* New York: Oxford University Press, 1951.

TURNER, E. D., AND BEVAN, W. Simultaneous induction of multiple anchor effects in the judgment of form. *J. exp. Psychol.*, 1962, **64**, 589-592.

WALK, R., GIBSON, ELEANOR J., AND TIGHE, T. The behavior of light- and dark-reared rats on a visual cliff. *Science*, 1957, **126**, 80-81.

WALLS, G. L. "Land! Land!" *Psychol. Bull.*, 1960, **57**, 29-48.

WERTHEIMER, M. Experimentelle Studien über das Sehen von Bewegung. *Z. f. Psychol.*, 1912, **61**, 161-265.

———. Untersuchungen zur Lehre von der Gestalt. 11. *Psychol. Forsch.*, 1923, **4**, 301-350. (English translation in *Readings in Perception*, D. C. Beardslee and M. Wertheimer (eds.). Princeton, N.J.: D. Van Nostrand Company, Inc., 1958).

WOODWORTH, R. S., AND SCHLOSBERG, H. *Experimental Psychology* (rev. ed.). New York: Holt, Rinehart & Winston, 1954.

CHAPTER 9

Engineering Psychology

JACK A. ADAMS
University of Illinois

INTRODUCTION

Engineering psychology is a technology that uses the scientific knowledge and tools of the science of behavior for the design and use of man-machine systems. *A man-machine system is an organization of men and machines working in an integrated fashion to achieve a goal*, and the system must be designed to achieve the goals of the system within limits of cost, complexity, and various other restraints. The engineering psychologist works to optimize the performance of man-machine systems by manipulating the variables which influence the performance of man, in contrast to the variables of the physical sciences that are manipulated by most other engineers and scientists who work on the design of a system. Examples of man-machine systems are the automobile and its driver, the telephone system, or a military weapon system.

In the beginnings of engineering psychology the concern was most often with an isolated dial that was difficult to read, or a control that was difficult to manipulate. While there are many who still conceive engineering psychology as being narrowly concerned with controls, displays, and work-space layout, it has become increasingly evident that optimization of a system requires a comprehensive view of personnel problems, and it is necessary to give personnel the same integrated attention that is given the aggregation of electronic and mechanical devices that comprise the equipment of a system. Personnel selection and classification, the relative capabilities of men and machines and how system duties should be distributed among them, the design of controls and displays, the design of safety and survival equipment, communications, and complex issues in the design and uses of training devices, are just some of the human factors problems that are now considered in the design and development of a system. The comprehensiveness of the human factors approach that is considered necessary for a successful system has led to conceptualization of the *personnel subsystem* of operators and maintenance men, as distinct from the so-called "hardware" subsystem of physical equipment. The weaving together of these subsystems emphasizes that an interdisciplinary approach is required to accomplish the system's design and development, where many sciences function as teams. For those who are unfamiliar with systems and their conceptualization, recommended reading is Goode and Machol (1957), Flagle, Huggins, and Roy (1960), Eckman (1961), and Gagne (1962).

DATA AND ISSUES IN ENGINEERING PSYCHOLOGY

This section will discuss representative variables, data, and issues that are of primary significance for the design and use of man-machine systems, and which ordinarily are studied in the laboratory under the rubric of engineering psychology rather than general experimental psychology. The studies reported here mainly will be laboratory studies, and they will show a search for regularities at quite a different level from that customarily examined by general experimental psychologists. Questions like these are asked: What is the best way to design a display for speedy and accurate response to visual stimuli? How should a control system be designed so that movements of it by an operator will promote minimum response error? Can the human operator give unflagging attention to a visual or an auditory display for long periods of time without serious decrements in performance? All of these questions inquire about important properties of human behavior, and answers to them have direct implications for the design of man-machine systems.

DISPLAY OF VISUAL DATA

Most data for human response in a man-machine system are presented visually. Audition plays its primary role in a system as the sensory channel by which personnel receive the complex code we call speech, and man's verbal communications are unquestionably a significant element in most man-machine systems. Audition is also used as a warning signal. However, audition does have distinct limitations, particularly for the complex and continuous information that is presented via the media of dials, instruments, and cathode-ray tubes. The chief merits of the visual modality are straightforward. The ear lacks selectivity and is at the mercy of all sounds; the eyes can select their stimuli. Vision is a spatial sense, whereas audition is basically a temporal sense that must discriminate the patterning of audio signals with respect to time. Auditory encoding of spatial relationships is feasible, such as high tones for up and low tones for down, but the possibilities are rather limited. As a spatial sense, visual data are usually arrayed in front of an operator, and he scans them and selects what he needs at will. Perhaps fifty dials on a display panel might confront a man, and by visual sampling he can assimilate the information and operate a system with effectiveness. There is no practical way to present so much data to the ear. Each channel would have to be distinctively coded for sequential presentation to the ear. Extensive auditory coding is rather difficult, and certainly it would be time-consuming to run through a series of coded auditory signals and interpet them. Data to the eye, on the other hand, are easily encoded and can be presented in many ways on graphs, charts, dials, counters, and cathode-ray tubes. Detailed discussions of the relative capabilities of the eye and the ear have been presented by Henneman and Long (1954) and Mowbray and Gebhard (1961).

Many of the visual data presented to a human operator are continuous, and research on visual displays has centered around the effectiveness of different ways of presenting continuous stimuli. A limited amount of research has been done on characteristics of discrete signals, such as for emergency warning, but the issues for discrete signals are relatively few and research on them will not be discussed in this chapter. What kinds of information do operators want from a visual instrument for the display of continuous visual stimuli? One or a combination of the following kinds of information is needed from visual instruments:

1. Numerical reading. The operator must obtain a number to guide his response input to the system.

2. Check and qualitative reading. Often it suffices to know the approximate value of an instrument, or merely that it has changed. For example, an operator may be able to ignore the value of an instrument as long as the needle does not fall into a red danger zone. Or, a panel of aircraft engine instruments might signify satisfactory performance as long as all needles lie in the three o'clock position.

3. A directorial or command function that tells the operator what he should be doing. This requires a reference value for the system, and a way of presenting a command signal for the operator to make a corrective response. Many tracking displays are director displays by this general definition, but the term has come to have a more specific meaning with respect to avant garde displays that will use computers to issue commands to the display for an operator's response.

Usually there is no need for a display to have all three of these features. Numerical reading, and/or check and qualitative reading, are sufficient requirements for most instruments. As with any design feature of a system, a careful analysis must be made to determine what is needed so that the simplest and most reliable device can be tailored for the job.

DISPLAYS FOR THE PRESENTATION OF CONTINUOUS DATA Most instruments are *symbolic displays*, because they present abstract, quantitative information, such as numerical values. Symbolic displays are distinguished from *pictorial displays* that present a quasi-realistic view of the system in relation to aspects of the real world (Chapanis, Garner, and Morgan, 1949; Grether, 1947; Matheny, 1961). There are three basic types of symbolic displays: counters, circular scales, and linear scales. Each of these will be discussed in turn, with the emphasis being on their respective merits in transmitting different aspects of basic data for the system. No particular attention will be devoted to properties of dials and instruments such as size, the nature of scale markings, and the size of numerals. For these features, see Baker and Grether (1954), Chapanis, Garner, and Morgan (1949), Fitts (1951), and McCormick (1957).

Research Methods for Visual Displays. There is always a question about the generalizability of laboratory research on visual displays, or any other research for that matter, that is intended to reveal relationships that can be used in complex man-machine systems. The engineering psychologist at some point decides that his relationships are strong enough, and he makes an inductive leap from his laboratory findings to the real world of systems.

What should be the characteristics of a research device in order to ensure generalization of laboratory findings to a system task that invariably is more complex than the laboratory task? Research on visual displays has used laboratory tasks that range in complexity from simple test booklets with pictures of instruments to read and tachistoscopes for brief projections of displays, to large electronic simulators where one or more new instruments are put in a panel, and which operate with high realism and closely approximate the complexity of the operator's task in the real system. There is no answer to the question of how complex a research device should be for making design decisions about visual displays. Whether a laboratory task has all of the relevant variables of the system can never be known with certainty. It is argued that more complex research devices, such as simulators, have a greater probability of containing more of the variables and interactions of the real world than simple tasks like test booklets. There is truth in this. On the other hand, when an experimental display is imbedded among other instruments in a simulator, and complex control manipulations are required to maneuver the simulated system, the investigator learns very little about the experimental display itself, although he can learn with some degree of confidence whether the operator's task as a whole will be satisfactory. For applied research this often is good enough, but it provides little in a substantive way for the next system that comes along. A broader approach, which some investigators have followed, is to abstract the problem for study in a succession of tasks and build up a stable network of relationships about visual displays in both simple and complex situations. This research strategy tends to enhance the power of generalization, and produces knowledge that is more useful for a wider range of design problems.

Symbolic Displays. Figure 9-1 shows the three basic kinds of visual indicators. The simplest instrument is a *counter*, and its strengths and weaknesses can be seen readily. Its chief merit, and an obvious one, is that it is unequivocal for the presentation of numerical information. If the numbers are of good size and snap into position rather than move continuously and get part in and part out of the windows, there is no doubt about the number to be read. A counter's greatest shortcoming is that it is poor for check and qualitative reading. An operator must read the displayed value explicitly, and then call upon his memory for the desired value to determine whether a change has taken place. Grether (1949) conducted a study on various design configurations for altimeters and included a counter among them. Subjects interpreted various altimeter designs printed in test booklets. Reading errors and time were recorded. The counter had the best performance of all. However, Grether could not seriously entertain a counter as an altimeter because of its poor check reading features. An altimeter must have good check reading characteristics so the pilot can quickly glance and tell if there has been a change in the altimeter's reading without actually reading it. Change in altitude for a circular or a linear scale with a moving pointer would be revealed in a direct perceptual

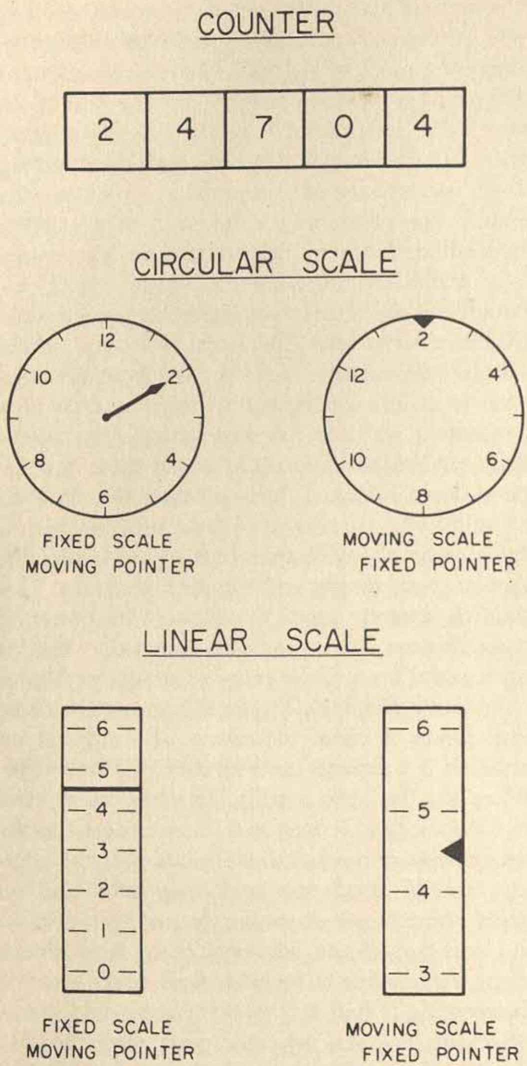


Fig. 9-1. The three basic kinds of instruments for presenting symbolic visual data.

fashion by a shift in the pointer's position. A counter may not be suitable as an altimeter, but much relevant data for many systems require nothing more than transmitting numerical values to an operator, and a counter is a splendid, inexpensive way of doing it.

The most common way of presenting continous visual stimuli is by *circular scales.* Figure 9-1 shows the two basic configurations of circular scales. The fixed scale-moving pointer instrument has a fixed background with printed numerals and a moving indicant. The moving scale-fixed pointer instrument again has the numerals printed on the background scale except that the scale moves and the reading is given opposite a fixed indicant. The fixed scale-moving pointer configuration seems to have the edge over its sister configuration with the moving scale and the fixed pointer. Baker and Grether (1954) state that the assignment of direction of control movement to direction of indicator motion (control-display relationships) is more straightforward with the fixed scale-moving pointer instrument. A point in favor of the fixed scale-moving pointer instrument is that check and qualitative reading are easier because the pointer position is easily seen for the approximate value. The moving scale-fixed pointer instrument is more like a counter—it is necessary to read the actual numerical value in order to determine its correctness. Matheny (1961, p. 99) reported a significant incident that speaks in behalf of the fixed scale-moving pointer circular instrument. The canopy of a high-speed jet bomber was jettisoned after an explosion, and the pilot elected to stay with the aircraft and return it to the base. Receiving heading instructions from a ground station, the pilot was able to discern the approximate position of the needle on his compass and hold an approximate heading, even though the wind blast was blurring his vision so severely that he was unable to make specific numerical readings. He returned the plane safely simply by making qualitative readings. The fixed scale-moving pointer might have a slight disadvantage in requiring the operator to momentarily search for the position of the moving pointer, which is not required by the moving scale-fixed pointer instrument, but ordinarily the operator is sufficiently aware of the state of the system to direct his check reading glance. All in all, it would seem that the fixed scale-moving pointer instrument is best, although Christensen's study (1952) suggests values of interacting variables, where the moving scale-fixed pointer instrument might work just as well.

Another primary type of symbolic instrument is the *linear scale,* which can be set vertically or horizontally. Just as circular scales, linear instruments can be built to have a fixed scale-moving pointer or a moving scale-fixed pointer configuration (Fig. 9-1). The latter is usually called a moving-tape instrument, because the numerals are printed on a long tape and are moved past the vertical window at a rate determined by the rate of change of the variable. One advantage of a moving tape is that a very long scale of values can be presented. For example, it would be feasible to present the altitude values for a space vehicle from sea level to 1000 miles in units of feet with a linear instrument and a moving tape. Presenting a long range of values on a circular instrument is clumsy, as the contemporary 3-needle altimeter testifies. In order to get a sufficient range for this instrument, one needle is for 0 to 99 ft, a second one for 100 to 999 ft, and a third for 1000 to 99,999 ft. Obviously three needles badly clutter an instrument that is only about three inches in diameter. Fitts and Jones (1961) and Grether (1949) both show that the 3-needle altimeter is gravely prone to pilot reading errors. Fixed scale-moving pointer linear instruments are less desirable for a long range of values, because the numerical separations often must be too gross for the small space available. A possible solution is a nonlinear scale, such as a logarithmic scale, where there is good spacing between values at the low end of the range but crowding and poor discrimination at the high end. There may be no objection to a nonlinear scale for some applications.

Are linear instruments better than circular ones? There is no particular evidence that they are. Sleight (1948), Graham (1956), and Wilkerson, Fox, and Matheny (1959) all found circular scales superior to linear scales. Grether's study (1949) on altimetry demonstrated that a properly designed circular instrument, such as a drum-pointer altimeter, compared very favorably with linear instruments. Graham (1956) suggested that the superiority of circular scales is because of reduced eye movements, but no evidence was presented for this hypothesis. These five studies all used simple research tasks. Grether used test booklets, and the others used the tachistoscopic method in one form or other.

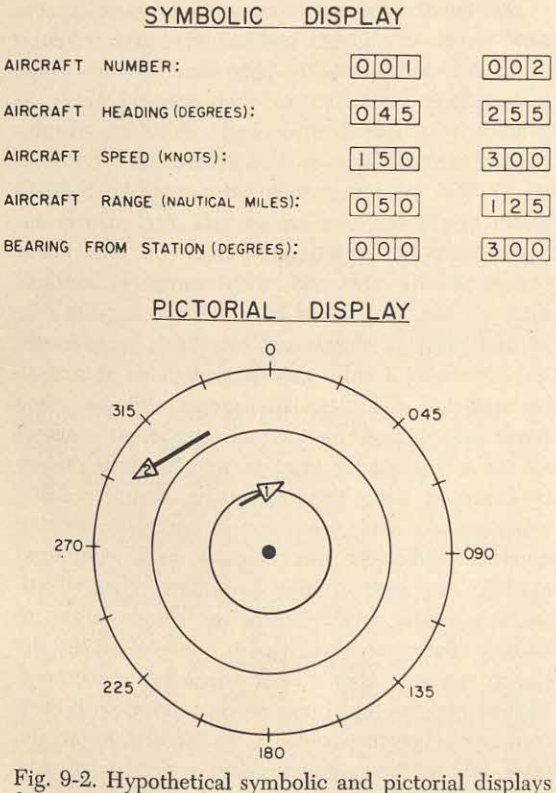
Elkin (1959) criticized the findings of Sleight (1948) because he used only 120-msec exposure time in comparing the different shapes of dials. Using a tachistoscope, Elkin extended previous work and compared circular and linear scales at 120, 360, and 1080 msec for quantitative reading. He also used free responses, where the subject's response terminated the display. With an exposure interval of 120 msec, the circular scale gave fewer errors than the linear scale, which confirmed Sleight's findings. At longer intervals, there were no significant differences, which is consistent with a finding by Christensen (1952) on the interaction between scale shape and exposure time. When qualitative reading was studied, reporting of the reading was in terms of only one of three major categories of scale division, and the circular scale was again superior to the linear scale. Overall, Elkin's study supported earlier findings in showing the superiority of circular scales over linear scales, and this is particularly true when the instrument is seen only very briefly, as with a quick glance.

All of these studies which show that circular scales are often superior to linear scales have used simple modes of display and a simple reporting response by the subject. Performance is assumed to be determined by perceptual properties of the simple instrument itself. This is a necessary and important approach. However, visual displays are often used in complex systems, such as aircraft, and investigators have pushed beyond simple laboratory tasks and have asked how linear and circular instruments compare in more complex tasks. Using flight simulators, Mengelkoch and Houston (1958*a*, 1958*b*, 1958*c*) and Mengelkoch (1959) were unable to show that a linear altimeter was better than a circular one.

Despite the failure of research to establish the superiority of linear instruments over circular ones, it is interesting to note that the Air Force is installing linear scales with moving tapes as primary instruments in cockpits of advanced military jet aircraft (Ritchie and Baker, 1957; Svimonoff, 1958). A conceivable advantage that linear scales might have in an aircraft, and which has been given little systematic study, is that linear scales can be designed with command markers that are set by ground or aircraft computers, or by the pilot. A command marker is a reference line that tells a pilot the desired value that is to be maintained for the instrument. By designing linear instruments so that all command markers are in a horizontal line across the panel when they are at their desired values, check reading becomes an easy matter of simply checking whether indicants are aligned with command markers, at least so the hypothesis goes. Command markers might create the disadvantage of adding clutter to the display, and offsetting the check reading advantage somewhat. Svimonoff (1958) reports that pilots were satisfied with their own flying performance in an experimental aircraft whose basic instruments were linear, but his conclusions were derived from casual flight test reports and were not based on a controlled experimental evaluation. The question is not whether an airplane can be flown with linear instruments, but whether flying is better with linear instruments than with other instrument designs.

Pictorial Displays. The distinctive characteristic of pictorial displays is that they represent a quasi-realistic view of the real world and have little or no symbolic data that must be translated into real-world terms. A principal advantage argued for pictorial displays is that they capitalize on the vast amount of past perceptual learning for objects and relationships in the real world. A second advantage is that they provide display integration, where the quantities from a number of symbolic instruments are combined and presented as a unified scene.

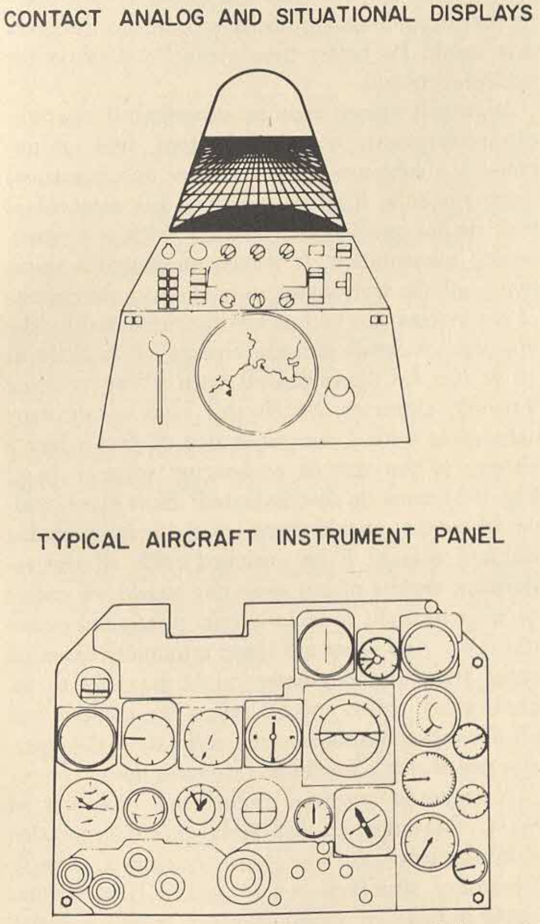
Figure 9-2 is a hypothetical example showing how air traffic control data might be presented by both symbolic and pictorial means. The traffic controller must know the aircraft heading, aircraft speed, and range and bearing of each aircraft from his site. The aircraft would be sensed by radar, and the quantities could be computed for presentation on counters as a symbolic display in the upper half of Fig. 9-2. An operator might have 100 sets of five counters if he was near a major airport. The lower part of Fig. 9-2 shows a pictorial display presenting the same information. The direction of the arrow indicates an aircraft's heading, the length of the arrow its speed, the distance from the center of the display its range in miles, and bearing as the angular displacement of the aircraft symbol from zero degrees at the top of the display. This is a direct, meaningful view of the air traffic situation and, in addition, shows the spatial relationships among aircraft. Another example might be a pictorial navigational map display in an aircraft cockpit that would show the pilot his aircraft as it moved in relation to the destination and other aircraft in the same general airspace. A distinction for pictorial displays, which will be discussed in greater detail later, is that these examples represent *horizontal or situational displays* which present a masterful view of system-environment relationships for choice and decision responses. Situational displays are distinguished from *vertical or contact-analog displays* which present a forward view from a vehicle and which provide cues to an operator for moment-to-moment tracking control of the system's attitude and/or orientation. The upper part of Fig. 9-3 shows a futuristic, and hypothetical aircraft with a contact-analog and a situational display. The lower half of Fig. 9-3 shows a conventional aircraft cockpit with symbolic instruments that are planned for replacement by pictorial displays (if and when research shows the way).



AIRCRAFT NUMBER:	001	002
AIRCRAFT HEADING (DEGREES):	045	255
AIRCRAFT SPEED (KNOTS):	150	300
AIRCRAFT RANGE (NAUTICAL MILES):	050	125
BEARING FROM STATION (DEGREES):	000	300

Fig. 9-2. Hypothetical symbolic and pictorial displays for a ground controller of an air traffic control system.

The most comprehensive research on pictorial displays, and a pioneering effort, was conducted at the University of Illinois. This research centered around the relative merits of symbolic and situational pictorial displays for omni-directional radio range (VOR), which is a navigational aid that gives the pilot continuous information about his azimuth position in relation to any VOR station that he selects, and the pilot can easily determine the compass heading for flying to or from a station on any desired track. The University of Illinois investigators posed the general research problem of pictorial versus symbolic instruments. The first study by Williams and Roscoe (1949) used test booklets with drawings of eight experimental displays. Response was selection from multiple-choice answers about the direction of turn and the heading required to fly the aircraft on a desired track. Aircraft pilots were used as subjects, and each was given ten problems on each of the eight displays. The measures of performance for a display were numbers of errors and time to complete a set of problems. Before presenting the results, it is important to point out a basic difference between the pictorial displays that were used. The drawings of the pictorial displays had a symbol for the station, the aircraft, and a selector line that indicated the track to fly, and it looked similar to the situational display shown in Fig. 9-3. One type of pictorial display had a fixed station in the center and a moving aircraft symbol. This has come to be called the outside-in display, as if the pilot was outside the entire system and looking down upon himself and the VOR station. The other type had the aircraft as a fixed symbol in the center and the station moving toward the aircraft symbol. This is an inside-out display, as if the pilot is inside his aircraft and looking out at the station moving toward him. The significant finding for the Williams-Roscoe experiment was that pictorial displays required about half the time to solve navigational problems as symbolic displays, and many fewer errors were made with them. Moreover, the moving aircraft-fixed station, or outside-in display, was the best of all.


CONTACT ANALOG AND SITUATIONAL DISPLAYS

TYPICAL AIRCRAFT INSTRUMENT PANEL

Fig. 9-3. A conventional instrument panel for a jet aircraft, and a situational and a contact-analog pictorial display of the types that are being researched as a possible replacement for it.

Payne (1950, 1952), also at the University of Illinois, conducted two follow-up investigations on the advantages of the outside-in situational display over the inside-out situational display for VOR navigation. In his first experiment, Payne (1950) used a variation of the usual test booklet method. He had subjects draw the aircraft path to indicate solution to the navigational problem shown on the page of the test booklet. The outside-in presentation was significantly superior to the inside-out one. Payne's second experiment (1952) used a functional mockup as the experimental device, and here again the outside-in display was superior to the inside-out version. Narva (1958) also found an outside-in display better than the inside-out one when pilots used a functional mockup to solve navigation problems.

Roscoe et al. (1950) had pilots fly realistic navigation problems in a flight simulator to assess the relative merits of pictorial and symbolic VOR navigation displays within the context of a realistic flight task. One experimental condition was the flight simulator with a conventional symbolic display, and the other had the moving aircraft-fixed station pictorial display. Pilots experienced in instrument flying each flew four problems on each of the displays. The pictorial display was impressively superior on all performance measures. In this same report Roscoe and his associates repeated the same experiment, but this time using private pilots who were considerably less experienced than the instrument pilots that were used before. The results were the same—distinct superiority of the pictorial display. These investigators noticed, however, that the pictorial display was inferior to the symbolic display in tracking, or "hitting the station." While conceivably this could be corrected with a proper design of the pictorial display, the evidence suggests that a situational pictorial display is better for planning and decision making than as a tracking display for close control of a vehicle.

The second principal class of pictorial displays is the contact-analog display that presents cues for continuous tracking control of a vehicle. Like the situational display, the contact-analog display is a single, integrated instrument that presents a quasi-realistic view of the real world, and it is intended to replace a whole set of symbolic instruments. Contact-analog displays, such as shown in Fig. 9-3, are still experimental, but it is hoped that research on them will yield design principles for their practical use in semi-automatic manned vehicles of the near future. As we shall see, there are many unresolved issues for the design of contact-analog displays. It is not at all clear how these displays should be designed for effective tracking control, or whether it is even possible to design them so that they will be at least as effective as symbolic displays for the moment-to-moment control of a vehicle.

Virtually all of the work on contact-analog displays has been under the auspices of the Army-Navy Instrument Program (Matheny, 1961), which was concerned with analyzing the informational requirements for flight vehicles and submarines, and for breaking with past design traditions and devising optimal new display presentations. Some of the research was concerned with the design of interim instrument displays, but the primary emphasis was on engineering and psychological aspects of a contact-analog display like the inside-out version shown in Fig. 9-3.[1] The Army-Navy Instrument Program was bold in seeking new techniques for displaying visual information, but in many ways it had a preconceived devotion to the contact-analog display that had no basis in psychological fact. The only work that had been done on pictorial displays was that which had been conducted on situational displays at the University of Illinois and, while this research had shown the superiority of pictorial over symbolic displays, it must be remembered that these studies had only minor interest in pictorial displays for moment-to-moment tracking control of the vehicle. In fact, in the one case where accuracy of control was measured (Roscoe et al., 1950), the pictorial display was inferior to the symbolic display. Also, Grether (1947) and Chapanis, Garner, and Morgan (1949) had expressed doubts about pictorial displays as a means of presenting the cues for vehicular control. Thus, at the outset of the Army-Navy Instrument Program, there was no evidence that a contact-analog display would be satisfactory for controlling. It might be said that this ambitious program started out with the unproved hypothesis that contact-analog displays could be designed that would be better than symbolic displays for vehicular control.

It is well known that an experienced operator can continuously control a system, like an airplane or a submarine, within rather fine tolerances using symbolic displays, but it is not established that similar precision is possible with a contact-analog presentation. It is easy to design a sensitive symbolic instrument so that a minute change of the system can be quickly discriminated by the operator. A small altitude change of as little as 10 ft can be discriminated with a conventional 3-needle altimeter. But is this kind of accuracy achievable with a contact-analog display when a change in the size of converging squares (e.g., Fig. 9-3) must be discriminated? How much real-world content should there be in a contact-analog display? Should it be enriched with all the redundant details of our everyday world, or can it be impoverished to include only minimally essential cues? And what are these minimally essential cues? Ingenious engineers might be able to include trees, rocks, and houses in the display, but all this may be unnecessarily complex if the operator needs only a few basic reference lines.

A research program under the direction of W. G. Matheny at Bell Helicopter Corporation studied some of these basic questions. Simple laboratory situations were used, where various configurations of visual displays were projected on a screen and subjects made judgments about them. Using a grid line display, like the earth surface shown in Fig. 9-3, Fox (no date) asked how much angular rotation of the display was required for subjects to notice it. Angular displacement of the display would be the cue for the bank attitude of the aircraft. From the subject's viewpoint, the grid display covered the lower half of the screen, leaving the upper half dark. The grid had horizontal lines crossed by vertical perspective lines, whose common vanishing point was slightly above the horizon line and so lacked a common focal point. The experimenter would change the angular position of the grid while it was blocked from the subject's view, and then expose it and ask if there had been a change. The JND, where approximately 50 per cent of the observations were reports of change and 50 per cent no change, was two degrees. Actually, the JND is probably an inappropriate measure for a contact-analog display. The control of many vehicles is very sensitive and critical, and it is important to ask what change is discriminated virtually 100 per cent of the time. The state of the vehicle and the need for corrective control action must be unequivocal to all operators all the time. Fox found that it took a 10 per cent change in the angular position of the grid for discriminations to be correct 97 per cent of the time. In a similar study by Sidorsky (1958), only six angles of pitch were found that could be discriminated without error when a contact-analog display was tilted throughout a range of 0-40°. Sidorsky's work is consistent with the Fox study. Apparently the angular displacement of grid squares with a vanishing perspective is a poor way of communicating angular displacement of a vehicle to an operator.

---

[1] The Army-Navy Instrument Program favored the inside-out version of the contact-analog display in most of its discussion and research, despite evidence from research on situational displays that outside-in displays are superior. It does not follow that outside-in displays will be superior for contact-analog displays also, but the research on situational displays strongly suggests that outside-in versus inside-out is a critical area for research, and that preconceptions about the merits of an inside-out presentation are hazardous. And the compatibility of frame of reference, where both situational and contact-analog displays are outside-in, or both inside-out, may be important. Little is known about a necessity for uniformity of principles throughout the instruments of a panel.

The change in the size of squares in the grid line display is one of the ways intended to depict altitude to a pilot, and Fox, Hardt, and Matheny (1959) used the same grid line projection apparatus as in the study by Fox. The grid was projected with converging vertical perspective lines, as in the previous study. Again using the criterion that display changes which yield near 100 per cent correct discrimination are more desirable, they found it took a 24.7 per cent change in the area of the squares and 13.6 per cent change in the distance between lines to produce correct discrimination of change 90 per cent of the time. No changes were judged correctly 100 per cent of the time. Altitude encoding is poor as the contact-analog display is presently conceived.

It is reasonable to expect accuracy of perceptual judgment to be importantly dependent upon the amount of detail and redundancy in the contact analog scene. Matheny and Hardt (1959*a*) assessed fifteen different visual displays for accuracy in discriminating changes in pitch and bank of a projected display. The subjects judged the direction of bank and pitch for their hypothetical aircraft. The fifteen visual displays had six different encodings of the ground plane, ranging from the grid line display that has been discussed, to dots and elipses with perspective, to a display that looked like cracked ground with perspective. Scenes of the real world with perspective were included too. Along with these ways of encoding the ground plane referent, the effects of sky texture, and obstacles on the various ground planes were studied. The results showed no significant differences among any of the fifteen displays. In a related experiment, Matheny and Hardt (1959*b*) assessed the effects of a single straight line across the display, of grid lines with vanishing perspective, and of a real world scene with vanishing perspective, on the interpretation of aircraft bank. The only differences among the three displays were small differences in errors of pitch interpretation, with a progressive reduction in number of errors as content increased. In another study in this series on display detail, Elam, Emery, and Matheny (1962) used an analog computer for simulating a helicopter, and subjects were required to control it and maintain a straight and level flight path with contact-analog cues as their reference. The four degrees of perceptual enrichment were a simple T symbol, two kinds of grid lines with vanishing vertical perspective lines, and a real world picture with vanishing perspective. None of the differences between displays were statistically significant.

An experimental program on the design of contact-analog displays for submarine control was carried out by the Electric Boat Division of General Dynamics Corporation. These studies used complex electronic equipment for the simulation of a submarine, rather than the simpler laboratory equipment that was used for much of the research by Matheny and his associates. Electric Boat had more luck in defining some of the characteristics of contact-analog displays that positively influenced proficiency of the operator. In one study Sidorsky and Newton (1959) investigated the control of a submarine simulator as a function of the contact-analog display having only one surface for the bottom of the ocean, or two surfaces representing the bottom of the ocean and the surface of the water. Submarine officers performed various depth maneuvers with each of the two displays, both with and without forward motion of the submarine in the simulation loop. Two surfaces resulted in better performance than one surface. The amount of error was still considerable, however, and it is doubtful whether these data show that a contact-analog display allows sufficient accuracy for actual use in a submarine.

The Army-Navy Instrument Program had certain preconceptions about the worthiness of the contact-analog display, and it did little in the way of comparing contact analog with corresponding symbolic displays for control, as Roscoe et al. (1950) did for situational displays. An important exception is a study by Blair and Plath (1962). Again, a submarine simulator was used. This experiment was a particularly critical test for the contact-analog display, because it employed the most advanced ideas. For example, two surfaces were used, which had been shown to be a superior configuration in the study by Sidorsky and Newton (1959). Moreover, the display had a director or command pathway that told the submarine operator how to steer. The contact-

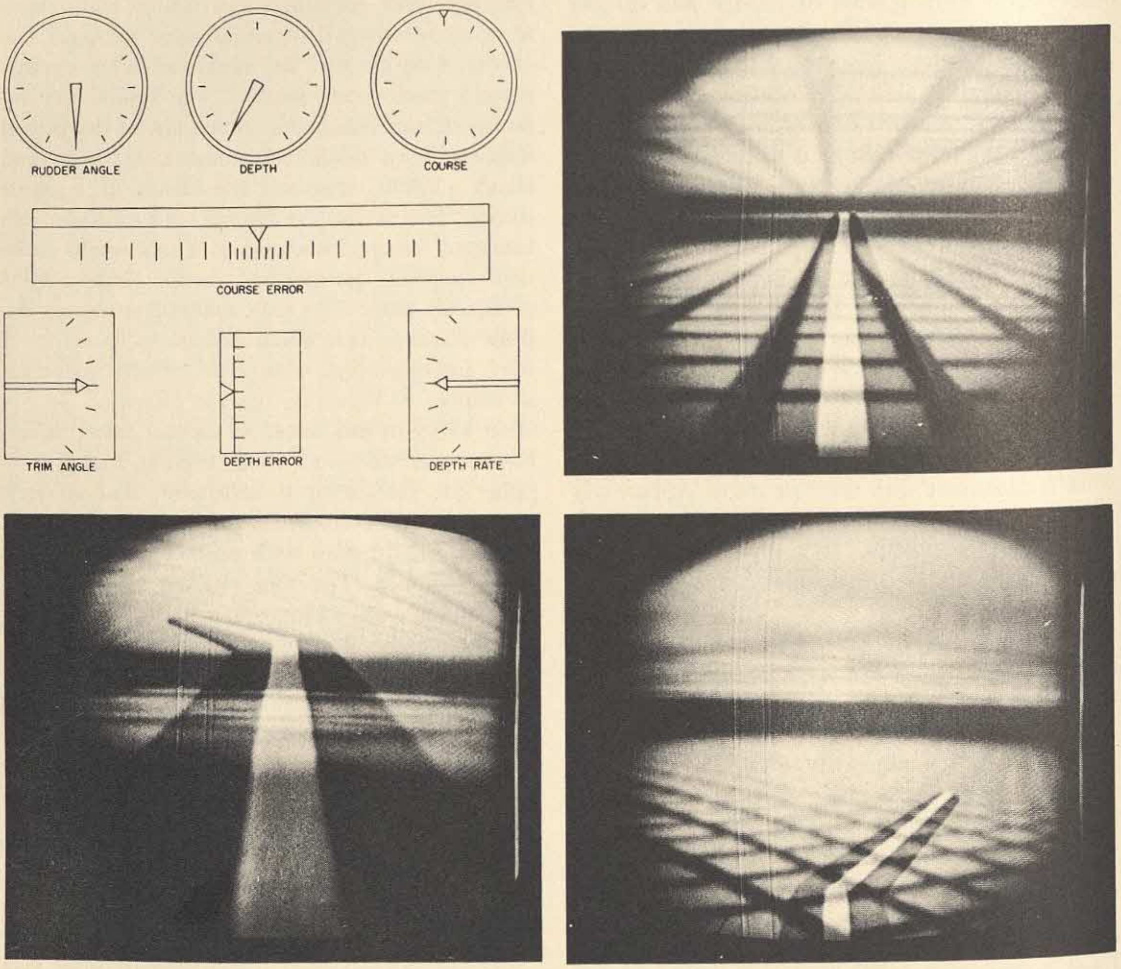


Fig. 9-4. The symbolic instrument panel and three views of the contact-analog display that were compared in the study by Blair and Plath (1962) for submarine control. The picture in the upper left is the symbolic panel. The contact-analog scene in the upper right shows the submarine on the correct course and depth. The contact-analog scene at the lower left can have either an Order Interpretation of go left 30° and up 100 ft, or an Error Interpretation of go right 30° and down 100 ft. The display in the lower right has an Order Interpretation of go right 30° and down 100 ft, or an Error Interpretation of go left 30° and up 100 ft.

analog display was compared with a combined instrument panel which was a symbolic display for depth and course control. Figure 9-4 shows three pictures of the contact-analog display that were used, and the combined instrument panel. Notice in Fig. 9-4 that the command pathway can be interpreted either as directing an order or showing an error to be corrected, and both of these uses were investigated in this study. Performance was measured in terms of "time on target," which was the time that course and depth were held correctly within defined tolerances. Simultaneous time on ordered course and ordered depth were also recorded. The results showed that the contact-analog display produced less than half the proficiency of the combined instrument panel, leaving little doubt of its inferiority to a symbolic display.

In critique and conclusion on contact-analog displays, the hypothesis of their virtue for close vehicular control has not been disproved, because there are many variables that. remain to be studied. It may be possible to devise a contact-analog display that will be superior to any configuration of symbolic instruments but, on the basis of present evidence, there is good cause for doubt. The research of Matheny and his associates provides no good leads on the best way to encode a contact-analog display, and the fundamental question of whether contact analog is better than symbolic presentation was answered in the negative (Blair and Plath, 1962). Perhaps it should not be surprising that symbolic displays are better than contact-analog displays for the multidimensional tracking problem of vehicular control. Many operational tracking problems are quantitative in nature, like "Hold altitude of 10,000 feet," and this type of problem is admirably suited to symbolic displays. Moreover, a pointer on a symbolic instrument is a simple and accurate way to show error, and the operator can see it easily and correct it, but the contact-analog display is more complex and it seems to connote deviation from a normative state rather poorly, as the work of Matheny and his colleagues, and Sidorsky, have shown. In defense of contact-analog displays, it must be said that they have not been assessed in more complex interpretative situations, where rapid knowledge of the system and its relationship with the external world are needed. Interpretability for decision, rather than tracking, may be the merit of the contact-analog display, just as for the situational display. But if this is to be the strength of contact-analog displays, it will take an experimental program to prove it, and symbolic instruments still will have to be retained for moment-to-moment tracking.

PRACTICE EFFECTS AND THE DESIGN OF VISUAL DISPLAYS Engineering psychologists often ignore the interaction of their variables with learning. A typical approach is to conduct an experiment and then assume that the relationships are immutable and stand firm as a guide that can be written into human engineering handbooks for the design of equipment. An assumption of most research on visual displays is that the human behavior under scrutiny represents highly overlearned responses from a lifetime of practice in interpreting visual scenes, and that it is fair to assume an asymptotic learning curve. Implicit in research on visual displays is the notion of *population stereotypes* (Fitts, 1951, p. 1306), or the response preferences in a population from past learning. To know about past learning and to capitalize on it is important, but there should not be sole dependence on it. While it can be granted that many response classes of interest to engineering psychologists are at the asymptote of their acquisition curves, other response classes may show marked effects of practice.

The engineering psychologist is interested in the interaction of learning and task design variables for several reasons. The goals of any system cannot be discussed meaningfully without including the training of personnel, and the engineering psychologist is interested in knowing the conditions under which individuals learn most rapidly and achieve the highest final level of performance. Also, there may be interactions of task variables with practice. A variable may be associated with inferior performance at one stage of practice and superior performance at another. There have been a few instances in research on visual displays that have studied and reported practice effects. Christensen (1952), in his tachistoscopic study of dial reading variables, found significant interaction effects from day 1 to day 2 of practice. Kappauf (1947) gave a test and a retest session in an experiment on dial reading variables. He found interactions attributable to practice effects, and he concluded that practice is a factor which must be studied in future experiments. Loucks (1947, p. 126) reported an experimental comparison of inside-out and outside-in aircraft attitude indicators.

The inside-out attitude indicator is the conventional one, still in general use, and it had as its moving element the earth's horizon line. The outside-in display had the moving element operating in synchrony with the airplane's wings, and a fixed horizon line. This latter display functioned as if the pilot was outside the system and watching the aircraft's wings move with respect to the stable horizon line. Loucks used a flight simulator as his research device, with a simplified panel that had just the experimental instrument. Air Cadets, relatively inexperienced in flying, were subjects. The outside-in version of the attitude indicator was significantly better for the first eight minutes of practice, but there was no significant difference for the second eight-minute period. Engineering psychologists are in need of more studies such as these on the interaction of practice and the equipment variables that are of interest to them. A display which promotes poor human performance at the outset may be quite satisfactory after enough practice.

HUMAN TRACKING BEHAVIOR

DEFINITION OF TRACKING Adams (1961) gives this general definition of a tracking task:

1. A paced (i.e., time function) externally programed input or command signal defines a system requirement for the operator, which he meets by manipulating a control mechanism.

2. The control mechanism generates an output signal.

3. The input signal minus the output signal is the tracking error quantity, and the operator's requirement is to null this error. The mode of presenting the error to the operator depends upon the particular configuration of the tracking task but, whatever the mode, the fundamental requirement of error nulling always prevails. The measure of operator proficiency ordinarily is some function of the time-based error quantity.

In simplest terms, tracking is keeping two indicators aligned by manipulating a control. We are all familiar with many different tracking tasks. Using the steering wheel of an automobile to hold alignment on the road is a good example. Controlling an aircraft, submarine, or space vehicle by maintaining alignment of pointers on instruments, are other examples. Wherever tracking is found, it can be conceptualized as a *closed-loop* task, where the display presents a response requirement to an operator, a corrective movement is made with the control, the consequences of the response appear on the display, which in turn may or may not create a new response requirement, and so on. This continuous process of displaying, correcting, redisplaying, and recorrecting, is the basic nature of a closed-loop task, and it is distinguished from an *open-loop* task where an operator's response has no influence on the display. No matter what the subject's response, a display derived from a motion picture film would be unchanged.

Tracking behavior stands as one of the early interests of engineering psychologists, and properly so. Human operators have played a central role in the continuous control of a number of major man-machine systems. Because of this, tracking is one of the best developed research areas in engineering psychology. Dramatic engineering advances in the development of automatic control devices since the Second World War are, however, sharply reducing the role of man as a continuous controller in many man-machine systems. A well-known example is the autopilot, which holds an aircraft on a predetermined set of basic flight values. Nevertheless, a reduction in the amount of time that is spent in tracking should not lead to the conclusion that human operators will be eliminated for continuous controlling. Even with automatic control, the human operator will be important for unusual tracking sequences that cannot reasonably be automated, and for redundant reliability purposes when automatic control subsystems fail and the human operator must assume manual control.

RESEARCH METHODS IN TRACKING Laboratory tracking tasks are often simplified versions of many of the tracking situations encountered in the real world of man-machine systems, but this should not imply that they are intrinsically simple. The simplest tracking task found in psychological laboratories is the Rotary Pursuit Test (Melton, 1947). The continuously moving target and the continuous requirement to null the error discrepancy between the stylus tip and the target fully qualify the Rotary Pursuit Test as a tracking task. However, tracking tasks used by engineering psychologists are almost always more complex than the Rotary Pursuit Test. It is common for their tracking tasks to use more elaborate mechanisms, like analog computers, which can allow for the controlled manipulation of such variables as the mathematical function for the input signal, scale factors, mathematical transformations of the output signal, and delays (lags) between control movement and changes on the display. When applied research is directed toward immediate problems about tracking tasks of a particular man-machine system, an attempt is often made to simulate in the laboratory as many of the properties of the system's tracking task as is feasible. Thus, if the problem is the multidimensional tracking task of flying an aircraft on instruments, the laboratory device may be a flight simulator with a full array of realistic instruments and controls. The choice of laboratory tracking tasks for generalization to real systems is the same methodological one as was discussed for visual displays. Research on complex tasks, which are realistic, may yield better generalizations to their parent systems. On the other hand, because of interactions and variables that we do not fully understand, results from complex tasks can be relatively unique and often of minor use for another system with a different tracking task. In a basic research sense, it profits us to amass data at different levels of complexity to build an understanding of as many relevant variables as we can. Our basic scientific knowledge of simple and complex tracking has a long way to go, but there has been a relatively large number of tracking studies, and a fairly sound understanding is beginning to emerge for both simple and complex tracking tasks.

BASIC TYPES OF TRACKING TASKS *Pursuit Tracking.* Fig. 9-5 has an example of a pursuit tracking display, which is one of two basic configurations of tracking tasks. The display has two moving indicators. One is the input signal, which is the command to the operator and is actuated by external influences on the system. The other is the output indicator which is actuated by the operator's movement of the control. The difference between the two indicators represents the operator's error in achieving the goals of the system, as defined by the position of the input indicator, and he manipulates his control to correct the error and keep it as close to zero as possible. There are two advantages to pursuit tracking, which has the input and the output displayed separately. One

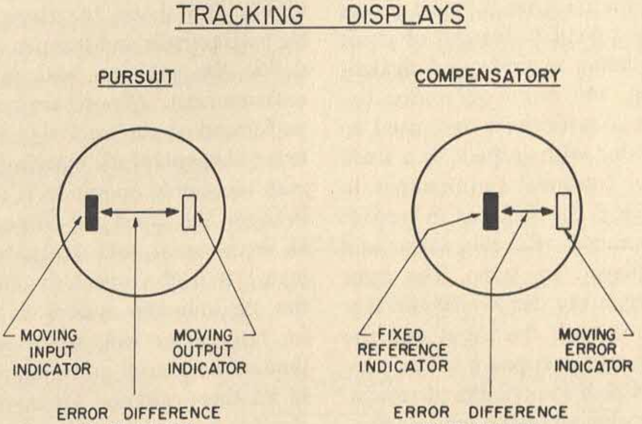


Fig. 9-5. Pursuit and compensatory modes are the two basic ways of presenting information in visual tracking tasks. Also shown are the three fundamental signals that operate in a continuous tracking task. The error signal, derived as the difference between the input and output signals, is ordinarily utilized to produce measures of human tracking proficiency.

is that the separate output indicator ensures good knowledge of results, because the operator always sees the consequences of his control movement. The other advantage lies with the separate input indicator. Being able to see the unimpaired action of the input signal allows the operator to learn prediction or anticipation of whatever regularities exist in the input signal (Adams, 1961; Adams and Creamer, 1962*a*, 1962*b*; Bartlett, 1951; Helson, 1949; Poulton, 1952*a*, 1952*b*, 1957*a*, 1957*b*, 1957*c*). By being able to predict the course of the input signal for the near future, such as a reversal of direction, the operator can get his corrective control action underway slightly before the actual signal change takes place. The result is reduced error and, ideally, with perfect anticipation, no error. When anticipation is absent, the operator must wait for the change to occur on the display, and his response will have error at least proportional to his reaction time. All input signal patterns do not allow prediction, but there are often regularities that can be learned, and anticipatory behavior can function to minimize overall tracking error and optimize performance.

Compensatory Tracking. Figure 9-5 also shows a compensatory tracking display. Instead of input and output indicators being separate and distinct as in pursuit tracking, the error difference between input and output is derived and used to actuate an error indicator with respect to a fixed reference marker. The continual requirement to null the error difference is the same as in pursuit tracking, but the advantages of seeing input and output separately displayed are gone. The error difference is the only quantity displayed, and it is a compound function of both the input and the output signal. Neither signal appears in its pure form. A consequence is poor knowledge of results, because any particular change of the error indicator might be due either to a change in the input or the output. Anticipation of regularities in the input signal are harder to learn for the same reason.

Relative Merits of Pursuit and Compensatory Tracking Displays. Because of a better display for anticipation and knowledge of results, pursuit tracking is often superior to compensatory tracking (Chernikoff, Birmingham, and Taylor, 1956; Hartman and Fitts, 1955; Poulton, 1952*b*; Senders and Cruzen, 1952). However, several other studies have established that the superiority of pursuit over compensatory tracking holds only for higher frequencies of the input signal. When the input frequency is low, compensatory tracking is as good as pursuit tracking (Chernikoff, Birmingham, and Taylor, 1956; Chernikoff and Taylor, 1957; Obermayer, Swartz, and Muckler, 1961). A probable reason is that changes on the display happen quite slowly when the input frequency is low, and a change in the input signal often occurs when the operator is doing little or nothing with his control. The result on the compensatory display is a reduced confounding of input and output signals and a better chance to learn to anticipate regularities in the input signal. Poulton (1952*b*) has shown that anticipatory behavior occurred with practice in compensatory tracking when a simple harmonic input signal was used, but that it was less than that found for pursuit tracking. Reasoning similarly, we might surmise that low input frequencies cause a control movement to be more readily discernible, and the operator has fairly good knowledge of results because his motor movements are momentarily of a higher frequency than that of the input signal (Adams, 1961, p. 71).

MEASUREMENT OF TRACKING The lower part of Fig. 9-5 shows the three fundamental signals for both pursuit and compensatory tracking, which define the problem and provide the basis for measurement. Almost always, some operation is performed on the error signal as a means of measuring the operator's tracking proficiency. A common electronic operation is to take the difference between the input and output signal, which yields an error signal, and integrate (continuously summate) it with respect to time. Sometimes, when the experimenter wishes to penalize the subject for large error values, the error function is continuously squared and integrated. Time on target is another common measure of tracking proficiency. A symmetrical scoring zone or target is defined around zero error, and a timer is activated whenever tracking error falls within the zone. Usually an arbitrary decision is made by the investigator to use only one of these measures. Bahrick, Fitts, and Briggs (1957) have pointed out that time-on-target has the methodological problem of yielding different performance functions depending upon the scoring zone that is chosen.

Webber and Adams (1960) have suggested using an analog-digital data system that rapidly samples the continuous error voltage, encodes and digitizes it, and permanently records the values on punched paper or magnetic tape. This basic digital record is then submitted to a digital computer with appropriate programs to derive almost any tracking error measure that is desired. The digital error quantities can be integrated, squared and integrated, or used to produce time on target for hundreds of scoring zones if desired. Moreover, there is a permanent record of tracking performance, and it can be reanalyzed for new measures whenever the need arises. Another advantage is that the high speed of a digital computer readily allows time series analyses, like autocorrelation and crosscorrelation (Adams and Webber, 1961; Fitts, Bennett, and Bahrick, 1956), that are prohibitively time-consuming by most other methods.

CONTROL SYSTEM DYNAMICS *Control dynamics* for a system is defined as the relationship of the system's output and changes on the display as a function of the operator's movements of the tracking control. These transformations define the operator's response patterns to correct the displayed error and accomplish a given output for the system, and they can be expected to have important implications for human tracking proficiency.

Integration. The relationship between output of the system and position of the control is basically expressed by integration of the control's output signal, and the degree of integration defines the *control order* for the system. The three basic control orders are position, velocity or rate, and acceleration. Figure 9-6 shows these three orders and their implications for tracking responses.

The basic schema for a tracking task is shown in the upper part of Fig. 9-6, where a step input signal has been chosen to displace the display indicator and create the tracking problem. The operator displaces his control in amount X_c to correct the displayed error, and the system responds in an amount X_s. Tracking, being a closed-loop operation, has the consequences of the system's action fed back to the display. If the response is completely satisfactory, the error is nulled, and the operator has no additional response requirements for the moment. However, if the response is inadequate, some error will remain, and the operator will have an additional response requirement.

Position control, schematized in Fig. 9-6, is simplest of all, and it is a linear and direct relationship between control movement and output of the system. The operator moves his control a specified amount, and the system moves a proportional amount X_s, where the constant of proportionality is K.

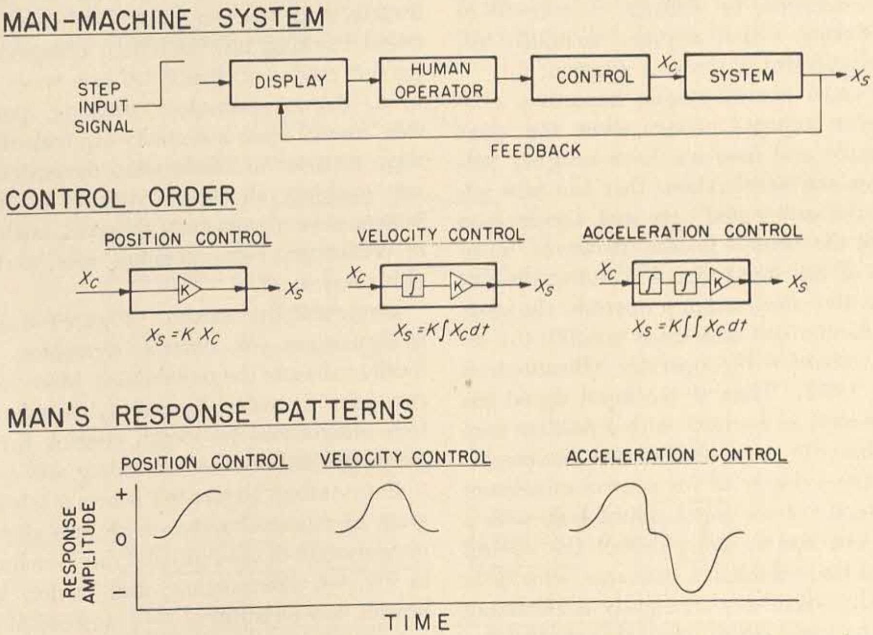


Fig. 9-6. Definitions of position, velocity or rate, and acceleration tracking. The implications of each for response sequence also are shown.

The velocity control performs one integration of X_c. The integration of X_c produces a change in the rate of X_s. Thus, if the operator introduces a step input into the control system, continuous integration with respect to time of this constant value induces a steady rate of change in the system. This is contrasted to the position control, where movement of the control causes a proportional movement of the system, but it does not effect its rate. A rate change of X_s with a position control requires a rate movement of the control by the operator. The velocity control, on the other hand, requires only a step displacement of the control to induce a rate change in X_s, and the greater the displacement the greater the rate. Response to the step input in Fig. 9-6 requires a displacement of the velocity control to establish the rate of movement, and then a reversal of the control to the neutral position to stabilize in the new position.

An acceleration control, shown in Fig. 9-6, performs two integrations and a step input to the control system produces steadily increasing rate, or acceleration, for X_s. One integration gives a constant velocity, and two integrations give a constantly increasing velocity, or constant acceleration. Control movement in response to the step input signal establishes an acceleration for the system, and a return to the neutral point holds a constant acceleration, or velocity. A reversal of the control system is then required to nullify this velocity and stabilize at the new position.

A higher-order control system imposes a relatively complex response pattern when the input signal is simple and does not have complex patterns of rates and accelerations that can take advantage of the automated rate and acceleration properties of the control system. However, when the pattern of the input signal is more complex and has rate and acceleration properties, the more complex dynamics can sometimes simplify the responses or *unburden* the operator (Birmingham and Taylor, 1954). Thus, if the input signal has constant velocity, an operator with a position control would have to make skilled rate movements and match the velocity of his control movement with the velocity of the input signal. But, with a rate control, he merely must deflect the control in an amount that establishes the same velocity as the signal. Or, consider a constantly accelerating segment of the input signal. Here the greatest unburdening comes with the acceleration system, because the human operator need only deflect his control a fixed amount to achieve a constant acceleration. Tracking a constantly accelerating missile with an optical sight would benefit from an acceleration control. The velocity control, on the other hand, must be moved skillfully at a constant rate to match the constantly increasing velocity of the missile, and the position control must be constantly accelerated by the operator. More complex dynamics of a control system can, therefore, under some circumstances, relieve the human operator of making intricate, time-sequenced response patterns, but it is not a panacea. Whether or not complex dynamics will unburden the operator is fundamentally dependent upon the nature of the input signal. Figure 9-6 shows a case where higher control orders increase the complexity of responding.

There has not been much research comparing various control-system dynamics, but the work that has been done indicates that the position control is best when quasi-realistic, complex input signals are used. Lincoln and Smith (1952) compared velocity and position controls in a pursuit tracking task. A complex input signal was used, with nine reversals of direction, and continuously changing velocity and magnitude of movement in each phase. Position control produced about twice the accuracy of velocity control. Obermayer, Swartz, and Muckler (1961) used a one-dimensional tracking task and both compensatory and pursuit modes of display. A sine wave input was used. For compensatory tracking, position and rate control were essentially equivalent, but both were superior to acceleration dynamics. For pursuit tracking, all three dynamics of the control system were significantly different, with the order of proficiency being position, rate, and acceleration control.

AIDING AND QUICKENING Figure 9-6 shows three configurations of control dynamics, but this hardly exhausts the possibilities. More integrations can occur between X_c and X_s. A submarine has four integrations for depth control, for example. Other possibilities are rate aiding and quickening.

Rate Aiding. Figure 9-7 shows a schematic diagram of a control system with rate aiding, and it is a variation of conventional rate dynamics shown in Fig. 9-6 (Birmingham and Taylor, 1954; Ely, Bowen, and Orlansky, 1957). Instead of a velocity output for a control displacement, as shown in Fig. 9-6, a feed-forward loop is used around the integrator, and the result for X_s is a change in both position and rate. The ratio of position sensitivity to velocity sensitivity (K_1/K_2) is called the *aiding time constant.* Fitts (1951, p. 1334) gives a good example of aiding when he says "If a watch had a single adjustment that would set the hands ahead when the watch had lost a few minutes and also cause it to run faster thereafter, the adjustment would make use of the 'aiding' principle." Rate aiding can benefit compensatory tracking (Helson, 1949), but it can be detrimental to pursuit tracking (Lincoln and Smith, 1952). Chernikoff and Taylor (1957), however, have shown complex interactions between pursuit and compensatory displays, frequency of the input signal, and values of the aiding time constant. It is evident that no simple recommendation can be made about aided tracking. As with other configurations of control dynamics, rate aiding may or may not simplify responding, depending upon the nature of the input signal. A search for the best overall dynamics of the control system, or the optimum aiding time constant, probably will not be successful. We appear to be faced with the more demanding scientific task of understanding interactions between control system dynamics, characteristics of the input signal, and probably a number of other variables as well. The best dynamics and aiding time constants appear to be quite specific to a system.

Quickening. There are delays, or lags, in complex systems. An operator's input to a control system does not always lead to an immediate response of the system and a change on his display. Delays often are inherent in the mechanics and electronics of systems, or they can be a function of the medium through which the vehicle is traveling. Integrators and filters in complex control systems also introduce delays. One of the consequences of lags is a degree of instability in the system, because the operator cannot immediately see the effects of his responses, and he tends to overshoot and undershoot as he "hunts" and attempts to find the correct position. Several research studies have experimentally verified that tracking proficiency steadily decreases as time delay increases (Conklin, 1957; Levine, 1953*a*, 1953*b*; Warrick, 1949).

Skilled operators can learn to anticipate to a degree the consequences of a control movement when lags are present, but a better solution is to use quickening, where feed-forward loops are used to provide more immediate knowledge of control action, as well as to simplify the requirements for response presented on the display (Birmingham and Taylor, 1954; Ely, Bowen, and Orlansky, 1957). Figure 9-7 uses a velocity control to illustrate quickening, although quickening will often be used for higher-order dynamics too. By tapping off a signal for control movement prior to the integration, and summating this with the velocity output signal, the operator receives im-

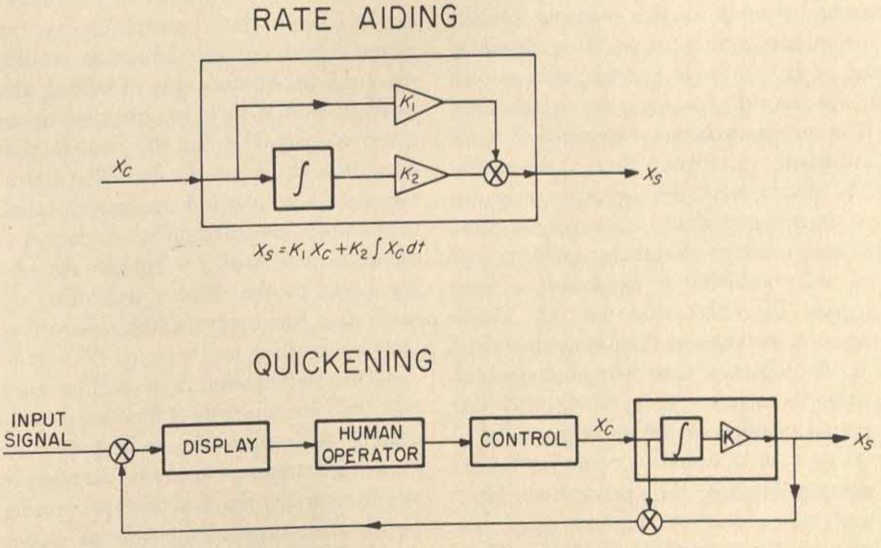


Fig. 9-7. Definitions of aiding and quickening in a rate tracking task.

mediate position control information added to the velocity output of the system, X_s, on the display. The quickened output indicator can be moved immediately in alignment with the command or reference indicator, and the operator merely must hold the two indicators in alignment with simple proportional responses essentially of the kind that he would use in position controlling. Quickening does not alter the operator's response pattern. The basic response pattern for a particular type of dynamics for a control system, such as shown in Fig. 9-6, is unchanged. Quickening has been established as a means of dramatically improving tracking performance (Bailey, 1958; Birmingham, Kahn, and Taylor, 1954; Holland and Henson, 1956; Sweeney, Bailey, and Dowd, 1957).

There are two additional points that should be made about quickened displays. The first is that quickening only influences information on the display, and it is contrasted to the use of a feedforward loop in rate aiding (Fig. 9-7), where both output of the system and displayed information are affected. The second point is that quickening, in bypassing delays in the system, introduces a discrepancy between actual state of the system and information displayed to the operator (Ely, Bowen, and Orlansky, 1957, p. 48). This is an often overlooked shortcoming in papers that extol the virtues of quickening. Because a complex system like an airplane or a submarine requires the operator to be constantly aware of relationships between his system and the outside world, it is necessary to provide instruments in addition to the quickened display for this purpose. Quickening can be advantageous for tracking displays, but a misuse of it can leave the operator out of touch with his world. In applying quickening principles it is necessary to keep in mind that an operator customarily does more than make motor corrections to displayed data; he also must be aware, in a larger cognitive sense, of system-environment relationships. Sidorsky (1961) was cognizant of this problem in proposing a new symbolic display for submarine control. Three symbols were on a rectangular display with course and depth as coordinates. One was a command symbol, another a quickened symbol, and the third gave actual position of the vehicle.

PROPERTIES OF THE TRACKING CONTROL Tracking controls manipulated by the operator can have many different characteristics, depending on the system and the predilection of the designer. They can take the form of a steering wheel in an automobile, cranks on a lathe, or a control stick on an aircraft. Controls are often multidimensional. The steering wheel of a car is a one-dimensional tracking control for turning in a horizontal dimension, but an aircraft stick is a two-dimensional control that both banks and turns the aircraft with left-right movements and establishes climbing and diving rates with backward-forward deflections. More dimensions than two have on occasion been used in complex multidimensional tracking tasks. This section will not discuss which type of control is best, such as whether a wheel control is better than a stick control. This omission is regrettable, but it is imposed by an absence of good data. It would seem better, therefore, to discuss general properties of controls that might be applied to many configurations.

Control Scale Factors. The issue is how far the control should be moved to accomplish a given amount of change on the display. This relationship is commonly expressed by the *control-display ratio*, which is a ratio of the amount that a control is deflected to the amount of change on a display (Rockway, 1954). Consider a display with a linear scale that is 10 in. long, to be used in a one-dimensional tracking task with a stick control. A high control-display ratio of, say, 1:1 could be defined as 1° deflection of the control to produce 1 in. of change on the display. A relatively large deflection of 10° would be required to move a pointer the full 10-in. scale, so this would be a low-sensitivity control. A high sensitivity control might have a low control-display ratio of 1:5, where 1° of control deflection would move the pointer 5 in. Another way of talking about control-display ratio is in terms of *gain*, or amplification given the signal from the control that serves to move the display indicator. The units expressing amount of gain stand opposite to those used for expressing control-display ratio. A high gain means that a small control movement will give a big boost to the display indicator, which is the same as a low control-display ratio for a sensitive tracking control.

It is obvious that a very low control-display ratio will be so sensitive that merely touching the control will cause the indicator to deflect, and even hand tremors would be hopelessly magnified as jitter on the display. But presuming that such an absurdly low control-display ratio would be avoided, what should the ratio be? The gist of all the research in the area is that the control-display ratio should be moderately high, so that generous amplitudes of control movements are required. In other words, the tracking control should have moderate to low sensitivity. This conclusion has been established for both one- and two-dimensional tracking tasks. For one-dimensional tracking, Hartman and Fitts (1955) found larger movements of the control uniformly better for both compensatory and pursuit tracking, different display scale factors (allowable indicator movement), and five different forms of the input signal. Hartman (1957), using a one-dimensional tracking task, found that larger control movements gave higher time-on-target scores than smaller movements. Rockway (1957) and Rockway and Franks (1959) used a one-dimensional tracking task and found superior performance with higher control-display ratios.

Two-dimensional tracking emerges with the same trend. Rockway, Eckstrand, and Morgan (1956), found that ratios of 1:3 and 1:9 produced higher performance levels than 1:27. In a companion study, Rockway, Morgan, and Eckstrand (1958) found that a 1:3 ratio gave significantly higher performance than 1:18. Briggs, Fitts, and Bahrick (1957), using a two-dimensional tracking task that simulated aircraft dynamics, found that increasing the amplitude of control movement by a factor of four produced better tracking performance. Andreas, Gerall, Green, and Murphy (1957) also found high control-display ratios best for two-dimensional tracking.

Obviously there must be a limit to the largeness of the control movement in relation to movement of the display indicator. Helson (1949) in a report of a comprehensive series of studies on one-dimensional tracking with a hand-wheel control, found that a U-hypothesis applied to a number of task variables. This hypothesis holds, as its name implies, that optimum performance is at an intermediate range of values for a variable, and that both smaller and larger values result in poorer performance. He found that a moderately low sensitivity of 50-200 rpm for hand-wheel turning was optimum, and that performance was noticeably poorer when sensitivity was increased or decreased. The U-hypothesis is a good statement relating tracking performance to control sensitivity, but it solves no specific control-design problems because it is a general hypothesis. Each control sensitivity problem must be solved empirically, but the U-hypothesis offers the general guidance that proper sensitivity values will lie neither in the high nor the low range. All the empirical data indicate that optimum sensitivity requires a substantial amount of control movement relative to display change.

Control System Variables Defining Proprioceptive Feedback. Proprioceptive stimulation in tracking is internal bodily stimulation that arises from force exerted on the control. It is hypothesized to provide information on the position, rate, and acceleration (and maybe higher derivatives) of control movement, and it is considered to be a valuable supplement to position, rate, and acceleration information in exteroceptive environmental stimuli. Just as environmental stimuli provide information for regulating responses, so proprioceptive stimuli provide similar information from internal sources. How this regulation actually occurs is not at all clear. Both intermittent and continuous internal control of motor responses are currently entertained as hypotheses in the research literature (Adams, 1961).

It must be admitted that our present knowledge of proprioceptive stimuli is meager. We know very little about proprioceptive stimuli because they are under the skin and difficult to manipulate for psychological and physiological study. Vision and audition are thoroughly researched sense modalities, and our knowledge in these fields represents some of the most sophisticated data in psychology, but a scientific understanding of the proprioception modality ranks far beneath them. Nevertheless, we have reason to be convinced of their importance. Using a two-dimensional compensatory tracking task, Gibbs (1954) reported tracking data from a patient with *tabes dorsalis*, which is an impairment of proprioceptors. The tracking records had a wild, oscillatory quality. The patient would drastically overshoot or undershoot as he attempted to correct error, apparently because he had no internal stimulus information from movement of his limb to tell him what his arm was doing. He had visual feedback, but this was not sufficient. It would seem that an operator must not only have visual indication of the adequacy of his responses, but also internal stimulus guidance to inform him of the moment-to-moment position of his limb.

Because of the problems in manipulating the almost inaccessible proprioceptive stimuli for experimental purposes, the main approach has been molar, where characteristics of the control system that are assumed to influence proprioceptive stimuli have been manipulated. These variables are mass, viscosity, elasticity, friction, and extent of movement. Bahrick (1957) has hypothesized that the physical variables of elasticity, damping, and moment of inertia for a control system improve an operator's perception of position, rate, and acceleration of limb motion, respectively, and Bahrick, Fitts, and Schneider (1955) and Bahrick, Bennett, and Fitts (1955) found preliminary evidence for this general view with nontracking tasks. Bahrick's hypothesis does not consider the effects of friction or extent of control movement. Extent of control movement is a well-known determiner of tracking proficiency, as was discussed in the previous section, and its advantages appear to come from improved proprioceptive stimulation when movements are relatively large. Friction has been given little study. Muckler and Matheny (1954) employed a brake drum to manipulate friction for the control of a one-dimensional pursuit tracking task. Six levels of friction were used, and no significant differences were found. Helson (1949), however, found friction a variable whose increase markedly reduced efficiency in handwheel tracking.

For tracking tasks, elasticity (spring tension), extent of control movement, and inertia are established as the best determiners of proficiency that can reasonably be ascribed to proprioceptive stimuli. Using a one-dimensional pursuit tracking task, Howland and Noble (1953) studied separately and in combination the variables of spring tension, viscous damping, and moment of inertia for the control. The best performance of all was spring tension alone on the control. Briggs, Fitts, and Bahrick (1957) used a two-dimensional compensatory tracking task with airplane dynamics and studied two degrees of spring tension and two amounts of control amplitude. The largest amounts of spring tension and amplitude of movement were found to yield best tracking performance. At least for handwheel tracking, added inertia decreases tracking error (Helson, 1949), and it would seem reasonable to relate this variable to proprioceptive stimuli.

Adams and Xhignesse (1960), Adams (1961), and Adams and Creamer (1962*a*, 1962*b*) have advanced hypotheses and supporting empirical evidence that proprioception is also a factor in temporal anticipation, or timing, in tracking. Temporal anticipation is clearly important for tracking proficiency, but virtually no research has been conducted on mechanisms and variables for it. By being able to anticipate when a change in the input will occur, the operator can get his control movement underway slightly before the input changes, and he has a good chance of lessening the amount of error. The studies on spring tension and extent of movement that have been cited are presumed to determine the quality and quantity of proprioceptive stimulation, and Adams and Creamer (1962*b*) call this *regulatory proprioceptive stimulation,* where proprioceptive stimuli perform a regulatory or feedback function for the response and help the operator to discriminate a correct movement from an incorrect one. Here proprioceptive stimuli supplement the feedback from environmental stimuli. However, Adams and Creamer further hypothesize that proprioceptive stimuli can function as a mechanism for timing, and they refer to this as *anticipatory proprioceptive stimulation.* This hypothesis holds that the time-persisting proprioceptive aftereffects of an overt or mediated response is the mechanism underlying the timing of motor responses. The occurrence of a response at time t is assumed to generate a time-varying stimulus trace, and the trace at time $t + \Delta t$ is the cue set to which the operator learns the motor response that is anticipatory to the environmental event. The anticipatory response occurs before the actual environmental event, so the stimulus for it must arise from within the operator himself, and it is hypothesized that these cues are proprioceptive traces. Evidence for anticipatory proprioceptive stimulation might be found in relationships between measures of anticipatory timing and task variables that are considered to define proprioception. To test this hypothesis, Adams and Creamer used a one-dimensional discrete tracking task and studied the timing of responses to stimulus change as a function of (1) time between responses, on the assumption that the trace is decaying and has a more effective cueing quality at shorter intervals, (2) spring tension on the control, and (3) amplitude of movement. They defined as anticipatory those response times that occurred within the range ±117 msec from onset of the stimulus, and these reaction times shorter than the expected optimum for reaction time of about 150 msec could reasonably be assumed to have an anticipatory mechanism functioning. Helson (1949) apparently was the first to suggest this means of defining the presence of anticipatory mechanisms. Adams and Creamer concluded in behalf of their hypothesis, because they found that anticipatory performance was better with spring loading and with shorter time intervals. The amplitude of control movement did not affect anticipation. Covert verbal time-keeping responses, such as counting, conceivably might account for the effect of time between responses on anticipation, but it is hard to account for the positive effect of spring loading in this fashion. Spring loading presumably enhances the characteristics of the proprioceptive trace and thus time keeping.

MULTIDIMENSIONAL TRACKING A multidimensional tracking task is defined as having two or more stimulus sources and a dimension of the control system for response to each. Everyone is familiar with multidimensional tracking tasks. Examples are airplanes, submarines, and space vehicles. There has been very little systematic research on how to organize the dimensions of multidimensional control mechanisms, so the discussion here will focus on display variables as they relate to tracking. An earlier section of this chapter dealt with methods of displaying visual data for various purposes, and much of that section is relevant here because it bears on the efficiency with which the human operator apprehends the signals that elicit and guide his tracking responses.

A fundamental consideration for multiple instruments of a multidimensional visual tracking display is that they must be successively scanned with head and eye movements for errors to be corrected. The operator may successively correct each error as he sees it, or he may discern error quantities in several instruments before making a single, coordinated set of movements with his multidimensional tracking control, if he is sufficiently skilled to do so. In recent years this visual scanning activity has come to be called the *observing response*, or the intervening response that produces discriminative stimuli for the instrumental response (Wyckoff, 1952). Given Wyckoff's general definition, any response that serves to produce stimuli for subsequent instrumental responding can be considered an observing response, and it does not necessarily have to be the observing response of sense receptor orientation (Lutz and Perkins, 1960; Prokasy, 1956). However, sense receptor orientations, particularly head and eye movements for visual stimuli, are certainly the most prominent observing responses for engineering psychology because of their roles in the reception of stimuli for tracking (Adams, 1961) and the searching of visual displays in nontracking, vigilance monitoring tasks (Frankmann and Adams, 1962).

Number of Stimulus Sources. An important variable for tracking proficiency is the number of stimulus sources. In a nontracking study, Senders (1952) using a tachistoscope studied the effects of the number of dials on reading time and reading errors. The number of dials studied varied from one to forty-five. Her results showed that both reading time and reading errors were a linear increasing function of number of dials displayed. Jackson (1958) verified Senders' findings with a multidimensional compensatory tracking task. Each meter on the display had associated with it a rotary control knob. Number of stimulus sources was an experimental variable, and either two, three, four, or five stimulus sources and their respective controls were evaluated. Jackson found tracking error increased linearly with number of stimulus sources. The conclusion from Jackson's study, which is quite straightforward, is that the more stimulus sources to be scanned, the longer the time between sensing acts for the observing response at each source, and the longer the time available for tracking error to build up.

Spatial Separation of Stimulus Sources. As with number of stimulus sources, we might expect that the wider the spatial separation of visual sources, the longer the time between successive visual samplings of a source, and the greater the tracking error. The empirical data support this expectation. Using the Grether Dual-pursuit Apparatus, which is a compensatory tracking apparatus with two instrument dials and two rotary control knobs, Fitts and Simon (1952) found that time-on-target scores decreased as the separation of the two stimulus sources increased from 4 to 16 in. on the panel. King (1961), using a two-dimensional compensatory tracking task and an aircraft stick control, found that time-on-target scores were lower for a 10-in. separation of the two instruments than for a 3½-in. separation. This finding held for both sine wave and irregular input signals. Briggs and Howell (1959) studied spatial separation of stimulus sources with a two-dimensional compensatory tracking task. A two-dimensional stick was used as the control. The display was a 21-in. cathode-ray tube that had two reference circles, whose spatial separation could be varied. A small dot in each dimension was the error indicator, and the subject's task was to null the error by keeping the dots in their respective reference circles. From a viewing distance of 20 in, the reference circles were either 2.1, 4.2, 6.3, 8.5, or 10.7 in. in separation, and these separations subtended approximately 0, 6, 12, 18, 24, and 30° of visual angle. Sine wave inputs were used in both dimensions, and the input frequency was either 4, 8, or 12 cycles per minute. The results showed that tracking error increased as the separation of the stimulus sources increased. The deterioration of tracking proficiency as a function of spatial separation was about the same for all input frequencies.

Display Integration. Integrating display elements, so one display element serves two or more functions, would be expected to reduce the demands on the observing response and increase the level of tracking performance. Briggs and Howell (1959) also studied this method of displaying in two-dimensional tracking. One experimental condition used a single reference circle, with two error dot cursors for the x and y dimensions. A subject still had to scan for the position of each error cursor, but he had only to relate them to a single reference circle. Another condition had a single reference circle and a single error dot cursor which was actuated by the tracking error in both x and y dimensions. These two conditions tended to yield the best performance of all, and it would seem attributable to a reduced scanning requirement for the visual observing response. In a similar study, Charipper (1959) used a two-dimensional compensatory tracking task with an analog computer that simulated submarine dynamics in course and depth. Three different configurations of two-dimensional controls were used. The experimental variable was the number of display elements to indicate the two dimensions of error. A two-element display had two error dot cursors to be centered on a cathode-ray tube, one for the x dimension and one for the y dimension. A single-element display had one error dot cursor that was actuated in both x and y. The single-element display was found to be superior for all conditions. Sweeney, Bailey, and Dowd (1957) used an analog computer that stimulated helicopter dynamics. There were three dimensions to be controlled in this experiment. Two of the dimensions were on an aircraft stick, which controlled the longitudinal and lateral motion of the simulated aircraft. To the subject's left was a pitch stick, and up-down movement of this control affected the vertical or altitude coordinate. Of interest here is the aspect of their study which manipulated the number of instruments on the panel. In one experimental condition, three instruments were used, and in another condition two of the instruments were integrated into one, leaving only two instruments on the display. Greater precision in flight control was found for the display panel with the integrated instrument, and the authors suggested that the advantage derived from a simplification of the visual observing process.

Anticipation and Multidimensional Responding. Adams and Xhignesse (1960) used a two-dimensional pursuit tracking task with discrete stimuli to study three major variables in two-dimensional responding. The visual display had two stimulus sources of three display lights each, and the subject's task was to position his two three-position controls as the lights at each source were programed according to a probabilistic sequence that defined the degree of coherency, or predictability of the stimulus lights. Two levels of each of three variables were studied: spatial separation of stimulus sources, signal coherency, and speed of signal event change. Durations of the discrete stimulus events were either 1 or 2 sec. Stimulus sources were either adjacent or separated, to define two levels of spatial separation. Adjacent sources were aligned 5° on either side of the median plane of the subject's visual field, and the separated sources were symmetrically aligned 30° on each side and required a bit of head movement in addition to eye movements for easy viewing. The two amounts of stimulus coherency were high and low. In the high coherency series a subject always had an event class present in one of the stimulus sources that was perfectly predictable as to type and time of occurrence. The low coherency series lacked these stable regularities, and a subject had to scan the two sources continually and always be alert for stimulus change. It was hypothesized that tracking proficiency would deteriorate with separated stimulus sources, because of the greater action of the observing response required, and with increased rate of events because of higher level of activity required of the observing response. The coherency of the stimulus series might be expected to interact with spatial separation and rate of events, because there should be a decreased requirement for the observing response when subjects can anticipate what event will take place and when it will occur. Being anticipatory, the cue for the response must arise from a learned source within the subject himself, and consequently he does not always need to orient visually and sense the environmental stimulus event directly. The findings supported these hypotheses and revealed that tracking proficiency was positively related to the proximity of stimulus sources, which is consistent with other research on the separation of stimulus sources, and inversely related to the speed of event change when stimulus coherency was low. But, when coherency was high, only event rate was a differentiating variable. Spatial separation did not distinguish between the groups of either rate condition when coherency was high, and evidences of anticipation in the data confirmed that subjects were utilizing their acquired capabilities for predicting high coherency events to reduce the demands of visual scanning when stimulus sources were separated. Thus, when the input signals have sufficient regularities, anticipation serves to overcome some of the deleterious effects of spatial separation of sources. Jackson (1958) also found anticipation operating when two or three stimulus sources comprised the task, but he found it generally absent when four or five sources were used.

The defining conditions and the mechanism of anticipation are only beginning to be understood, but it is becoming evident that anticipation serves as a potent determiner of proficiency in multidimensional tasks by reducing the need for the observing response. Studies on the definition of temporal anticipation and its mechanism, which have been discussed earlier in this chapter, are relevant here (Adams, 1961; Adams and Creamer, 1962*a*, 1962*b*; Bartlett, 1961; Helson, 1949; Poulton, 1952*a*, 1952*b*, 1957*a*, 1957*b*, 1957*c*).

MATHEMATICAL MODELS OF TRACKING BEHAVIOR

Man, as a tracking element in a system, interacts with electronic and mechanical components, and the ideal system design would be based on detailed knowledge of the capabilities of men and machines, so that they could be integrated in a maximally complementary relationship. Eventually it should become possible to devise mathematical models of man-machine configurations to determine the properties of men and machines that would be best for achieving the system's goals. Mathematical description of tracking behavior would be only a part of the requirement from the standpoint of engineering psychology. Hopefully, techniques might be worked out for the complete mathematical description of machine characteristics and complex behaviors like multidimensional tracking, tracking that is time-shared with discrete response acts, effects of learning, fatigue, perception, motivational change, etc. Engineering psychologists customarily work with complex tasks and behavior, and mathematical tools must encompass these complexities to be useful.

Mathematical models for the design of man-machine systems are best characterized as empirical models, and they should be distinguished from models that enter psychological theory. A man-machine model focuses on mathematical descriptions of interacting men and machines, with the aim of deciding the properties of men and machines to achieve a system's goals. Mathematical description of behavior found in psychological theory, on the other hand, takes the form of a limited axiomatic set from which the empirical laws of behavior are deduced. Mathematical models for system design primarily would function at the level of empirical laws, and they would be less concerned with the mathematical statements that integrate them into theory. But whether the model building interest is empirical or theoretical, neither can advance very far without quantitative statements of behavior. Siegel and Wolf (1961) conducted a study where they used a digital computer and the Monte Carlo method (Guetzkow, 1962) in building a mathematical model of human response sequences in landing a jet aircraft on an aircraft carrier. One of the difficulties that they faced was inadequate quantitative data in the psychological literature for response elements of the relatively long behavioral sequence of interest to them. Nevertheless, they were quite successful in their undertaking, and their approach is encouraging for computer model building in engineering psychology.

What are the desirable properties of mathematical models for the complex behaviors of interest to engineering psychology? Mainly, they are four:

1. The model, in principle, must be able to accommodate the task and behavioral complexities that are commonly encountered in engineering psychology.
2. The model should be stochastic. Intra- and

intersubject variability make behavioral data fundamentally statistical.

3. The model must produce output data of human performance in meaningful units so that it can be validated against empirical data.

4. The defining variables for psychological states such as learning, perception, fatigue, and motivation, eventually must be incorporated in these models.

Servotheory. The most prominent attempt to describe tracking mathematically has used the feedback theory of closed-loop servomechanisms as a model for man-machine tracking systems (Adams, 1961; Bower and Schultheiss, 1958; Brown and Campbell, 1948; Ellson, 1949, 1959; Goode and Machol, 1957; Licklider, 1960). A closed-loop servosystem is an electro-mechanical error-nulling device that compares an input and an output signal and operates to reduce the error difference between them. Because this is what a human operator does in a tracking task, the servomodel has been hopefully eyed as a means for describing continuous human tracking behavior. Figure 9-8 shows the analogy that is commonly drawn between a servomechanism and human tracking. Servoanalysis mathematically states the transformation that the system performs in response to the input signal, and the transformation is expressed by a transfer function. Servotheory is a "black box" approach, and there is no direct concern about the specific nature of signal transformations that take place internally in the system. Some engineering psychologists have tried to use this black box approach for describing the human operator in a tracking task, and they have hoped to find the transfer function for the operator as he processes and responds to tracking error (Adams, 1961; Ellson, 1949, 1959; Licklider, 1960).

A principal problem for servoanalysis as a means of describing human tracking behavior is that the underlying mathematical assumptions fail to account for the characteristics of the human system. Briefly, the mathematics of servoanalysis are grounded in the assumption of linearity. This assumption holds that the system obeys the superposition theorem, which states that system response to the sum of a set of inputs is equal to the responses made to each input separately. Given response of the system to a number of separate input signals, response to a complex signal made up of the separate constituent signals can be predicted. Another assumption is that the system is time invariant—that is, the system does not make one class of transformation at time t and another class at time $t + \Delta t$. There are many examples of the human operator's failure to abide by the strict assumptions of linearity and time invariance. Learning, fatigue, and motivational changes are common examples of the human response properties changing as a function of time-dependent variables. Through learning or

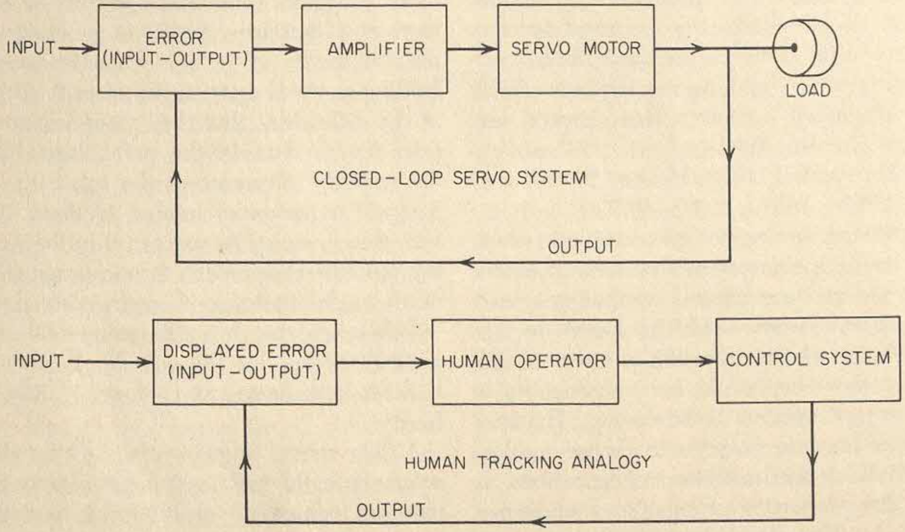


Fig. 9-8. Analogy that is commonly drawn between a servomechanism and the human operator in a closed-loop tracking task. From Adams (1961*a*).

fatiguing, for example, the operator will show one set of response characteristics at one time, and another set at another time. At best, therefore, servotheory might be applicable at the asymptote of performance for an operator with stable motivations and with widely spaced practice occasions to control fatigue effects. Evidences of human nonlinearity are experiments on the range effect. Studies by Craig (1949), Searle and Taylor (1948), and Ellson, Hill, and Craig (1949) have shown that responses to a series of discrete stimuli are governed by characteristics of the series as a whole, and that response to any given displacement is influenced by their relation to other displacement magnitudes in the series. It would appear that the human operator adjusts his responding in the direction of the expected size (Searle and Taylor, 1948, p. 626), rather than the actual size, and he lacks a standard response to any particular input. Birmingham and Taylor (1954), in a well-known paper on control system dynamics, emphasize the adaptable characteristics of man that make him quite unlike the machines described by servotheory.

The concern for applying servotheory to human tracking behavior has been an admirable effort to quantify tracking behavior, and efforts along these lines are still continuing in some quarters. A future development of nonlinear servotheory to accommodate these intricate adaptive propensities of man eventually may make servotheory a valuable tool for system design, but at present servotheory fails to conform mathematically to man's behavior. A more telling criticism is that it is hard to see how servotheory could ever be adapted to produce all of the four desirable properties of man-machine models of interest to engineering psychology that were cited.

Monte Carlo Model of Tracking Behavior. The various difficulties with the servoapproach is beginning to produce searches for other means of describing tracking behavior. Adams and Webber (1963) developed a statistical model, which utilized the Monte Carlo method and a digital computer to derive the model's implications. In an earlier paper (Adams and Webber, 1961) these investigators observed that tracking error records were characterized by *error peaks.* Each error peak appeared as the rising of an error value to a point of maximum, followed by a systematic decrease of the error to zero, or nearly so. While numerous operator and system factors function to increase tracking error, the human operator is the only systematic error-reducing entity in the system and, except for occasional fortuitous changes in the input signal that would serve to reduce error, all error nulling can be ascribed to him. The presence of an error peak, therefore, can be interpreted as the fundamental response act of sensing an error value and nullifying it with a corrective motor movement. This error peak was taken as the "unit" of tracking error and was used as the bit of behavior to be described mathematically, and from which long tracking response records were assembled from the model.

The Monte Carlo method is a statistical technique that is most commonly used where deterministic descriptions are not feasible (Guetzkow, 1962), and tracking behavior at this time appears to be one of them. The method, while not strictly dependent upon the use of computers, is made feasible by them. The computer is used to sample repeatedly from one or more probability distribution functions, perhaps subject these values to transformations, and then generate a distribution of output values which, for psychology, are response measures. The Adams-Webber model of tracking used the computer for sampling and mathematically relating quantities descriptive of the error peak. By successively generating error peaks, and relating the properties of peaks to functions of practice, characteristics of the input signal, and one- and two-dimensional tracking, they were able to generate simulated tracking error records that compared favorably with those generated by actual subjects.

Figure 9-9 shows the steps that Adams and Webber (1963) used in generating tracking records of synthetic subjects. Step 1 was to select a random number with the computer. The random number was used in Step 2 to select a value from a normal distribution of error peak magnitude values (E). This error peak magnitude was related to a time function in Step 3, which gave a value (t) for the time to rise to the E value. This function was empirically determined, and had the property of more time required for the system to achieve larger E values. Step 4 was the correction time for the human error-nulling response, and the greater the magnitude of error the longer the correction time, although the function is nonlinear. This correction time was called descent time (T), because it was the time taken to descend from the peak point of the error to near zero. The *T* function in Step 4 was empirically determined. In Step 5 another random number was generated, and in Step 6 it was used to define, in accord with an empirical in-time function (t_i), the amount of time that the cursors were kept closely aligned before another error began to develop. Step 7 defines a small band around zero error which is the in-time region, and the particular closeness of the subject's cursor alignment was again specified by a random number. These four fundamental values were used in Step 8 to define a linearized error peak, whose digital values were equally spaced in time. The random process of generating an error peak shown in Fig. 9-9 was continually repeated for successive error peaks, until a simulated tracking error record was defined for a hypothetical subject on a practice trial. For two-dimensional tracking, two series of concurrent tracking peaks were defined by this random process. The output of the computer was digital tracking error values on punched paper tape just as from human subjects, whose actual tracking performance was being recorded by an analog-digital data system, and both records could be given a common analysis and compared. The research issue was the comparability of simulated tracking error records and empirical ones from subjects. Would measures, such as time-on-target from simulated records, show correspondence with empirical time-on-target records from human subjects under various conditions of the task and practice?

Adams and Webber (1963) sought an answer to this question in an experiment that used a pursuit tracking task. Empirical data were collected for one-dimensional tracking with a regular signal (R), one-dimensional tracking with an irregular input signal (I), two-dimensional visual tracking with regular signals at each source (RR), and two-dimensional tracking with irregular signals (II). Forty practice trials were given so that

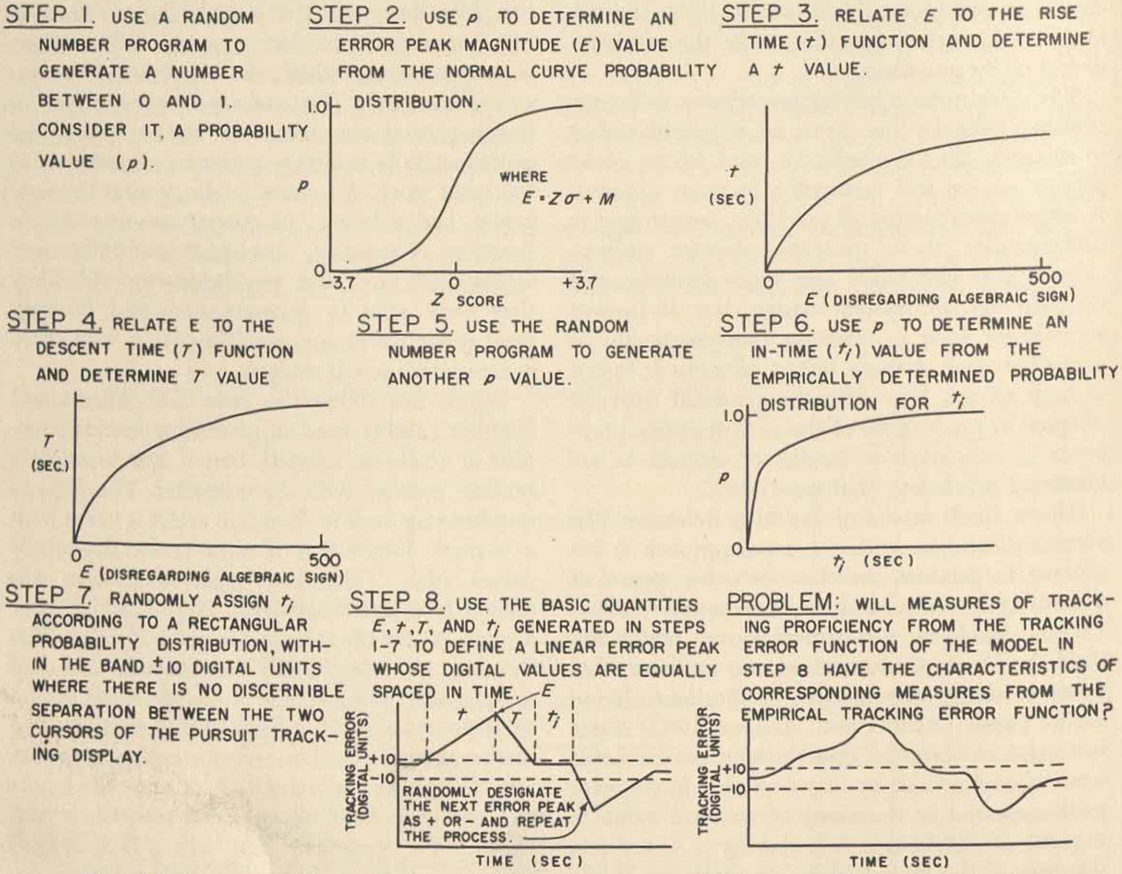

STEP 1. USE A RANDOM NUMBER PROGRAM TO GENERATE A NUMBER BETWEEN 0 AND 1. CONSIDER IT A PROBABILITY VALUE (p).
STEP 2. USE p TO DETERMINE AN ERROR PEAK MAGNITUDE (E) VALUE FROM THE NORMAL CURVE PROBABILITY DISTRIBUTION.
1.0
p
0
WHERE
E = Zσ + M
−3.7
0
+3.7
Z SCORE
STEP 3. RELATE E TO THE RISE TIME (t) FUNCTION AND DETERMINE A t VALUE.
t
(SEC)
0
500
E (DISREGARDING ALGEBRAIC SIGN)
STEP 4. RELATE E TO THE DESCENT TIME (T) FUNCTION AND DETERMINE T VALUE
T
(SEC.)
0
500
E (DISREGARDING ALGEBRAIC SIGN)
STEP 5. USE THE RANDOM NUMBER PROGRAM TO GENERATE ANOTHER p VALUE.
STEP 6. USE p TO DETERMINE AN IN-TIME (t_i) VALUE FROM THE EMPIRICALLY DETERMINED PROBABILITY DISTRIBUTION FOR t_i
1.0
p
0
t_i (SEC)
STEP 7. RANDOMLY ASSIGN t_i ACCORDING TO A RECTANGULAR PROBABILITY DISTRIBUTION, WITHIN THE BAND ±10 DIGITAL UNITS WHERE THERE IS NO DISCERNIBLE SEPARATION BETWEEN THE TWO CURSORS OF THE PURSUIT TRACKING DISPLAY.
STEP 8. USE THE BASIC QUANTITIES E, t, T, AND t_i GENERATED IN STEPS 1-7 TO DEFINE A LINEAR ERROR PEAK WHOSE DIGITAL VALUES ARE EQUALLY SPACED IN TIME.
E
t
T
t_i
TRACKING ERROR (DIGITAL UNITS)
+10
−10
RANDOMLY DESIGNATE THE NEXT ERROR PEAK AS + OR − AND REPEAT THE PROCESS.
TIME (SEC)
PROBLEM: WILL MEASURES OF TRACKING PROFICIENCY FROM THE TRACKING ERROR FUNCTION OF THE MODEL IN STEP 8 HAVE THE CHARACTERISTICS OF CORRESPONDING MEASURES FROM THE EMPIRICAL TRACKING ERROR FUNCTION?
TRACKING ERROR (DIGITAL UNITS)
+10
−10
TIME (SEC)

Fig. 9-9. Steps in a Monte Carlo model for generating probabilistic tracking error records (Adams and Webber, 1963).

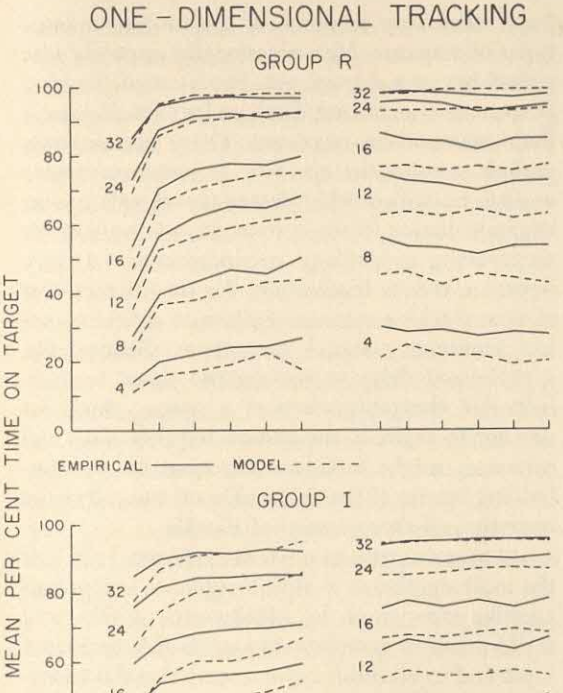


Fig. 9-10. For six time-on-target scoring zones, comparisons of empirical data from live subjects and of data from synthetic subjects that were generated by a Monte Carlo model and a digital computer. One-dimensional tracking. From Adams and Webber (1963).

functions showing the effects of practice could be obtained. Similarly, using the Illiac computer and a Monte Carlo model containing the four basic equations shown graphically in Steps 2, 3, 4, and 6 of Fig. 9-9, tracking error records were simulated for synthetic subjects under the conditions of regular and irregular signals, one and two-dimensional tracking, and forty practice trials—just as for empirical data. There were twelve actual and twelve synthetic subjects in each experimental condition. The constants in the four basic equations were changed to reflect changing properties of the error peak quantities as a function of the task and practice variables that were being investigated.

Figure 9-10 presents the results for actual subjects and synthetic subjects under conditions of one-dimensional tracking. Time-on-target data are shown for six scoring zones, regular and irregular input signals, and forty practice trials. The model approximates the empirical data reasonably well, and it appears to capture the trend of learning with good fidelity. Figure 9-11 shows the same type of data for two-dimensional tracking. Simultaneous time on target for the two dimensions is the performance measure. Again, the simulation was rather successful.

The Monte Carlo approach is only in the beginning stages for psychology, but it appears to show promise as a general technique for the mathematical description of complex man-ma-

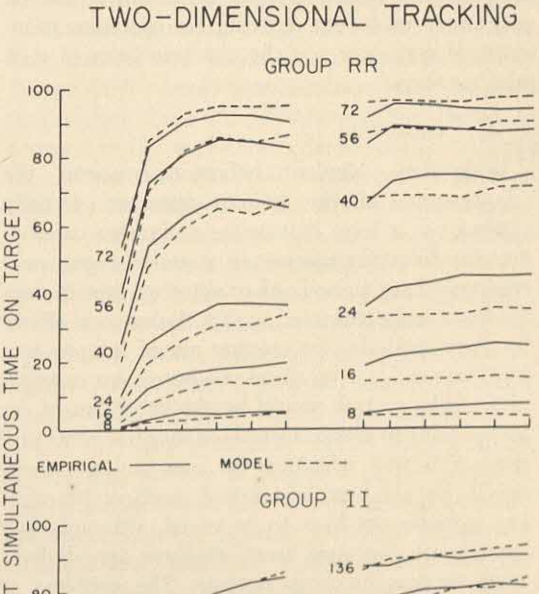


Fig. 9-11. For six time-on-target zones, comparisons of empirical data from live subjects and data from synthetic subjects that were generated by a Monte Carlo model and a digital computer. Two-dimensional tracking. From Adams and Webber (1963).

chine systems. Adams and Webber (1963) have shown that it is capable of doing a good job of handling the complexities of one and two-dimensional tracking and practice effects, and there is no reason why increased behavioral complexities cannot be incorporated in it. Among these can be n-dimensional tracking tasks, the interplay of tracking and discrete responses, task variables such as effects of different displays and controls, and psychological state variables such as fatigue and motivation. Siegel and Wolf (1961) incorporated stress in their Monte Carlo model of an aircraft landing. How well the model will contribute to the design of man-machine systems will be dependent upon the accuracy of the empirical laws of behavior that enter the model. As these laws become better known, Monte Carlo models may prove useful for deriving the optimum properties of machines and the characteristics of men who use them.

HUMAN VIGILANCE

DEFINITION Vigilant behavior concerns the attentiveness of the human operator. Usually vigilance is a term that refers to human capabilities for detecting changes in stimulus events over relatively long periods of monitoring, but it does not have to be restricted to that. Behavioral effects for short periods of attending are of interest too, but they will not be discussed here.[2] An example of a vigilance task would be the requirement for an operator to watch a display for three hours and press a button whenever he sees an instrument needle deflect from a standard position. Monitoring tasks do not have to be visual, although they usually are, because visual displays are so dominant in man-machine systems. The problem of attention can be raised for any sense modality.

SIGNIFICANCE OF VIGILANCE FOR MAN-MACHINE SYSTEMS The human operator always has had a role as monitor of signals in man-machine systems, even when systems were predominantly manual. Witness the monitoring requirements for a driver of the commonplace automobile. However, the dramatic rise of computer and automatic control subsystems in modern systems has made the operator even more of a monitor, because machines have taken over so much of the routine, manual types of response. Man occasionally responds with a decision or a manual act, but most of the time is spent in monitoring displays for critical signals that command his responses. These interresponse periods require the operator to remain attentive so that he will quickly detect the signals for his response inputs to the system. It is important for engineering psychology to understand attentive behavior and its implications for the effectiveness of man-machine systems. Failure to detect a critical emergency signal can mean disaster. Or, a prolonged delay in sensing the signal to manually fire the retrorockets of a space vehicle for descent to earth, if the system requires a manual response, might land an astronaut in the forbidding wastes of the Sahara Desert instead of the receptive off-shore waters of Florida.

REPRESENTATIVE EMPIRICAL FINDINGS One of the most significant works on vigilance was an impressive monograph by Mackworth (1950), and it did much to stimulate interest in this important topic and to establish basic research methodology. One of his laboratory tasks, which has been widely used, is the Clock Test. The Clock Test has an appearance that is consistent with its name. It is a small circular display with a blank face and a hand that moves regularly at one step per second. On occasion, and randomly, the hand moves two steps. The operator's task is to detect this double jump and report it by pressing a response button. This is a simple task, but it is a good one for studying attention, because it requires the subject to pay steady attention over a long monitoring period. Failure to pay attention causes double jumps to be missed and the performance score to be lowered. Figure 9-12 is an example of Mackworth's findings for the Clock Test. The data, plotted as number of missed signals per half-hour period, are mean values for twenty-five subjects. These data nicely illustrate the vigilance decrement that has intrigued so many investigators. The subjects missed about 26 per cent of the signals at the end of the first hour and, at the end of two hours, 28 per cent of the signals were omitted. Mackworth found similar curves for the detection of transient auditory signals.

Vigilance decrements were not universally found in Mackworth's experiments. Like any other aspect of behavior, vigilance decrement is a function of many variables, and Mackworth found that certain variables, or values of variables, had no vigilance decrement associated with them. Vigilance decrement was eliminated with a rest period of one-half hour, and Adams (1956) has substantiated this finding for visual monitoring by showing that the decrement can be eliminated by a rest period of as little as 10 min. Mackworth found that Benzadrine could eliminate vigilance decrement. Knowledge of results, where the subject was told each time whether or not a signal had been detected, eliminated vigilance decrement. Other investigators have substantiated Mackworth's original finding on knowledge of results (Baker, 1959; Pollack and Knaff, 1958; Weidenfeller, Baker, and Ware, 1962). Another of Mackworth's laboratory tasks simulated surveillance radar, and Mackworth found that brighter visual signals kept monitoring performance at a significantly higher level than dim signals. Adams (1956) subsequently verified this finding.

---

[2] Short-term attention has been rather extensively studied within the framework of temporal and spatial expectancy states that are learned through experience in a vigilance task. See Deese (1955), Baker (1959), Frankmann and Adams (1962), and Boulter and Adams (1963).

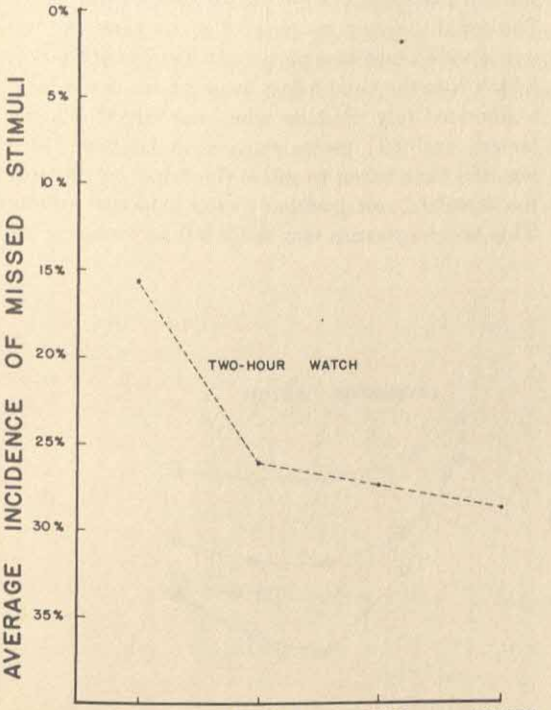
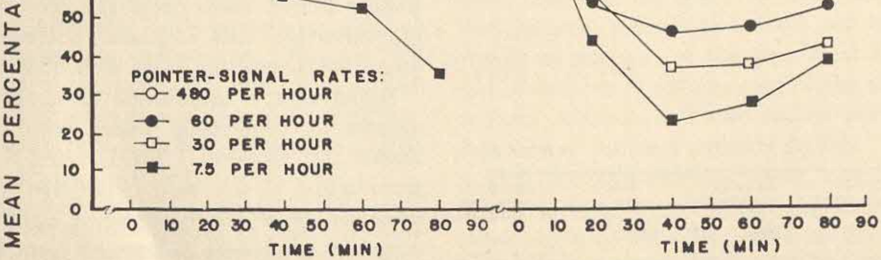


Fig. 9-12. Per cent of critical signals missed during two hours of monitoring the Clock Test (Mackworth, 1950).

Vigilance research has its roots in the military problems of the Second World War where a single stimulus source, such as a surveillance radar scope or a sonar auditory source, was monitored. Monitoring tasks in present-day man-machine systems, however, almost always require attending to multiple visual sources, and the number of experiments on multiple-source monitoring is increasing. A curious fact, and only recently have there been exceptions, is that complex visual tasks with multiple sources have not shown the usual vigilance decrement (Frankmann and Adams, 1962). Broadbent (1950) and Loeb and Jeantheau (1958) used a task with twenty dials and no vigilance decrement was found. Howland (1958) found no decrement when four meters were used as the display. Hoffman and Mead (1943) used a central tracking task with peripheral vigilance tasks and found no decrement over four hours of continuous operation. Using the Mackworth Clock Test, Jerison and Wallis (1957) and Jerison and Wing (1957) found that detection level remained stable when three Clock Tests were the display, in contrast to the well-known decrement for a single Clock Test unit. But more recently, in a series of vigilance experiments at the University of Illinois, Adams and his associates regularly have obtained decrement for visual monitoring tasks with multiple stimulus sources (Adams and Boulter, 1960, 1962; Adams and Humes, 1963; Adams, Stenson, and Humes, 1961; Adams, Humes, and Stenson, 1962). Rather than the brief, transitory signal used in most vigilance experiments, these Illinois studies all used persistent signals lasting up to 20 sec and latency of response as the measure. The traditional per cent detection measure was not discriminating, because persistent signals are almost always detected. It is not clear why the recent Illinois studies obtained vigilance decrement and other studies with complex tasks did not, but perhaps the abandonment of the transitory signal and the per cent detection measure contributed to the discrepancy. In any event, the Illinois studies have upset the previously sound generalization that vigilance decrement is not found for monitoring tasks with multiple sources.

Signal rate is established as a determiner of monitoring proficiency under some conditions. Deese and Ormond (1953), using a simulated radar scope as the task, found that overall performance level increased when signal rate was increased. Figure 9-13 presents data by Jenkins (1958) that illustrate the effect of signal rate on visual monitoring performance. The subject watched a periodic movement of a pointer and his task was to press a switch whenever a needle movement of greater than normal amplitude occurred. A morning and afternoon session of 90 min each were given, and a group had a signal rate of either 7.5, 30, 60, or 480 an hour. Figure 9-13 shows that overall performance level is higher and vigilance decrement is less when signal rate is high. Jenkins' data also show other well-documented vigilance findings—a decline in proficiency as observation time progresses, and a recovery in performance when rest is given. An exception to the positive effect of signal rate on performance is a study by Adams and Boulter (1960) that used a simulated air defense monitoring task. The stimulus sources were symbolic, as might be emitted by a digital computer aiding the data processing, and they were programed on animated strip film to move slowly across the display during a three-hour observation period. The critical signal, which persisted for 20 sec, was the change of a letter G to F at a source. Six sources were on the display at all times. Critical changes in the six sources had rates of either 1 per min or 1 per 5 min. No significant difference between rates was found.

Another important body of vigilance data concerns the effects of repeated sessions on monitoring performance. Most operators who have monitoring tasks must repeat their jobs day after day, and the question is whether boredom, as revealed vigilance decrement, shows a cumulative effect of sessions. Most vigilance research has used only one laboratory session, so there is a limited number of studies on repeated sessions which can be generalized to actual operating conditions. The first study of repeated sessions was by Lindsley et al. (1944). A simulated radar task in the laboratory was used under conditions closely resembling military field operations. Consecutive daily work periods of four hours were administered for three weeks. Loss of efficiency did not become apparent until the third day, indicating that the number of sessions can be a variable in vigilance. Webb and Wherry (1960) reported no consistent effects as a function of five consecutive daily monitoring sessions. Jenkins' data (Fig. 9-13) show that the afternoon performance level tends to be lower than the morning session. Adams, Humes, and Stenson (1962), using the same simulated air defense surveillance task as described above for the study by Adams and Boulter (1960), gave three-hour sessions to subjects for nine consecutive days. A tenth session was administered after a seven-day interval. Figure 9-14 shows the results. Two measures of performance were used. The total latency to press the response button was divided into two parts: (1) detection latency, which was the time taken to move the hand from a standard rest position when the signal was detected, and (2) motor movement latency which was the time taken to move the hand 16 in. from the standard rest position to the response button. This latter measure was recorded as a way of re-



EFFECT OF RATE ON DETECTION

Fig. 9-13. Effect of signal rate on vigilance in a visual monitoring task (Jenkins, 1958).

vealing whether a portion of the overall vigilance decrement could be ascribed to a slowing of motor processes, in addition to changes in visual-perceptual processes. A significant decrement was found for detection latency within each session, but the slight tendency for decrement to increase as a function of number of sessions that is hinted in Fig. 9-14 was not supported statistically. Nor was there a significant change in performance over the longer seven-day rest period. Motor movement latency had both a significant within- and between-sessions effect, but Fig. 9-14 shows that sessions are significant because of a positive learning influence, not a decremental effect. Vigilance decrement, therefore, is revealed as a general slowing down of the subject for both response measures, but it is a within-session, not a between-sessions effect. Notice too that the absolute amount of decrement is small for both measures. These findings by Adams, Humes, and Stenson are cross-validated for visual monitoring by Baker, Sipowicz, and Ware (1961), and for auditory monitoring by Ware, Sipowicz, and Baker (1961). The earlier research suggested a negative influence of number of sessions on monitoring proficiency, but these more recent studies show that monitoring performance can be independent of sessions. The circumstances under which sessions will or will not be a variable have not been delineated, but at least all man-machine systems cannot be expected to show degradation as a function of repeated monitoring sessions.

THEORIES OF VIGILANCE DECREMENT Vigilance decrement and work decrement from prolonged muscular action superficially seem subsumable under a work inhibition construct like I_R (Hull, 1943, 1951, 1952). The nature of the empirical facts for these two areas of research, however, weigh against a common theoretical explanation. The most telling data are those from signal rate, where higher signal rate often results in better monitoring performance. Contrary to a work inhibition view, where more responses per unit of time decrease performance level (e.g., massing of practice), increased signal rate and more responses per unit of time actually increase proficiency. This does not deny the usefulness of constructs to explain fatigue effects from work, but it does suggest the possibility for a two-factor explanation of decrement. Muscle fatigue from prolonged work may be a phenomenon quite different from vigilance decrement.

Mackworth (1950) made the first attempt to present a theory of vigilance, which was based on

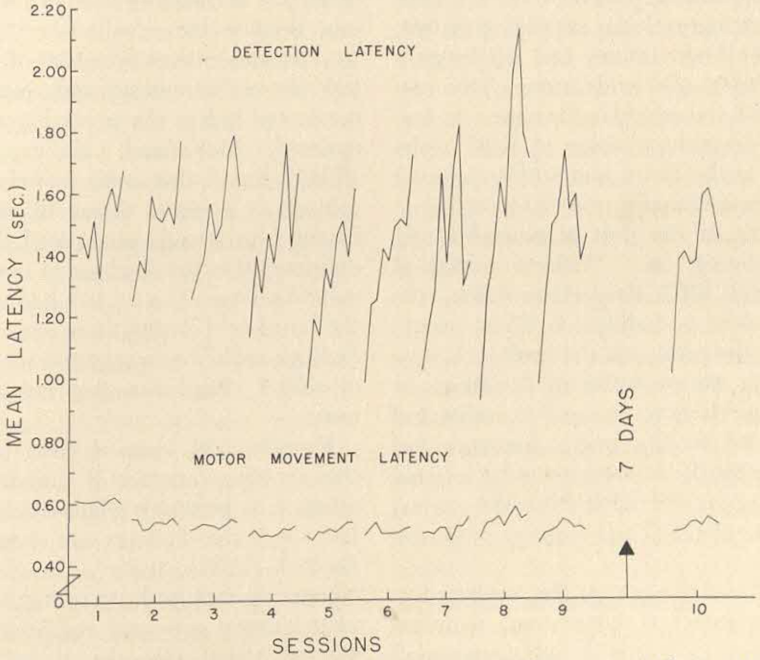


Fig. 9-14. Effect of repeated sessions on visual monitoring performance (Adams, Humes, and Stenson, 1962).

classical conditioning and Pavlov's constructs. The theory was not too successful because there were straightforward deductions from the relationships of classical conditioning that failed to be supported by vigilance data (Frankmann and Adams, 1962). Mackworth's theory never stimulated much empirical work to verify or negate it, as a provocative scientific theory would do, but certainly his empirical studies have been very influential in vigilance research.

The vigilance theory with highest contemporary status is the *arousal hypothesis,* or activationist hypothesis as it is sometimes called (Broadbent, 1958; Frankmann and Adams, 1962). The arousal hypothesis of human attentiveness holds that stimuli impinging on the subject from external sources, or acting within him, determine alertness through the generalized arousal properties of the reticular activating system of the brain (Broadbent, 1958; Fiske, 1961; Frankmann and Adams, 1962; Lindsley, 1957; Malmo, 1959; Samuels, 1959; Rossi and Zanchetti, 1957). Recent physiological research has shown that impulses from a sensory stimulus (environmental, proprioceptive, etc.) reach the cortex by two different routes. Sensory impulses travel the traditional route along the sensory pathway to the corresponding nucleus in the thalamus and terminate in the projection area of the cortex, and they travel a second route through the ascending reticular activating system, which is a lower brain center, and discharge a diffuse bombardment over wide areas of the cortex. The firing of the reticular formation is correlated with a desynchronization of EEG alpha waves recorded at the cortex and with behavioral changes that we customarily associate with general attentiveness on the part of animals (ears pricked up, orienting, etc.). Without arousal of the reticular and EEG desynchronization, the animal is somnolent or indifferent. Thus, arrival of a stimulus at the cortex via the traditional sensory pathway is not a sufficient condition to evoke a response. Moreover, arousal is not limited to stimuli coming to the brain. Research has shown that the reticular formation can be aroused from stimulus inputs fed back from the cortex, so presumably mediational and memory responses can be arousing.

Another important property of the reticular formation is habituation, or adaptation, with repeated applications of a stimulus. Sharpless and Jasper (1956) show that behavioral adaptation in the cat is correlated with habituation of the reticular formation and, after repeated presentations of a stimulus, neither the reticular formation nor overt behavior of the cat can be appreciably aroused by the habituated stimulus. Another aspect of the habituation process was a longer latency of the cat's arousal response as adaptation progressed. Sharpless and Jasper found adaptation to be quite specific to the habituating stimulus. When the stimulus was changed, arousal quickly returned. Scott (1957) has used stimulus habituation as a hypothesis to account for decrement in both vigilance and nonvigilance repetitive tasks.

The arousal hypothesis has considerable merit for explaining vigilance behavior, apart from its physiological significance. In general, it holds that stimulation and attentiveness are related via the reticular formation, and that attentiveness is high when the stimulation level is adequate and low when stimulation is inadequate or when habituation has set in from repeated exposure to stimuli. Vigilance decrement would be explained by habituation from continuous exposure to the same, unchanging stimuli during the long session. Fast signal rates and high stimulus intensities keep the performance level high because they heighten the level of stimulation. Even a rest period, which is customarily thought to allow recovery from fatigue, can be subsumed under the arousal hypothesis, because the usual unstructured rest period is a stimulus change from that of the experimental task. In an interesting and relevant experiment conducted before the arousal hypothesis was formulated, McFarland, Holway, and Hurvich (1942) found that brief periods of talking to a subject in a visual threshold experiment, or of having him stretch, eliminated the decrement in discrimination that had set in over a long experimental session. The subject had to keep watching the visual field during these interruptions, so these findings could be interpreted as being a function of added stimulation that increased responsiveness.

Experimental tests of the arousal hypothesis that specify properties of stimuli beforehand and attempt to predict vigilance behavior have been few, and the findings are mixed. Adams and Boulter (1960), using a simulated air defense monitoring task with six stimulus sources, manipulated signal rate and response complexity. Increased signal rate should be associated both with increased display stimulation and increased response-produced stimulation from more frequent responses. They found that signal rate made no difference, although other research in this area (Deese and Ormond, 1953; Jenkins, 1958) has shown effects in the direction predicted by the arousal hypothesis. However, response complexity, defined as choices for the response, resulted in less decrement than simple, nonchoice responses. Response complexity was hypothesized to be cortical-centered and a way of stimulating the reticular formation by neural feedback. Adams, Stenson, and Humes (1961) used the same task as Adams and Boulter (1960) and found the same general effect for response complexity, although increasing the number of stimulus sources from six to thirty-six, and thus the amount of environmental stimulation, failed to influence decrement over three hours. McGrath (1960), however, found that musical stimulation increased the level of detection for visual stimuli. Similarly, McGrath found that thumbing through photo albums during a session of monitoring for audio signals improved the level of signal detection.

Adams and Boulter (1962) attempted to amplify the earlier successes for response complexity by asking whether other types of response-produced stimulation might be effective as a deterrent for vigilance decrement. Four stimulus sources were used, and the stimulus events to be detected were two-digit numbers on a normally blank display. They studied the amount of proprioceptive stimulation from head and eye movements by manipulating the spatial separation of stimulus sources, and it was hypothesized that greater amounts of head and eye movements would induce higher levels of arousal. Another part of their experiment sought to manipulate variety, as opposed to amount, of proprioceptive stimulation in scanning by the use of cue lights that dictated a required scanning pattern. By changing the pattern midway in the monitoring session they hoped to induce variety in proprioceptive stimulation and thus decrease vigilance decrement. Also, in this same study, they investigated the effects of immediate running memory as a source of internal, cortical-centered stimulation for the reticular formation. The subject was required to keep in mind the last four numbers that had occurred at the four sources to guide his next response. The gist of their results was that no effects on vigilance decrement were found for any of the experimental manipulations.

In summary on the arousal hypothesis, it appears that we have a long way to go before it is useful in engineering psychology for predicting the characteristics of tasks and the conditions of work to keep the monitoring level high. Yet, it is the best hypothesis now available for vigilance. Among the efforts to predict from the arousal hypothesis, only the findings by Adams and Boulter (1960) and Adams, Stenson, and Humes (1961) on response complexity, and McGrath's findings on musical and visual stimulation (1960), have been positive. On the other hand, the failure of stimulus load (Adams, Stenson, and Humes, 1961) and signal rate (Adams and Boulter, 1960) to support the hypothesis have been discouraging. There may be something to the arousal hypothesis, and it may be possible to give it careful predictive status, but there is a long road to travel before it is defined well enough to be scientifically useful. One possibility suggested by the positive side of the research findings is that response-produced stimulation of a meaningful kind may be the most relevant class of stimuli for human arousal. The studies on response complexity are suggestive in this vein, as well as McGrath's work on music, if it is assumed that subjects were humming and singing to themselves. A similar kind of covert responding might have occurred when the subjects thumbed through photo albums. Cautions in this interpretation are urged, however, because the Adams and Boulter study (1962) showed that response-produced stimulation from a running memory task was unrelated to vigilance decrement. Perhaps there was insufficient variety in the immediate memory task and habituation set in, or perhaps memory and response complexity are fundamentally different kinds of internal stimulation.

It has been suggested by Hebb (1955) and Malmo (1959) that the ascending reticular activating system may be the general basis for organismic motivation. This is a widened view for motivational theory, because it gives stimulation a motivational character. If this position, along with the arousal hypothesis for attentive behavior, can be empirically verified, then attention will be related to motivational laws but hardly fully explained by them. Attention is far more complex than high motivation producing high performance.

CONCLUSIONS ON VIGILANCE RESEARCH AND THEORY It is evident that man's role as a monitor will increase as automation increases. More and more of man's time will be spent in watching and listening for stimuli that are signals for his specialized responses, and less and less time will be spent in actually making the responses themselves. If we are to predict the effectiveness of man-machine systems where man has a large monitoring role, it becames important for us to develop a sound understanding of monitoring behavior.

Empirical research on vigilance is accelerating, but the field is underdeveloped theoretically. Nevertheless, the mounting number of studies are making it possible to state useful observations about human vigilance in man-machine systems. The most important of these is that vigilance is probably overrated as a debilitating variable for many present and future man-machine systems. The early research (e.g., Lindsley et al., 1944; Mackworth, 1950) demonstrated decrements that can be damaging for the efficiency of man-machine systems of the Second World War, like radar and sonar, whose critical signals for detection were transitory and near threshold. But in many older systems, and in almost all modern ones, monitoring tasks lack these classes of signals that characterized radar and sonar in the Second World War era. The critical stimuli in modern systems are often superthreshold and persistent. Even surveillance radar today is disappearing in its classical form of low intensity, transitory blips to be detected. Raw radar signals nowadays are often processed through a digital computer and presented to the operator as symbolic, persistent, superthreshold signals, and it is changes in these signals that must be detected. Moreover, the operator usually must watch multiple stimulus sources. Given tasks such as these, research shows that waning vigilance is far less a threat to system performance than heretofore surmised, at least up to three hours of observation. Figure 9-14 is an example of the amount of decrement that is usually found for complex tasks, and some complex tasks have shown no decrement at all. Ordinarily the decrement is less than one second, and it would be a very specialized system in a critical state for a response delay of this minor magnitude to impair attainment of the system's goals. Research on observation periods longer than three hours, or special task circumstances, may reveal decrements that could threaten a modern system's performance, but our present knowledge from research shows that vigilance decrement is trivial for many applications.

SUMMARY

This chapter hardly covers all topics that are of specialized interest to engineering psychology and have been studied under its rubric. Rather, an attempt has been made to show how psychological research is conducted and brought to bear on some applied engineering matters. The emphasis that is given to laboratory experimental work has resulted in an intentional neglect of many important issues and problems that are system-wide in scope, including such topics as the relative capabilities of men and machines, equipment and human reliability, job and task analysis, workspace layout, training and training devices, and system testing from a human factors point of view. These omissions were largely dictated by available space, not a conviction that they are of less significance for the effectiveness of man-machine systems.

REFERENCES

ADAMS, J. A. Vigilance in the detection of low-intensity visual stimuli. *J. exp. Psychol.*, 1956, **52**, 204-208.

———. Human tracking behavior. *Psychol. Bull.*, 1961, **58**, 55-79.

———, AND BOULTER, L. R. Monitoring of complex displays: I. Effects of response complexity and intersignal interval on vigilant behavior when visual load is moderate. *USAF Command and Control Development Division Tech. Note 60-63*. October, 1960.

———, AND ———. An evaluation of the arousal hypothesis of human vigilance. *J. exp. Psychol.*, 1962, **64**, 495-504.

———, AND CREAMER, L. R. Anticipatory timing of continuous and discrete responses. *J. exp. Psychol.*, 1962(*a*), **63**, 84-90.

———, AND ———. Proprioception variables as determiners of anticipatory timing behavior. *Human Factors*, 1962(*b*), **4**, 217-222.

———, AND HUMES, J. M. Monitoring of complex visual displays: IV. Training for vigilance. *Human Factors*, 1963, **5**, 147-153.

———, ———, AND STENSON, H. H. Monitoring of complex visual displays: III. Effects of repeated sessions on human vigilance. *Human Factors*, 1962, **4**, 149-158.

———, STENSON, H. H., AND HUMES, J. M. Monitoring of complex visual displays: II. Effects of visual load and response complexity on human vigilance. *Human Factors,* 1961, **3,** 213-221.

———, AND WEBBER, C. E. The organization of component response error events in two-dimensional visual tracking. *J. exp. Psychol.,* 1961, **61,** 200-212.

———, AND ———. Monte Carlo model of tracking behavior. *Human Factors,* 1963, **5,** 81-102.

———, AND XHIGNESSE, L. V. Some determinants of two-dimensional visual tracking behavior. *J. exp. Psychol.,* 1960, **60,** 391-403.

ANDREAS, B. G., GERALL, A. A., GREEN, R. F., AND MURPHY, D. P. Performance in following tracking as a function of the sensitivity of the airplane-type control stick. *J. Psychol.,* 1957, **43,** 169-179.

BAHRICK, H. P. An analysis of stimulus variables influencing the proprioceptive control of movements. *Psychol. Rev.,* 1957, **64,** 324-328.

———, BENNETT, W. F., AND FITTS, P. M. Accuracy of positioning responses as a function of spring loading in a control. *J. exp. Psychol.,* 1955, **49,** 437-444.

———, FITTS, P. M., AND BRIGGS, G. E. Learning curves—facts or artifacts? *Psychol. Bull.,* 1957, **54,** 256-268.

———, ———, AND SCHNEIDER, R. Reproduction of simple movements as a function of factors influencing proprioceptive feedback. *J. exp. Psychol.,* 1955, **49,** 445-454.

BAILEY, A. W. Simplifying the operator's task as a controller. *Ergonomics,* 1958, **1,** 177-181.

BAKER, C. A., AND GRETHER, W. F. Visual presentation of information. *USAF WADC Tech. Rep.* 54-160, August, 1954.

BAKER, C. H. Towards a theory of vigilance. *Canad. J. Psychol.,* 1959, **13,** 35-42.

BAKER, R. A., SIPOWICZ, R. R., AND WARE, J. R. Effects of practice on visual monitoring. *Percep. mot. Skills,* 1961, **13,** 291-294.

BARLETT, F. C. Anticipation in human performance. In G. Ekman, T. Husén, G. Johansson, and C. I. Sandström (eds.), *Essay in Psychology.* Uppsala: Almquist & Wiksell, 1951, pp. 1-17.

BIRMINGHAM, H. P., KAHN, A., AND TAYLOR, F. V. A demonstration of the effects of quickening in multiple-coordinate control tasks. *U.S. Navy NRL Rep.* 4380. June, 1954.

———, AND TAYLOR, F. V. A design philosophy for man-machine systems. *Proc. IRE,* 1954, **42,** 1748-1758.

BLAIR, W. C., AND PLATH, D. W. Submarine control with the combined instrument panel and a contact analog-roadway display. *J. eng. Psychol.,* 1962, **1,** 68-81.

BOULTER, L. R., AND ADAMS, J. A. Vigilance decrement, the expectancy hypothesis, and intersignal interval. *Canad. J. Psychol.,* 1963, **17,** 201-209.

BOWER, J. L., AND SCHULTHEISS, P. M. *Introduction to the Design of Servomechanisms.* New York: John Wiley & Sons, Inc., 1958.

BRIGGS, G. E., FITTS, P. M., AND BAHRICK, H. P. Effects of force and amplitude cues on learning and performance in a complex tracking task. *J. exp. Psychol.,* 1957, **54,** 262-268.

———, AND HOWELL, W. C. On the relative importance of time sharing at central and peripheral levels. *U.S. Nav. Trng. Dev. Cen. Tech. Rep.* NAVTRADEVCEN 508-2, October, 1959.

BROADBENT, D. E. The Twenty Dials Test under quiet conditions. *Med. Res. Council, Appl. Psychol. Res. Unit Rep.* APU 130/50. 1950.

———. *Perception and Communication.* New York: Pergamon Press, 1958.

BROWN, G. S., AND CAMPBELL, D. P. *Principles of Servomechanism.* New York: John Wiley & Sons, Inc., 1948.

CHAPANIS, A., GARNER, W. R., AND MORGAN, C. T. *Applied Experimental Psychology.* New York: John Wiley & Sons, Inc., 1949.

CHARIPPER, B. A. Ship Control VIII. Single-element vs. two-element display in two-dimensional tracking. *General Dynamics Corp., Elec. Boat Div. Tech. Rep.* SPD 59-003. February, 1959.

CHERNIKOFF, R., BIRMINGHAM, H. P., AND TAYLOR, F. V. A comparison of pursuit and compensatory tracking in a simulated aircraft control loop. *J. appl. Psychol.,* 1956, **40,** 47-52.

———, AND TAYLOR, F. V. Effects of course frequency and aided time constant on pursuit and compensatory tracking. *J. exp. Psychol.,* 1957, **53,** 285-292.

CHRISTENSEN, J. M. Quantitative instrument reading as a function of dial design, exposure time, preparatory fix, and practice. *USAF WADC Tech. Rep.* 52-116, September, 1952.

CONKLIN, J. E. Effect of control lag on performance in a tracking task. *J. exp. Psychol.,* 1957, **53,** 261-268.

CRAIG, D. R. Effect of amplitude range on duration of responses to step function displacements. *USAF Air Materiel Command Tech. Rep., 5913.* September, 1949.

DEESE, J. Some problems in the theory of vigilance. *Psychol. Rev.,* 1955, **62,** 359-368.

———, AND ORMOND, E. Studies of detectability during continuous visual search. *USAF WADC Tech. Rep.* No. 53-8. September, 1953.

ECKMAN, D. P. (ed.) *Systems: Research and Design.* New York: John Wiley & Sons, Inc., 1961.

ELAM, C. B., EMERY, J., AND MATHENY, W. G. Tracking performance as affected by the position of the attitude display. *Bell Helicopter Corp. Rep.* No. D228-421-010. March, 1962.

ELKIN, E. H. Effects of scale shape, exposure time, and display-response complexity on scale reading efficiency. In P. M. Fitts (ed.), *Human Engineering Concepts and Theory.* Ann Arbor: University of Michigan Press, 1959, pp. 13.5.1-13.5.12.

ELLSON, D. G. The application of operational analysis to human behavior. *Psychol. Rev.,* 1949, **56**, 9-17.

———. Linear frequency theory as behavior theory. In S. Koch (ed.) *Psychology: A Study of a Science.* Vol. 2. New York: McGraw-Hill Book Company, Inc., 1959, pp. 637-662.

———, HILL, H., AND CRAIG, D. R. The interaction of responses to step function stimuli: II. Equal opposed steps of varying amplitude. *USAF Air Materiel Command Tech. Rep. 5911.* August, 1949.

ELY, J. H., BOWEN, H. M., AND ORLANSKY, J. Man-machine dynamics. *USAF WADC Tech. Rep.* 57-582. November, 1957.

FISKE, W. Effects of monotonous and restricted stimulation. In D. W. Fiske and S. R. Maddi (eds.) *Functions of Varied Experience.* Homewood: Dorsey, 1961, pp. 106-144.

FITTS, P. M. Engineering psychology and equipment design. In S. S. Stevens (ed.), *Handbook of Experimental Psychology.* New York: John Wiley & Sons, Inc., 1951, pp. 1287-1340.

———, BENNETT, W. F., AND BAHRICK, H. P. Application of autocorrelation and crosscorrelation analysis to the study of tracking behavior. In G. Finch and F. Cameron (eds.), *Symposium on Air Force Human Engineering, Personnel, and Training Research.* Baltimore: USAF Air Research and Development Command, 1956. (Tech. Rep. 56-8.)

———, AND JONES, R. E. Psychological aspects of instrument display. I. Analysis of 270 "pilot-error" experiences in reading and interpreting aircraft instruments. In H. W. Sinaiko (ed.), *Selected Papers on Human Factors in the Design and Use of Control Systems.* New York: Dover, 1961, pp. 359-396.

———, AND SIMON, C. W. Some relations between stimulus patterns and performance in a continuous dual-pursuit task. *J. exp. Psychol.,* 1952, **43**, 428-436.

FLAGLE, C. D., HUGGINS, W. H., AND ROY, R. H. *Operations Research and Systems Engineering.* Baltimore: The Johns Hopkins Press, 1960.

FOX, G. A. Detection of angular change in a grid line display. *Bell Helicopter Corp. Rep.* No. D228-420-001. No date.

———, HARDT, H. D., AND MATHENY, W. G. Detection of small changes in the size of the squares in a grid line display. *Bell Helicopter Corp. Rep.* No. D228-420-002. February, 1959.

FRANKMANN, JUDITH P., AND ADAMS, J. A. Theories of vigilance. *Psychol. Bull.,* 1962, **59**, 257-272.

GAGNE, R. M. (ed.) *Psychological Principles in System Development.* New York: Holt, Rinehart, and Winston, Inc., 1962.

GIBBS, C. B. The continuous regulation of skilled response by kinaesthetic feedback. *Brit. J. Psychol.,* 1954, **45**, 24-39.

GOODE, H. H., AND MACHOL, R. E. *System Engineering.* New York: McGraw-Hill Book Company, Inc., 1957.

GRAHAM, NORAH E. The speed and accuracy of reading horizontal, vertical, and circular scales. *J. appl. Psychol.,* 1956, **40**, 228-232.

GRETHER, W. F. Discussion of pictorial versus symbolic aircraft instrument displays. *USAF Air Materiel Command Tech. Rep.* TSEAA-694-8B, August, 1947.

———. Instrument reading. I: The design of long-scale indicators for speed and accuracy of quantitative reading. *J. appl. Psychol.,* 1949, **33**, 363-372.

GUETZKOW, H. *Simulation in Social Science: Readings.* Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1962.

HARTMAN, B. O. The effect of the extent of movement (control sensitivity) on pursuit tracking performance. *U.S. Army Med. Res. Lab. Rep.* No. 308. December, 1957.

———, AND FITTS, P. M. Relation of stimulus and response amplitude to tracking performance. *J. exp. Psychol.,* 1955, **49**, 82-92.

HEBB, D. O. Drives and the C.N.S. (conceptual nervous system). *Psychol. Rev.,* 1955, **62**, 243-254.

HELSON, H. Design of equipment and optimal human operation. *Amer. J. Psychol.,* 1949, **62**, 473-497.

HENNEMAN, R. H., AND LONG, E. R. A comparison of the visual and auditory senses as channels for data presentation. *USAF WADC Tech. Rep.* 54-363. August, 1954.

HOFFMAN, A. C., AND MEAD, L. C. The performance of trained subjects on a complex task of four hours duration. *OSRD Rep.* No. 1701. 1943.

HOLLAND, J. G., AND HENSEN, JEAN B. Transfer of training between quickened and unquickened training systems. *J. appl. Psychol.*, 1956, **40**, 362-366.

HOWLAND, D. An investigation of the performance of the human monitor. *USAF WADC Tech. Note* 57-431. July, 1958.

———, AND NOBLE, M. E. The effect of physical constants of a control on tracking performance. *J. exp. Psychol.*, 1953, **46**, 353-360.

HULL, C. L. *Principles of Behavior.* New York: D. Appleton-Century Company, Inc., 1943.

———. *Essentials of Behavior.* New Haven: Yale University Press, 1951.

———. *A Behavior System.* New Haven: Yale University Press, 1952.

JACKSON, K. F. Behaviour in controlling a combination of systems. *Ergonomics,* 1958, **2,** 52-62.

JENKINS, H. M. The effect of signal-rate on performance in visual monitoring. *Amer. J. Psychol.*, 1958, **71**, 647-661.

JERISON, H. J., AND WALLIS, R. A. Experiments on vigilance: one-clock and three-clock monitoring. *USAF WADC Tech. Rep.* 57-206. April, 1957.

———, AND WING, S. Differential effects of noise and fatigue on a complex vigilance task. *USAF WADC Tech. Rep.* 57-14. January, 1957.

KAPPAUF, W. E. Design of instrument dials for maximum legibility: I. Development of methodology and some preliminary results. *USAF Air Materiel Command Rep.* No. TSEAA-8/PMF/jc. October, 1947.

KING, W. J. Coordination in a complex tracking task as a joint function of spatial separation and predictability of stimuli. Unpublished M. A. thesis, University of Illinois, 1961.

LEVINE, M. Tracking performance as a function of exponential delay between control and display. *USAF WADC Tech. Rep.* 53-236. October, 1953(*a*).

———. Transfer of tracking performance as a function of a delay between the control and the display. *USAF WADC Tech. Rep.* 53-237. November, 1953(*b*).

LICKLIDER, J. C. R. Quasi-linear operator models in the study of manual tracking. In R. D. Luce (ed.), *Developments in Mathematical Psychology.* Glencoe, Ill.: Free Press, 1960, pp. 166-279.

LINCOLN, R. S., AND SMITH, K. U. Systematic analysis of factors determining accuracy in visual tracking. *Science,* 1952, **116,** 183-187.

LINDSLEY, D. B. Psychophysiology and motivation. In M. R. Jones (ed.), *Nebraska Symposium on Motivation: 1957.* Lincoln: University of Nebraska Press, 1957, pp. 44-105.

———, et al. Radar operator "fatigue." The effects of length and repetition of operating periods on efficiency of performance. *OSRD Rep.* 3334. January, 1944.

LOEB, M., AND JEANTHEAU, G. The influence of noxious environmental stimuli on vigilance. *J. appl. Psychol.*, 1958, **42,** 47-49.

LOUCKS, R. B. An experimental evaluation of the interpretability of various types of aircraft attitude indicators. In P. M. Fitts (ed.), *Psychological Research on Equipment Design.* AAF Aviat. Psychol. Prog. Res. Rep. No. 19, 1947, pp. 111-135.

LUTZ, R. E., AND PERKINS, C. C., JR. A time variable in the acquisition of observing responses. *J. comp. physiol. Psychol.*, 1960, **53,** 180-182.

MCCORMICK, E. J. *Human Engineering.* New York: McGraw-Hill Book Conmpany, Inc., 1957.

MCFARLAND, R. A., HOLWAY, A. N., AND HURVICH, L. M. *Studies of Visual Fatigue.* Cambridge: Harvard Graduate School of Business Administration. 1942.

MCGRATH, J. J. The effect of irrelevant environmental stimulation on vigilance performance. Project on Human Factors Problems in Anti-Submarine Warfare: Technical Report No. 6. Psychological Sciences Division, *Office of Naval Research.* November, 1960.

MACKWORTH, N. H. Researches on the measurement of human performance. *Med. Res. Council Spec. Rep. Ser.*, 1950, No. 268.

MALMO, R. B. Activation: a neuropsychological dimension. *Psychol. Rev.*, 1959, **66,** 367-386.

MATHENY, W. G. Human operator performance under non-normal environmental operating conditions. In S. B. Sells and C. A. Berry (eds.), *Human Factors in Jet and Space Travel.* New York: The Ronald Press Company, 1961.

———, AND HARDT, H. D. Further study in the display of spatial orientation information. *Bell Helicopter Corp. Rep.* No. D228-421-002. August, 1959(*a*).

———, AND ———. The display of spatial orientation information. *Bell Helicopter Corp. Rep.* No. D228-421-001. August, 1959(*b*).

MELTON, A. W. (ed.) *Apparatus Tests.* Washington: United States Government Printing Office, 1947. (*AAF Aviat. Psychol. Prog. Res. Rep. No. 4.*)

MENGELKOCH, R. F. Pilot simulator performance with standard and vertical reading primary flight instruments. *The Martin Co. Eng. Rep.* No. 10,846. August, 1959.

———, AND HOUSTON, R. C. Investigations of vertical displays of altitude information. I: Comparison of a moving-tape and standard altimeter on a simulated flight task. *USAF WADC Tech. Rep.* 57-384, March, 1958(*a*).

———, and ———. Investigations of vertical displays of altitude information. II. The effect of practice on performance of a simulated flight task using a moving-tape altimeter. *USAF WADC Tech. Rep.* 57-385, March, 1958(*b*).

———, and ———. Investigations of vertical displays of altitude information. III. The effect of an expanded scale on performance of a simulated flight task using a moving-tape altimeter. *USAF WADC Tech. Rep.* 57-549, March, 1958(*c*).

Mowbray, G. H., and Gebhard, J. W. Man's senses as informational channels. In H. W. Sinaiko (ed.), *Selected Papers on Human Factors in the Design and Use of Control Systems.* New York: Dover Publications, Inc., 1961, pp. 115-149.

Muckler, F. A., and Matheny, W. G. Transfer of training in tracking as a function of control friction. *J. appl. Psychol.,* 1954, **38**, 364-367.

Narva, M. A. Evaluation of decision making performance on three pictorial navigation displays. *USAF WADC Tech. Rep.* 58-49. November, 1958.

Obermayer, R. W., Swartz, W. F., and Muckler, F. A. The interaction of information displays with control system dynamics in continuous tracking. *J. appl. Psychol.,* 1961, **45**, 369-375.

Payne, T. A. A study of the moving figure and orientation of symbols on pictorial aircraft in instrument displays for navigation. *U.S. Navy Spec. Dev. Cen. Tech. Rep.* SDC 71-16-6. July, 1950.

———. A study of the moving part, heading presentation, and map detail on pictorial air navigation displays. *U.S. Navy Spec. Dev. Cen. Tech. Rep.* SDC 71-16-10. November, 1952.

Pollack, I., and Knaff, P. R. Maintenance of alertness by a loud auditory signal. *J. Acoust, Soc. Amer.,* 1958, **30**, 1013-1016.

Poulton, E. C. The basis of perceptual anticipation in tracking. *Brit. J. Psychol.,* 1952(*a*), **43**, 295-302.

———. Perceptual anticipation in tracking with two-pointer and one-pointer displays. *Brit. J. Psychol.,* 1952(*b*), **43**, 222-229.

———. Learning the statistical properties of the input in pursuit tracking. *J. exp. Psychol.,* 1957(*a*), **54**, 28-32.

———. On prediction in skilled movements. *Psychol. Bull.,* 1957(*b*), **54**, 467-478.

———. On the stimulus and response in pursuit tracking. *J. exp. Psychol.,* 1957(*c*), **53**, 189-194.

Prokasy, W. F., Jr. The acquisition of observing response in the absence of differential external reinforcement. *J. comp. physiol. Psychol.,* 1956, **49**, 131-134.

Ritchie, M. L., and Baker, C. A. Psychological aspects of cockpit design—a symposium report. *USAF WADC Tech. Rep.* 57-117. April, 1957.

Rockway, M. R. The effect in variations in control-display ratio and exponential time delay on tracking performance. *USAF WADC Tech. Rep.* 54-618. December, 1954.

———. Effects of variations in control deadspace and gain on tracking performance. *USAF WADC Tech. Rep.* 57-326. September, 1957.

———, Eckstrand, G. A., and Morgan, R. L. The effect of variations in control-display ratio during training on transfer to a low ratio. *USAF WADC Tech. Rep.* 56-10. October, 1956.

———, and Franks, P. E. Effects of variations in control backlash and gain on tracking performance. *USAF WADC Tech. Rep.* 58-553. January, 1959.

———, Morgan, R. L., and Eckstrand, G. A. Effects of variations in control-display ratio and amount of original practice on transfer of tracking skill. In G. Finch and F. Cameron (eds.), *Symposium on Air Force Human Engineering, Personnel, and Training Research.* Publication 516, National Academy of Sciences —National Research Council. Washington, D.C., 1958, pp. 108-113.

Roscoe, S. N., Smith, J. F., Johnson, B. E., Dittman, P. E., and Williams, A. C., Jr. Comparative evaluation of pictorial and symbolic VOR navigation displays in the 1-CA-1 Link Trainer. *CAA Div. of Res. Rep.* No. 92. October, 1950.

Rossi, G. F., and Zanchetti, F. The brain stem reticular formation. *Archives Italiennes de Biologié,* 1957, **95**, 199-435.

Samuels, Ina. Reticular mechanisms and behavior. *Psychol. Bull.,* 1959, **56**, 1-25.

Scott, T. H. Literature review of the intellectual effects of perceptual isolation. Report No. HR66, Defence Research Board, Department of National Defence, Canada. July, 1957.

Searle, L. V., and Taylor, F. V. Studies of tracking behavior: I. Rate and time characteristics of simple corrective movements. *J. exp. Psychol.,* 1948, **38**, 615-631.

Senders, J. W., and Cruzen, M. Tracking performance on combined compensatory and pursuit tasks. *USAF WADC Tech. Rep.* 52-39, 1952.

SENDERS, VIRGINIA L. The effect of number of dials on qualitative reading of a multiple dial panel. *USAF WADC Tech. Rep.* 52-182. November, 1952.

SHARPLESS, S., AND JASPER, H. Habituation of the arousal reaction. *Brain,* 1956, **79**, 655-680.

SIDORSKY, R. C. Absolute judgments of static perspective transformations. *J. exp. Psychol.,* 1958, **56**, 380-384.

———. Ship Control XIV. Advanced FBM submarine ship control console. *General Dynamics Corporation, Elec. Boat Div. Tech. Rep.* U411-61-102. August, 1961.

———, AND NEWTON, J. M. Ship Control V. The effects of motion and number of surfaces on depth control with a contact analog display. *General Dynamics Corporation, Elec. Boat Div. Tech. Rep.* SPD-59-010. February, 1959.

SIEGEL, A. I., AND WOLF, J. J. A technique for evaluating man-machine system designs. *Human Factors,* 1961, **3**, 18-28.

SLEIGHT, R. B. The effect of instrument dial shape on legibility. *J. appl. Psychol.,* 1948, **32**, 170-188.

SVIMINOFF, C. The Air Force integrated flight instrument panel. *USAF WADC Tech. Rep.* 58-431. October, 1958.

SWEENEY, J. S., BAILEY, A. W., AND DOWD, J. F. Comparative evaluation of three approaches to helicopter instrumentation for hovering flight. *U.S. Navy NRL Rep.* 4954. June, 1957.

WARE, J. R., SIPOWICZ, R. R., AND BAKER, R. A. Auditory vigilance in repeated sessions. *Percep. mot. Skills,* 1961, **13**, 127-129.

WARRICK, M. J. Effect of transmission-type control lags on tracking accuracy. *USAF Air Materiel Command Tech. Rep.* 5916. September, 1949.

WEBB, W. B., AND WHERRY, R. J., JR. Vigilance in prolonged and repeated sessions. *Percep. mot. Skills,* 1960, **10**, 111-114.

WEBBER, C. E., AND ADAMS, J. A. Issues in the use of an analog-digital data system for the measurement of tracking behavior. *USAF Office of Scientific Research Tech. Note* 59-528. April, 1960.

WEIDENFELLER, E. W., BAKER, R. A., AND WARE, J. R. Effects of knowledge of results (true and false) on vigilance performance. *Percep. mot. Skills,* 1962, **14**, 211-215.

WILKERSON, L. E., FOX, G. A., AND MATHENY, W. G. The effect of scale form on the speed and accuracy of scale reading. *Bell Helicopter Corp. Rep.* No. D228-420-003, October, 1959.

WILLIAMS, A. C., JR., AND ROSCOE, S. N. Evaluation of aircraft instrument displays for use with the omni-directional radio range. *CAA Div. of Res. Rep.* No. 84, March, 1949.

WYCKOFF, L. B. The role of observing responses in discrimination learning. Part I. *Psychol. Rev.,* 1952, **59**, 431-442.

CHAPTER 10

Behavior in Unusual Environments

WILLIAM BEVAN
The Johns Hopkins University

INTRODUCTION

Within the family of the behavioral sciences, the psychologist is, perhaps, best qualified to address himself to the question of behavior in unusual environments, for the major portion of his formal data has come from the laboratory and the clinic, both atypical settings in the behavioral ecology of his own species and of the warm-blooded animals that interest him most. Sensitive to the constraints of the scientific method, he has preferred laboratory control to field observation, and his controls have been marked primarily by two characteristics: relative simplicity and behavioral ahistoricity. The nonsense syllable and the albino rat are materials designed to eliminate from observational data the contaminations of nonlaboratory life and experience. They have been used either with little concern for the broad applicability of results or with the confident assumption that, allowing for obvious differences in complexity of setting, broad applicability is implicit. Little critical concern has been evidenced, except by Brunswik (1956) and those influenced by his thinking, for either the theoretical or the methodological significance of the problem of representativeness.

Meanwhile, the current heightened interest in behavior in unusual environments has been prompted largely by the practical needs of the military and space agencies. This is not to ignore the literature generated by Hebb's theoretical formulations (1949) concerning the influence of early experience upon behavioral organization. But a larger number of empirical studies have been atheoretical in nature and associated with such questions as: How does one ensure efficient monitoring performance by radar watchkeepers? What are the effects of physical isolation upon the behavior and mental processes of prisoners of war? How will an astronaut be able to carry out his assigned duties during periods of acceleration and weightlessness? And what are the problems of visual orientation in an extraterrestrial environment?

The aim of this chapter is twofold: first, to examine the concept of environment as it has been understood by psychologists during the past half century, and second, to provide a substantive review of particular segments of the literature that have captured the attention of psychologists during the last decade.

THE PSYCHOLOGIST'S TREATMENT OF THE ENVIRONMENT

American psychology—and since the Second World War most psychology—has been dominantly stimulus-response in orientation and deeply ingrained by empiricism, but the concept of environment has received little deliberate systematic attention (Chein, 1954). Rather like the father of the bride at the wedding, it has been an indispensable ingredient in behavior theories but scarcely noticed. It would appear that everyone has assumed that everyone knows what environment is. A cursory examination of five introductory texts published in 1961 and 1962 (Geldard, Hilgard, Morgan, Munn, and Sells) reveals references to environment to occur, for the most part, in discussions of intelligence and to be centered about the complementarity of heredity and environment as determinants of this property of organisms. One of the five (Morgan) does not include the term in its index and only one (Munn) gives it definition. However, this definition is so broad as to be of limited systematic value: "Everything which surrounds the units of inheritance." (Munn, 1962, p. 503). This is representative of the literature. Murphy (1947), in his excellent and comprehensive study of personality, defines environment as a "group of factors potentially capable of influencing an organism" (Murphy, 1947, p. 985). A check through the *Psychological Abstracts* and the *Annual Review of Psychology* indicates little mention of environment except in connection with intelligence and temperament. In the past few years, index listings for unusual environments have been increased somewhat by the empirical reports on isolation, brainwashing, acceleration, weightlessness, vigilance, etc.

Preconceptions of the Environment. If one reviews the literature of the major theoretical movements in psychology during the last fifty years, one gains the impression that, without exception, environment finds its place as an a priori concept. While most certainly each theorist must have been brought to his view of environment as much by his own transactions as a human being as by logical exercise, he begins his theory with a notion of what environment is. Certainly he does not evolve it from empirical data specifically generated by his theory.

Environment is an ambiguous concept and these preconceptions may be characterized by pairs of contrasting adjectives: physical versus phenomenological, objective versus subjective, outer-directed versus inner-directed, observer-oriented versus perceiver-oriented, peripheral versus central, substantive versus functional, concrete versus abstract. These dichotomies are not completely independent of each other. Theorists who have interpreted environment in physical terms have most frequently asserted its objectivity and have been observer-oriented and outer-directed. That is, they have viewed the properties of the environment, in a basic sense, to be independent of the characteristics of the organism immersed in it. They have been preoccupied with the observation of the activity of other organisms in the environment in contrast to their own. And they have viewed behavior as instigated by the action of the environment. In contrast, the phenomenologists have been preoccupied with the world as each observer is himself aware of it, and they have thought of the effective environment as constructed in the consciousness of the behaving organism and as acted upon by it.

Conceptualizations have, of course, changed with time. The physical environment was first of all an array of solid palpable objects that impinged upon the sensorium and gave rise to perceptions through the processes of association. Later, it was conceived of as a composite of external physical energies that instigated neurophysiological events within the organism. These, in turn, resulted in an ultimate neuromuscular event, the response. For a period, objectivity was coextensive with the world of Newtonian physics. More recently, under the influence of the positivistic movement in philosophy and science—which itself has mellowed over the years—objectivity has come to be identified with certain scientific operations, until, as Adams says (1954, p. 66), "A sentiment or a psychical system or a superego is just as physical a notion as a cell assembly, an engram, or even an atom." And Carnap (1956, pp. 70-71) comments: "Although many of the alleged results of introspection were indeed questionable, a person's awareness of his own state of imagining, feeling, etc., must be recognized as a kind of observation in principle not differing from external observation and therefore as a legitimate source of knowledge, though limited by its subjective character."

Over the years psychological thinking also has shifted its focus from the substantive environment to the environment as an agent in the etiology of specific behaviors and, as psychologists have become philosophically and methodologically more knowledgeable, notions of the environment have become more deliberately abstract.

Behavior and the Physical Nature of the Environment. Behavior is always, in an essential sense, a physical phenomenon, for it is always instigated, immediately or otherwise, by a physical energy change impinging upon the organism. As George Herbert Mead (1939, p. 245) put it, "The only environment to which the organism can react is the one that its sensitivity reveals."

Physicalism in psychology has been identified with behaviorism. For Watson (1919, 1925) it was a reaction against what he regarded to be the sterility and static nature of introspectionism. This he associated with the formalism of introspectionism and its preoccupation with mental content. But, as Bergmann (1962) has pointed out, his extreme environmentalism comes not so much from his physicalism as from his Platonism. His often cited assertion (Watson, 1925, p. 82) that he could take a dozen healthy, well-formed infants and, with his own specified world to bring them up in, could guarantee to make any of them, selected at random, the sort of person he chose is an easily recognized instance of the ideal of scientist as ruler. While Watson's environmentalism was not a logical requirement of his behavioristic position but a reaction against uncritical traditionalism, it became an uncritically accepted tradition itself, and it reached its extreme in the early 1930s in the position of E. B. Holt (1931). To Holt the organism was an automaton capable of two basic types of reaction, adient or attractive responses and abient or withdrawal responses. Behavior was viewed as a concatenation of reflexes in a physical-physiological environment. Environment was assumed to direct activity and, if it was stable, led to the fixation of responses and the eventual establishment of a nonrandom behavioral repertoire. Thus there could be no mysterious ghostly inner "will."

To the Watsonians the major principle governing the evolution of behavior was that of contiguity: responses came to be reliably evoked by particular stimuli by virtue of their temporal proximity. But two other concepts of importance gained attention and, with their ascendance, conceptualizations of environment became explicitly more functional. These are the notions of homeostasis and of reinforcement. Homeostasis is identified with Cannon (1932), who conceived of metabolic systems as dynamic systems, responding to extrasystematic change in such a manner as to maintain a condition of balance. The homeostatic model was extended by Freeman (1948) to describe the total organism and its relation to its physical environment. The principle of reinforcement gained prominence through Thorndike's connectionism, and it was brought to theoretical maturity within Hull's system of behaviorism (1943). According to this principle, response strength is altered by virtue of the stimulation which follows contingently upon or as a consequence of (this varies with the theoretical version of the concept employed) the occurrence of the response. Thus, behavior is shaped not only by the action of the environment upon the organism, but also by the organism's reaction to the environment.

Physical interpretations of environment have been most prominently identified with psychological theories explaining learning and behavioral development. Over the years, perceptual theories have most often been phenomenological in orientation. But physical interpretations of the environment also have had a place in perceptual theory. A notable recent example is Gibson's psychophysics of surface and edge (1959). For Gibson the visual environment is an array of physical surfaces or interfaces between matter in different states. For terrestrial man, it is a gaseous medium (the air) and a solid substratum (the ground) with other solid surfaces in various spatial relations to each other and for the most part in contact with the substratum (objects). The geometrical arrangement of these components gives rise to a patterned impingement of energy upon the retina, which in turn is the site of neurophysiological events characterized in terms of spatial and temporal gradients. These are the correlates of the three-dimensional world of visual perception.

The Phenomenological Approach to Environment. If physicalistic conceptions of environment have been identified with learning theories, phenomenological conceptions are found most often within perceptually oriented interpretations of behavior. While these latter may differ in the importance they assign to the environment as a determinant of behavior, they have in common the fact that they begin with the direct experience of the perceiving organism, and behavior is understood as it relates to this. Experience, as Köhler pointed out in his *Gestalt Psychology* (1929, p. 323), has a bipolar structure: it consists of the Self and the Environment and behavior is directly regulated by these two components. This approach does not deny the physical environment in the way that Watson, in his early radical rejection of introspectionism, denied the world of consciousness, nor does it dispute the relevance for psychology of the physical environment. Rather it simply assumes a different starting place and the physical environment receives theoretical consideration as it is mediated through the world of experience. Koffka's frequently cited story of the horseman of Lake Constance (1935, p. 27) makes clear the relation of behavior to the world of experience. This apocryphal incident centers about a horseman who gallops across a broad plain only to be informed by an innkeeper on the far side that he has just crossed the snow-covered Lake Constance, whereupon he promptly drops dead. The horseman's physical action was riding over a lake, his behavior was riding over a plain, and the nature of the physical environment, when this was translated into behavioral terms, was such as to produce a traumatic effect. According to Koffka, behavior always occurs in a behavioral environment, one's own or that of another person. One's own behavior in one's own behavioral environment he calls "phenomenal" behavior. The actions of another person as they project into one's own behavioral environment are termed "apparent" behavior. The complement holds for events in the behavioral environment of another perceiver.

A first reading of Lewin (e.g., 1936) yields the impression that his theorizing is phenomenological: Behavior is a function of the Person and the Psychological Environment. But closer examination makes it clear that both his Person and his Environment, clothed in a terminology borrowed from topology and vector analysis, are extremely abstract.

Roger Barker, a serious student of Lewin for many years, has pointed out that a major difficulty with Lewin's theoretical orientation, a difficulty which Lewin himself recognized, was that it was an encapsulated system of purely psychological constants that was incapable of accommodating the influence of the nonpsychological ecological environment—i.e., the physical setting in which behaving organisms found themselves. Barker himself (1964) has proposed as an approach to this dilemma the study of behavior episodes, the natural (or natural history) units of activity that relate persons to other persons or objects in a behavioral setting. By behavioral setting he means a concrete situation, objective in the sense that it is independent of any previous perception of it, with a particular time-space locus, made up of events and entities in a nonrandom array, and all with a clearly discernible boundary. By meticulously categorizing the structure of episodes and settings and by repeatedly examining relationships between psychological and ecological variables like setting-size and response, he believes that the psychologist may discover general principles of eco-behavioral organization and thus predict behavioral events without a preoccupation with the specific substantive content of the phenomena involved.

The great resistance of American psychologists over the years to the phenomenological approach has been prompted chiefly by the conviction that the world of direct experience lies beyond the realm of scientific objectification. But the past decade or two has brought an increasing recognition that the basic data of science, regardless of the medium within which the scientist works or the metaphysical bias of his concepts, are statements about observations rather than observations themselves. As a consequence, we have become to an increasing degree metaphysically neutral, and the acceptability of the approach is a matter of the efficiency of operational manipulation.

The Functional Definition of Environment. With the passage of time, conceptions of the environment, both *S-R* and cognitive, have become more functional in emphasis. Among the *S-R* theories, we have identified this trend with the ascendance of the principles of homeostasis and reinforcement. This is to oversimplify and to ignore the influence of persons like Dewey, William Stern, Woodworth, and Tolman. Dewey (Roback, 1952, p. 101) had early identified the complementarity of the concepts of organism and environment and pointed out their necessary reciprocity. Stern (1938) had recognized that the individual organism, to a degree at least, selects the environmental milieu in which it carries on its affairs. Tolman, whose theoretical orientation was behavioristic, but whose theoretical style was cognitive, coined along with Brunswik (Tolman and Brunswik, 1935), the succinct functional definition of environment by reference to its causal texture. Woodworth over the years, and most recently in his 1958 introductory text, emphasized that behavior involves an active give and take between the organism and the environment. Indeed, for him the pervasive property of behavior is one of keeping the organism in touch with the environment. This involves the organism's first actively exploring its surround, secondly, the organism's getting stimulus cues which, through the processes of perception, are translated into information both about the physical properties of the surround and its significance for the organism, thirdly, the organism's employing this information to determine its course of action, and finally, the organism's using its muscle system to adapt to the stimulus character of the surround.

Gestalt theory, with its assumption that the structure of perception depends upon the operation of autochthonous brain factors, so thoroughly dominated thinking about perception during the major portion of the past half century that perceptually-based theories of behavior gave scant attention to the place of environment as an active behavioral determinant. Three foci of interest during the late 1940s and early 1950s have had a compensatory influence: Hebb's concern for the role of early experience in behavioral development (1949); the impact upon psychologists of Dewey and Bentley's transactionism (1949); and the studies of the interrelationship of perception and motivation (Bruner and Krech, 1950).

According to Hebb, a mature response depends upon the activation of a complexly organized brain system that has been elaborated gradually and over a long period of time through the interaction of stimulus impingement and effector excitation. Thus the structure of the visual world depends as much upon eye movements as upon patterns of retinal stimulation. The thoroughgoingness of Hebb's functionalism is seen in his treatment of motivation and his rejection of both explicit and implicit drive theory (1955*b*).

Hebb's is an *S-R* approach to behavior that has many of the earmarks of cognitive theory. Others have written in a similar vein. Where Hebb has referred to cell assemblies and phase sequences, Krech (1950) has discussed Dynamic Systems. More recently, Miller, Galanter, and Pribram (1960) have written of Plans. The effect that an event has upon behavior depends upon the representation of the event in the organism's image of itself. This representation is a system of concepts and relations, and the crucial question is how perception is translated into action. The answer is, of course, through the evolution of a Plan.

Transactionism in psychology is identified with Adelbert Ames and a small group who were at Dartmouth and Princeton in the early 1950s. Here the major focus of interest has been on perception, and the central thematic assumption has been that this is the psychological process by which the perceiving organism creates its own environment or *assumptive world.* This is the environment within which it carries out its purposes. The assumptive world is the product of a transaction, an action each upon the other, of the physical surround and the organism. The visual environment, for example, impinges upon the retina, creating a pattern of stimulation. A large number of spatial arrangements in the physical surround (equivalent configurations) will, of course, produce the same retinal pattern. However, the organism, through its transactions with the environment, learns to identify with various degrees of certainty the "true" nature of the configuration, and the sum total of its probability estimates about the external surround constitutes, at any given moment, its assumptive world.

A similar conceptualization was developed by Bruner and Postman (cf. Bruner, 1951) out of their concern for the relationship between perception and values. Through their studies of motivation and perception these writers were brought to the view that the organism is a hypothesis-testing system and experience with the system's physical surround was seen as resulting in an increasingly complex set of hypotheses about the world through a continuous pattern of formulation, testing, and confirmation or reformulation.

Coutu (1949), about the same time, took an important conceptual position by his insistence that the environment is a schematic concept, and that the organism reacts only to those stimuli to which it is sensitive in some peculiar way. The behaviorally relevant environment then is a system of meaning structures, some highly individual, deriving from the peculiar relationships that a particular organism shares with its surround, while others are common meanings shared by the organism with other organisms by virtue of its experience as a member of a particular biological class. Thus behavior is normal (i.e., in principle, predictable) to the conditions under which it occurs.

Garfinkle (1963), more recently, has emphasized that the environment of the *social* psychologist is a common-sense environment, constituted of actual events in a real and common world. For the observing-behaving organism it is *invari-*

ably presupposed and *invariantly* understood. Being psychologically invariant, the assumed uniformities of the common-sense environment serve as standards and the behavior that occurs represents action in terms of this assumed normative order.

The issue of the substance—physical versus psychological—of the environment becomes irrelevant when the model of the environment that is employed is a functional one. Such a model is, as was noted above, necessarily one that is schematized in terms of properties that are behaviorally relevant—that is, meaningfully related to the need systems, the perceptual apparatus, and the response repertoire of the organism, for which it provides the behavioral setting. Lewin conceptualized this "life-space" as a continuous field in which an interplay of forces produced some resultant behavioral locomotion. Simon (1956), however, has made it clear that any environment that will accommodate behavioral adaptation must possess properties that allow some simplification of the organism's choice mechanisms. It is not biologically or psychologically efficient that on each occasion of choice the organism engages in an exercise in problem solving. Instead of a continuous surface, Simon's life space is a branching system of paths, each branch constituting an occasion for choice. Since the environment is not a completely random arrangement of behaviorally relevant characteristics, each choice point possesses clues for response, with particular regions rich in properties that support some classes of behavior, poor in support of others. And when the model is thus schematized, it makes little difference whether these clues are physical or social.

The Formal Properties of the Environment. The concept of environment within the theoretical models of psychology, while it may vary in certain characteristics over the spectrum of views, has certain clearly identifiable formal properties. These may be categorized as either structural or functional.

Among the structural properties are dimensionality, complexity, intensity, continuity, contexture, extensity, locality, concreteness, nonrandomness, and contemporaneity. Every environment may be defined by reference to some finite, if undetermined, number of properties that differ in degree. The frame of reference may be physical, phenomenological, biological, or social, but the defining characteristics are, first of all, dimensions. Some environments, e.g. those of the traditional psychophysical experiment, may be successfully described by reference to a rather small number of dimensions. Others, most commonly those of the social psychologist, are much more complex and have a larger number of unidentified characteristics that are relevant for behavior. Some environments, e.g. those of the soldier facing an infantry attack, must be described as intense and insistent; others, like those of the daydreamer, as less impressive. All environments are contextual. They are always inclusive of the behaving organism. They have the quality of continuity, regardless of whether they are conceptualized in terms of Euclidean space in the manner of early students of perception, continuous surfaces in the manner of Gibson, topological space in the manner of Lewin, or an elaborate maze in the manner of Simon. Environments vary in extensity and a given environment may vary from a few square feet around the behaving organism to a rather larger area. It is problematical, with modern techniques of communication, etc., that the geographical environment may be considered to include areas widely distant from the organism. But in every case the distant stimulus is represented in the immediate surround. Environments always have locality and contemporaneity. They always constitute the "here and now" for the behaving organism. Just as a distant stimulus must have representation in the immediate surround, so must a past or future influence be represented in the specious present. All environments possess the quality of concreteness. Regardless of any theoretical orientation, psychologists identify them with immediate experience or with the sensorium. Finally, environments are nonrandom in their organization. Elements identified with particular localities occur in combination. The terrestrial environment, for example, always has an horizon or its equivalent. The textures of the environment above and below the horizon are systematically different, and it always has an up and down, a left and right, and a front and rear. Where there are doors, there are windows; where there is tropical fruit, there is sunshine and warmth; where there are people, there are certain human artifacts. Michotte's well-known studies of causality (1963) are an attempt to give psychophysical definition to this nonrandomness.

Considered in functional terms, environment always provides the medium in which behavior occurs. It always has a causal texture. That is, the behavior which occurs in it is linked inevitably, in more or less direct fashion, to the structure of the environment that accommodates it. Thus environment and behavior are complementary concepts, the definition of one being dependent upon the other. Meanwhile, the theorist's view of the direction of influence relating these two entities, has varied with his theoretical persuasion. On the one hand, the press of environment upon the organism has been emphasized by some; on the other hand, the role of the organism in shaping its environment has been stressed by others. Still again, their mutual interdependence has been pointed up by others. Thus, regardless of the model involved, there is agreement that environment has what may be termed, at least in everyday language, "means character." The nonrandom structure of a specific environment implies that it can accommodate only a limited number of classes of behavior, and that environments may be identified in terms of the types of behavior they will accommodate. By the same token, functional descriptions of environments are schematized and simplified in terms of the relatively small number of variables that are the major determinants of these classes of behavior. While the occurrences of specific behaviors within particular environments, and, indeed, even the structure of the environments themselves, may be described with a set of probability statements, specific environments have an assumed stability that is absolute, as Garfinkle (1963) has pointed out. Finally, while the functional environment contains forces that instigate behavior, it also includes elements that constrain or otherwise shape behavior. The effect of barriers in the life space upon the psychological mobility of the organism was a major concern of the Lewin group.

The Methodological Issue of the Environment. Bergmann (1962) has observed recently that Watson's important contribution was methodological rather than metaphysical: namely, the recognition that it is possible, in principle, to predict the behavior of the organism from information about relationships, both present and past, between the organism and its surroundings. This methodological assumption opened the door for the development of a modern analytical science of psychology. One may press the point a step farther. The nature of the assumptions that the behavioral scientist makes about these relationships will determine the nature of the methods which he devises to study these and thus the nature of the corpus of the behavioral science he creates. The Berlin Gestalt group, in their elaboration of the very important organizing principles, rejected or minimized the roles of both the environment and past experience in the structuring of perception. Their methods for studying perception have involved the use of simple, linear, two-dimensional forms, often nonsense figures, usually in black and white, with adult human observers. These are hardly circumstances capable of demonstrating the long-term and complex role that we now know that early experience plays in the organization of perception. In contrast, the view of the transactionists concerning the relationship of perception to the concept of equivalent configurations led to the use of simplified three-dimensional visual environments in establishing correlations between response mechanisms like accommodation and convergence and perceptual dimensions like perceived distance. The early behaviorist's assumption concerning the physical-physiological nature of behavioral variables, and his rejection of consciousness as a scientifically meaningful concept brought the dog, the white rat, and other subhuman animals into their own as laboratory materials. And the behaviorist's assumed primacy of the physical stimulus for behavior has been associated, for example, with the use of independent groups in studies of the relation of magnitude of reinforcement to performance efficiency.

Two broad methodological attitudes have been evidenced toward environment by experimental psychologists. On the one hand, environment, or more specifically the nonlaboratory environment, has been viewed as noise in the laboratory-observation system. Good experiments mean eliminating its influence or at least reducing this to a minimum. This is the rationale for the use of nonsense syllables as a laboratory device. It is the rationale of the maze, the problem box, the anechoic chamber, the nonsense form, etc. It is the rationale of the once popular procedural dictum that the handling of experimental animals was to be avoided at all costs. On the other hand, the environment may be regarded as a shaping, supporting influence that must be a central consideration in the design of experiments. Such a concern is found in Hebb's early studies (1949, pp. 296-299) of the enriching influence of the nonlaboratory environment upon rat behavior and in the more recent literature on handling and on early experience (cf., e.g. Hebb, 1955*a*).

The strategy of simplification that marks the tradition in the laboratory sciences poses serious problems for the psychologist. It has been easy to take for granted that nicely controlled situations yield data of general significance for psychology. Brunswik (1956) has challenged this assumption with his concepts of representative design and ecological validity. To yield data that may be used confidently to guide predictions of behavior outside of the laboratory, observational situations in the laboratory must resemble in their essential aspects the nonlaboratory situations to which generalization is intended. Resemblance, of course, is in terms of formal structure rather than substantive elements. By the same token, generalizations about behavior are about species over the range of situations that constitute their normal environment, and the laboratory studies aimed at producing such generalizations must sample the full spectrum of representative situations.

A final methodological point must be made. The concepts of environment at present available to the psychologist are, as noted earlier, essentially a priori concepts. While it must be granted that they have come in part from experience, what is needed is a systematically elaborated set of empirical concepts derived from experimental or quasi-experimental observations. Such observations should be directed toward identifying criterion variables that differentiate the several classes of environment that define the human (or monkey, dog, or rat) behavioral world, the dimensional properties of these variables, and their representative multidimensional patterns (Bevan, 1955).

IDENTIFICATION OF THE UNUSUAL ENVIRONMENT

Behavior as a natural phenomenon is always in some sense molar, involving the organism as a unit of participation, organized, and predictable. Thus, it is inevitable that it is always in some sense identifiable and interpretable. That is, it is characterized in terms of relationships between the organism and its environment that are viewed as stable, representative, and somehow reasonable or meaningful. It necessarily follows from this that behavior cannot occur in an environment that is totally novel to the organism. This is not to say that reactions will not occur in the novel environment, for the organism as a physical-physiological system is capable of response to physical-physiological change. But when the reaction is identifiable as behavior, it is also representative of, if not indigenous to, the ecology as well as the behavioral repertoire of the organism. Nor does this mean that the organism will not display behavior in strange settings—the Hottentot, if placed in the middle of Broadway, will most certainly display behavior even though he has never been there before, and the newly hatched duckling most probably will imprint the human experimenter. However, there is a clear distinction between the strange and the novel setting: a setting that supports behavior may differ substantively from any to which the organism has been exposed, but it also must be capable of characterization in terms that are formally similar to those that mark the organism's usual environment. The Hottentot may never have before seen tall buildings and noisy, fast-moving traffic, but he has been exposed to large, noisy, fast-moving objects. If his reaction to the latter is fear, then so should his response to the former. The duckling imprints to objects that subtend a particular visual angle and display certain kinds of movements. The mother duck is only one member of a class of equivalent stimulus configurations in this regard. Formal similarity ultimately means functional similarity, and this is what distinguishes the behavioral from other domains.

Examination of the recently rapidly expanded literature on behavior in unusual environments yields two impressions: first, that often motivating studies of this nature is the expectation that such behavior will be unusual behavior, and second, that frequently the primary referent in the identification of the unusual environment is the life space of the experimenter or observer. It is a dictum of psychological science that all behavior is natural to—i.e. lawfully follows from—the conditions under which it occurs. It is normal to the extent that it is representative of the organism and its ecology. It is easy for the human observer confined to a particular terrestrial environment to confuse the criteria for his own species and setting with those of other organisms and other settings. A review of papers on weightlessness prior to empirical reports from space flight reveals a great range of speculation about the unusual effects exposure to weightlessness was expected to produce. These have proved, for the most part, to be imaginative flights of fancy. Of course, for an environment to be unusual, it must constitute some departure from the environmental conditions to which the individual has become adapted. Thus, the conditions of stimulus impoverishment applied to the albino rat from birth in certain studies of early experience may well represent an unusual environment for the Norway rat in its natural habitat, but not for the laboratory animal which has known no other surroundings. For the latter it is the usual environment. Thus it is as important to investigate the effects of shift from rich to impoverished environment and vice versa upon the individual as it is to compare the effects of these contrasting conditions upon the behavior of independent groups of animals. Indeed, the great range and adaptability of behavior is perhaps most readily revealed by exposure of the individual to different settings whenever this is experimentally feasible.

The Properties of the Unusual Environment. Environments may be unusual in one or more of several respects. First of all, an environment may be unusual in that the dominant dimensions may differ qualitatively from those that typically characterize the organism's habitat. For example, man is primarily a visual animal. If he is placed in a situation in which orientation and adaptation are dependent to a major degree upon kinesthetic or auditory stimulation, he is in an unusual environment. This must certainly be the case on initial exposure to weightlessness or in orientation under water. The Douroucouli monkey is a nocturnal species. It is in an unusual environment when it is tested under diurnal conditions. Examination of unusual environments will undoubtedly reveal that environments that are truly qualitatively different are rare indeed.

More likely the unusual environment represents some difference in complexity or level of stimulation. Most laboratory environments are unusual in that they are simply beyond the range of ordinary experience outside the laboratory. In visual discrimination experiments, for example, the discriminanda may be characterized as differing on one or, at most, two or three dimensions. Care is taken to ensure that they are presented in the region of clearest vision. The visual environment is simple. Homogeneous, unstructured backgrounds are used. Shadows are removed. Illumination levels are uniform and constant. Stimuli potentially distracting to other modalities are ruled out. Hebb (1955*b*) has pointed out that sensory events have two functions. Focal stimuli give rise to perception or otherwise serve as cues for the evocation of specific responses. In addition, they provide a background of nonspecific stimulation identified with arousal level. Deese (1955) has proposed an activation hypothesis for vigilance behavior, which contends that vigilance reflects general arousal level. In the visual vigilance task, which involves the detection of the occurrence of weak, infrequently occurring stimuli, performance ordinarily displays a marked decrement over time. In contrast, in a recent experiment in the writer's laboratory (Bevan and Turner, 1964*a*) the task involved the occasional *absence* of a weak but frequently occurring auditory signal. Under this circumstance, detection performance was maintained at a constant level throughout a moderately long period of testing. Most commonly, unusual environments, on the one hand, involve extremes in level of stimulation impoverishment and, on the other, excessive stimulation. The former is the environment of Riesen's chimpanzees (1950), and, in certain respects, of the subject in the anechoic chamber, and of the astronaut in orbit. The latter is the environment of visual glare, of the boiler factory, and of the astronaut on launch and re-entry.

The Unusual Environments of the Present Chapter. I have been arbitrary both in deciding what would be the distinguishing features of the unusual environments selected for attention in the present chapter and in choosing the environments and behaviors to be discussed. Five criteria have been used: the environments selected are, first of all, unusual physical environments. In every case they are settings differing in some marked way from the natural environmental settings of higher-order animals, particularly of man. Secondly, they are environments which exert their influence upon behavior through their capacity for specific sensory stimulation. Thus, consideration has nowhere extended to effects upon behavior of such conditions as changes in oxygen-tension level, ambient temperature, the presence of harmful radiation, or the existence of toxic agents in the air, food, or water. Thirdly, they are environments readily identified as unusual by the subject exposed to them. Thus we have not been concerned with effects upon behavior of subtle variables like hypnotic suggestion or subliminal stimulation. Fourthly, the settings discussed are unusual environments for normal and intact organisms. Thus, the literature dealing with behavior in subjects suffering from sensory defects like blindness and deafness is not considered. Finally, each of the situations reviewed is marked as unusual in the level of stimulation. This could mean more intense than normal or less intense than that which usually characterizes the stimulus situation to which the human organism is exposed. However, the behavior situations reviewed all have in common the circumstance that they represent drastically *reduced* levels of sensory input. They are perception in the unstructured visual field, performance during sensory isolation, performance during weightlessness, and vigilance behavior.

The selection of these topics has been arbitrary but guided by several considerations. First, these topics represent laboratory problems of unusual interest to the present writer. Secondly, they are topics which have attracted special attention from a wide spectrum of psychologists during the past decade. But, most important of all, they involve problems which have been vehicles for conceptual development—even reorientation—of the most fundamental and general sort. Extended treatment has been accorded the topic of vigilance behavior, because it is providing for the experimental psychologist a means of effectively examining the influence of a wide range of both situational and personal variables upon performance efficiency. Serious consideration was given to including a review of selected behavior situations marked by intense stimulation —i.e. behavior during high-level noise, performance during acceleration, and audiogenic seizures. Examination of .the literature on these topics, however, provoked the impression that they are essentially narrow, or at least specialized, in their implications for behavior and empirical in their treatment. The first two are of peculiar practical concern to the psychologist concerned with behavior tolerances in such special environments as the modern industrial plant or during space flight. The last topic mentioned is of interest to physiologists concerned with convulsive mechanisms.

The present writer prepared a review of the literature on audiogenic seizures several years ago (1955), and Chamber's recent comprehensive treatment (1963) of behavior during acceleration is well worth the attention of the reader interested in this problem.

PERCEPTION IN UNSTRUCTURED VISUAL ENVIRONMENTS

Vision in the Ganzfeld. Psychologists have for some time been interested in the visual response under homogeneous retinal stimulation. In Gestalt theory the simplest perceptions were identified with this condition. In 1930 Metzger reported the observations of subjects facing a large plaster wall with the ambient illumination such that the texture of the surface was not apparent (the *Ganzfeld*). Since that time a number of experimenters (Hochberg, Triebel, and Seaman, 1951; Gibson and Waddel, 1952; R. H. Brown, 1957; Cohen, 1957; Westheimer, 1957; and Miller and Ludvigh, 1958) have used a variety of techniques to produce the condition of homogeneous stimulation. Subjects exposed to this condition express difficulty in describing their experience. Most commonly, it is characterized as being immersed in a dense diffuse fog of indeterminate size and distance. Sometimes there is an awareness of the vitreous opacities and retinal blood vessels. And exposure to homogeneous stimulation for from ten to twenty minutes frequently produces periods of "blanking out," in which there is apparent loss of vision and even an uncertainty as to whether the eyes are opened or closed (Cohen, 1960).

An interesting aspect of vision in the *Ganzfeld* is the color adaptation that occurs with exposure. Hochberg, Triebel, and Seaman (1951) report a disappearance of hue from a red or green *Ganzfeld* after about three minutes. This adaptation appears to be for the most part a peripheral phenomenon, since stimulation of an unexposed eye will yield the chromatic response, while the opposite exposed eye gives black or dark gray. Cohen (1958*a*) has reported similar observations and in addition noted that there is an absence of whiteness constancy in the chromatic field.

Figure Perception and the Ganzfeld. Detection of inhomogeneities in the *Ganzfeld,* meanwhile, is relatively difficult. Miller and Ludvigh (1959) report that in the *Ganzfeld* it may take as long as twenty seconds to detect a patch which is six times the threshold size. Of course, the smaller the size, the longer the detection time. When the target subtends less than a half degree of visual angle, detection times may be quite long. Ten-minute targets, for example, may require something of the order of 100 seconds to find, and whether the target is moving or stationary seems to make little difference. Partly, at least, this appears to result from the subject's being unsure of where he is looking. When a target-figure does not represent a sharp brightness gradient with the ground, the fog appears to lie in front of the figure. When the brightness contrast is pronounced, the reverse relationship holds (Cohen, 1957). With the presence of an inhomogeneity in the field, the density of the fog is reduced and its apparent distance enhanced. Stationary figures in the *Ganzfeld* tend to be unstable. Autokinetic movement is a common observation (Cohen, 1958*b*).

If detection is difficult in the *Ganzfeld*, so is figural recognition. When the inhomogeneity results from a color difference, the impression is of something amorphous, identified chiefly as more highly saturated than its surround (Cohen, 1958*a*). When an intensive gradient is involved, depending on how steep it is, figure qualities (the spatial differentiation of figure-ground and the presence of contour and surface texture) are more pronounced, and apparent size tends to be greater than actual size (Cohen, 1957). When figures are presented tachistoscopically during periods of blank-out, they are recognized only as a flash, and, even during intervening periods of active seeing, short periods of exposure to the *Ganzfeld* interferes with recognition. Among simple geometrical figures, the most readily identified are circles, squares, diamonds, and equilateral and right triangles. More difficult are rectangles, parallelograms, and the letters T and L. Most difficult are stars, pentagons, hexagons, octagons, crosses, and the letter X. Generally, the greatest interference is with the geometrically more complex figures (Cohen, 1960).

Interesting in this connection is Whiteside's discovery of empty-field myopia (Whiteside, 1957). Confirmed by a number of investigators (e.g., Westheimer, 1960), this consists of a continuously fluctuating state of accommodation with focus at a maximum distance of 6 or 7 ft. The practical implication of this information for the efficiency of visual detection in the empty field is obvious. As Brown points out (1957), if this condition could be corrected, the range of target detection could be doubled. One suggestion has involved the use of devices that would produce focus at optical infinity. Brown has employed a collimated reticle in two experiments on target detection with negative results. However, he does provide evidence that persons normally displaying some degree of hyperopia were superior in performance.

When the intensity gradient between figure and ground is increased, the figure appears to advance and the fog recede. Meanwhile, rotation of the figure tends not to be noticed (Cohen, 1960). And while location in depth in the *Ganzfeld* is difficult, the detection of lateral displacement is another matter. Miller and Hall (1962) report that subjects can detect displacements of two or three degrees from the center of the field and also identify the occurrence of the lateral movement of the target, although the movement threshold is higher in a homogeneous field (Brown, 1931).

Perceptual Constancy and the Ganzfeld. The perception of the three-dimensional objects of everyday life relates to the nature of the stimulus context with just as much intimacy as does the perception of the simple two-dimensional configurations of the laboratory. The phenomenal experience of the size, the shape, the color of objects depends in part upon the perceptual characteristics of their surround. Consideration of this fact takes place within the general problem of the perceptual constancies, and psychological thought on this matter has been largely determined by Gestalt psychology. Theorists of this persuasion have all begun with the assertion that all perceptual organization is organization within a framework. Koffka (1935, Chap. 6) divides the behavioral environment into two parts, objects and framework, and identifies one overriding principle, that of the *invariant*, which defines their relationship. Underlying every instance of perceptual constancy is some critical relationship between objects and ground that is invariant. In the case of the most widely considered types of constancy (size, shape, and color), the invariants are, respectively: size at a given distance, shape in a given orientation, and relative whiteness at a given ambient illumination level. Gibson states the position in the extreme in his view that "we perceive a quality in the visual world which might be called *scale*" (1950, p. 180). In this he leaves nothing to cognitive construction but attributes constancy to the configurational properties of stimulation.

The experimental paradigm for studying constancy has involved comparing judgments of some property or properties of objects in a structural setting with judgments of the same objects viewed through an occlusion screen, reduction tube, or some other device that restricts perception of the surround. This usually has the effect of not only limiting the size of the ground but of reducing the amount of detail available for view. Katz's experiment on whiteness constancy is representative (Koffka, 1935, p. 241). A light gray paper is presented in a dark corner of a room, and a color wheel with adjustable black and white sectors is placed near a window. The observer is required to adjust the sectors to a match. The observer next views the patch and the disc through separate apertures in a screen. When this is done, the matching disc is seen to be brighter than the gray patch. Now if a match is made with the reduction screen present, the rotating disc will appear almost black when the screen is removed.

In contrast to Gibson's view that the constancies derive from properties inherent in the stimulus configuration is that of the transactionists, who contend that the constancies result from the set of assumptions that the perceiver makes about a particular pattern of proximal stimulation (Ittleson, 1951). Evidence is drawn from an ingenious set of demonstrations, in which common objects or representations of them are presented against a homogeneous background. For example, if three photographic reproductions of an ordinary playing card are presented against a black field, such that a double-sized card is 8 ft distant, a normal-sized card is 6 ft distant, and a half-sized card is 4 ft away, an observer, viewing this array with one eye from a specified fixed viewing position, will report seeing three normal-sized cards at different distances. But the large card will be reported as near, the normal as intermediate, and the small as far. While size is constant, apparent distance varies negatively with actual distance. If next a wrist watch in a transparent case the size of the half-sized card is placed at 4 ft, the normal card at 6, and a magazine the size of the double-sized card at 8 ft, the report will be of three different sized objects at different distances—the watch nearest, the card at an intermediate distance, and the magazine farthest away. Here physical distance and apparent distance correlate positively, and the condition of constancy is preserved. In this latter case, it is held that the assumptions made by the judge concerning the relative size of the three objects leads the observer to interpret their respective retinal sizes, such that veridical judgments are made of their location in the homogeneous field. Indeed, Ittleson and Ames (1950) report that accommodation and convergence are to apparent rather than actual distance. The *assumptive world*, which is the transactionist's version of the behavioral environment, is built up as a pattern of probable significances and the weighted average of past experience.

Visual Efficiency in the Ganzfeld of Outer Space. While early concern for perception in the *Ganzfeld* was motivated largely by considerations of a theoretical nature, the present lively interest is prompted more by practical problems related to high altitude and space flight. This latter interest is primarily empirical, because the problems involved are more those of behavioral engineering than of behavioral science and because, as was pointed out on another occasion (Bevan, 1962), extant perceptual theories provide little in the way of highly articulated, precise predictions of specific perceptual events in outer space. The problem of empty-field myopia is of less concern for orbital and interplanetary space flight than for high-altitude terrestrial flight, for, as Colonel Glenn noted after his orbital flight, there has been a general underestimation of the amount of detail that can be seen during orbit (1962) and, as Brown, et al. (J. L. Brown, 1961*a*) have observed, the lack of atmospheric dispersion in space will permit constant visibility of stars and thus make available a basis for distance accommodation. Meanwhile, visual orientation in outer space will require the capability of recognizing changing stellar patterns as the celestial position changes. It will also involve a dependence upon binocular cues for distance, and the identification of objects in terms of whatever portion of their surface happens to be illuminated by the sun. Finally, it will involve a new set of references that yields the impression of up and down (Arnoult, 1959).

BEHAVIOR UNDER CONDITIONS OF STIMULUS IMPOVERISHMENT

The last decade has seen a rapidly expanding interest in behavior under conditions of stimulus impoverishment. Originating within a theoretical context, interest in this phenomenon has been sustained, in part at least, by the potential practical significance that information about it may have for confinement associated with POW camps and submarine and space flight operations. A number of recently published books have directed major attention to the effects of sensory deprivation (e.g., Biderman and Zimmer, 1961; Burns, Chambers, and Hendler, 1963; Fiske and Maddi, 1961; Solomon et al., 1961) and the list of relevant journal articles must certainly approximate two hundred. In this present section interest is centered about the effect of the deprivation environment upon the behavior of adult human beings. Consideration of the influence of early impoverishment upon behavioral development is left for another context and another time.

Early interest in sensory deprivation is identified with Hebb's model of the neurophysiological substratum of behavior (1949). In developing his thesis that even the simplest forms of behavior represent a long stimulus-response history, he relied heavily on Riesen's pioneer studies of primate visual development (1950). In these experiments, chimpanzees deprived of visual stimulation during the early months of their lives failed to make simple form and color discriminations, displayed poor visual acuity, and the acquisition of simple visual responses occurred only after much practice and was of a low order. By 1955 Hebb (1955*b*) had created a conceptual nervous system that needs and actively seeks stimulation for its growth and development. In this exercise he relied heavily upon an experiment carried out in his laboratory by Bexton, Heron, and Scott (1954). In this investigation, subjects, who spent several days reclining on a bed in a small cubicle, with no pattern vision and with auditory, tactual, and kinesthetic stimulation reduced to a minimum, displayed restlessness, emotional lability, poor motor control, and, after release from the cubicle, decrement in IQ test performance. The most striking effect, however, was the occurrence of vivid—primarily visual—hallucinations that varied from free forms to highly organized perceptual experiences. About the same time, Lilly (1956) drastically reduced the level of stimulation by submerging subjects in a tank of water at near body temperature and reported emotional tension and hallucinations. His conclusions about brain function were similar to Hebb's.

The condition of sensory deprivation provides at once both a technique of elemental behavioral analysis and a representative environment that may become in the years ahead a common human experience. As an experimental technique it has taken several forms: the drastic reduction of level of sensory input; the elimination of the patterning or organization of sensory input; and social isolation (Burns and Kimura, 1963). Data collection techniques have involved both the periodic sampling of a wide range of behavior tasks and the intensive, essentially continuous observation of performance on a single task. One situation involving the drastic reduction of sensory input is that used by Vernon and Hoffman (1956) to study the effect of sensory deprivation on learning rate. Isolation occurred in a lightproof, soundproof, floating room. Subjects wore earplugs and movements were restricted by their wearing of cardboard gauntlets while reclining. An example of isolation with the elimination of stimulus patterning is found in the original McGill study (Bexton, Heron, and Scott, 1954). Subjects wore translucent goggles that prevented form discrimination, and they were exposed to a constant level of masking noise. Movement patterns were reduced by reclining and the wearing of gauntlets. The USAF School of Aviation Medicine Space Cabin Simulator provides an example of social isolation (Flaherty, Flinn, Hauty, and Steincamp, 1960). This is a small altitude chamber containing an aircraft cockpit seat that can be made up into a cot and a command panel containing twenty-four lights and switches. Various flight profiles are simulated, and the subject performs discrimination, judgment, vigilance, and problem-solving tasks. The study of Zubek et al. (1962) on the effect of prolonged isolation upon perception illustrates designs which sample behavior on a wide variety of tasks. Eight different perceptual tests were administered before and following isolation. In contrast, the study of Vernon and Hoffman (1956) traced changes in anticipation learning during the course of isolation and over a period of time following the isolation.

Two broad types of variables have been considered in the literature of sensory deprivation. These are the two classes found generally in psychological investigations: situational factors and personal variables. Among the former are the level of sensory input (in the McGill experiments —e.g. Bexton, Heron, and Scott, 1954—translucent goggles and white noise were used to maintain sensory input but minimize its patterning) and the duration of the deprivation period (the period of deprivation has varied from a matter of minutes in the 1959 Rosenbaum, Dobie, and Cohen experiment on recognition thresholds to fifteen days in the Adams and Chiles 1961 confinement study). Among the personal variables are the subject's prior knowledge of the deprivation phenomenon (Jackson and Kelly, 1962, suggest that prior knowledge has a dominant influence upon the hallucinatory experiences of subjects during isolation), prior experience in the deprivation situation (Pollard, Uhr, and Jackson, 1963, compared data from two eight-hour periods of deprivation one week apart), suggestibility (Kandel, Myers, and Murphy, 1958, compared the reports during visual deprivation of subjects who had been told that visual experiences were a normal occurrence with the report of those who had been told that such phenomena occur in psychiatric patients) and personality characteristics (Holt and Goldberger, 1959, correlated responses on a series of personality tests and clinical Q-sort ratings with reactions during isolation).

Perception during Stimulus Impoverishment. The general impression of deprivation effects is that they are decremental. However, there are frequent exceptions. Doane, Mahatoo, Heron, and Scott 1959 report data that suggest that both visual acuity and the two-point tactual limen improved during isolation, and Vernon and McGill (1961) report that four days of isolation increased pain sensitivity. Meanwhile, although Ormiston and Finklestein (1961) found no decrement in warning light monitoring during forty-eight-hour confinement in a small capsule, Adams and Chiles (1961) reported decrement in auditory vigilance over a fifteen-day confinement, Zubek et al. (1962) described decreased efficiency in both auditory and visual vigilance, and Alluisi and Hall (1963) in auditory vigilance during shorter periods of confinement. Vernon, McGill, Gulick, and Candland (1959, 1961) describe progressive loss in performance on a color discrimination task with confinement up to seventy-two hours, and Zubek (1962) reports a similar finding with prolonged isolation. Consistent with this are the results of Doane, Mahatoo, Heron, and Scott, which indicate an enhancement of color adaptation with confinement.

The McGill studies indicate that after several days of isolation distortions of visual shape are prominent (Bexton, Heron, and Scott, 1954; Heron, Doane, and Scott, 1956; Doane, Mahatoo, Heron, and Scott, 1959). Objects appear fuzzy in outline and lose their three-dimensional appearance, shapes change, and shape constancy is reduced. Freedman, Grunebaum, and Greenblatt (1961) report similar perceptual changes with simple geometrical figures—segments stretch and bend and halos occur—which persisted for as much as an hour. However, Vernon and Hoffman (1956), with as much as forty-eight hours of deprivation, failed to obtain evidence of phenomenal changes in form on questioning their subjects. Rosenbaum, Dobie, and Cohen (1959) found no shift in visual recognitive thresholds after short periods of visual deprivation, and Adams and Chiles (1961) report improvement in pattern discrimination for subjects in the Lockheed confinement situation. Both Doane, Mahatoo, Heron, and Scott (1959) and Freedman, Grunebaum, and Greenblatt (1961) describe a measurable loss of size constancy with simple figures, while Zubek et al. (1962), with prolonged isolation, failed to obtain such a change. Meanwhile there have been no reports of reliable change in depth perception in two studies in which this has been measured (Vernon, McGill, Gulick, and Candland, 1961; Zubek et al., 1962). In contrast to these ambiguous data on the perception of static forms, data indicate clear effects of deprivation upon the perception of moving objects. After only brief isolation, Ormiston (1958) obtained both lowered initial and increased terminal thresholds for apparent movement, and both Held and White (1959) and Freedman and Held (1960) describe a decrease in apparent speed for stimuli in both a dark and a patternless visual field. In curious contrast to these last data are the Goldberger and Holt (1958), the Ruff, Levy, and Thaler (1961), and the Pollard, Uhr, and Jackson (1963) reports of the underestimation of time during confinement.

Perhaps the most striking of the perceptual or perceptual-like effects are the dramatic hallucinations reported throughout the literature, beginning with the first McGill (Bexton, Heron, and Scott, 1954) and the Lilly (1956) studies. Typically, they begin as simple geometric forms —dots and simple patterns. Then they become more complex abstract patterns or fragments of recognizable figures. Finally integrated scenes appear: for example, soldiers march across a field, or prehistoric animals roam in a tropical habitat (Heron, 1957). The subject has no apparent control over the occurrence of these hallucinations. The following is an excerpt from a protocol taken during a simulated space flight (Flaherty, Flinn, Hauty, and Steinkamp, 1960, p. 6): "Subject C was a 32-year-old professionally trained man. After entering the chamber, he dozed for the greater part of the 6 hours of simulated pad time. Throughout the early half of the subsequent flight his spirits were excellent and his proficiency remained high. At the end of the 22nd hour of flight the subject suddenly shouted that the screen of the television set in front of him on which the operator task was presented, had turned brown and was smoking. He asked for it to be turned off immediately and held a blanket up to protect his face from what he believed to be a fire. The observer quickly uncovered the chamber port to view the TV monitor and saw nothing unusual. He was unable to allay the anxiety of the subject, who insisted that a fire had occurred." While the hallucinations identified with isolation are predominantly visual, they also occur for hearing, taste, smell, and somesthesis (Jackson and Kelly, 1962). Reports differ concerning the extent to which hallucinations occur in a sample of subjects. Twenty-five of twenty-nine subjects in the McGill laboratory reported some form of hallucinatory activity. In contrast, eight of seventeen confined in a respirator in Wexler, Mendelson, Leiderman, and Solomon's laboratory (1958) and ten of fifty-five in the Princeton studies (Vernon, Marton, and Peterson, 1961) reported such activity. Vernon, Marton, and Peterson point out that absolutely maximum conditions of deprivation do not produce hallucinations. This coincides with the findings of Held and White (1959), indicating that shifts in apparent speed during isolation were less pronounced during darkness than during unstructured illumination. Jackson and Kelly (1962) attribute the occurrence of hallucinations in part at least to the subject's prior knowledge of isolation phenomena and an attitude of expectation that hallucinations would occur. Pollard, Uhr, and Jackson's (1963) findings that subjects given suggestions about the experimental situation made more reliable responses than a control group and also displayed a greater reduction in responses on a repeated isolation session support this position. In addition, Ziskind and Augsburg (1962) present evidence that deprivation hallucinations are fragments of normal imagery.

Motor Performance During Stimulus Impoverishment. Little attention has been directed toward motor responses under conditions of sensory isolation. Available data indicate deterioration with increased exposure to this environment, although the relationship may not be monotonic. Bexton, Heron, and Scott (1954) suggest poorer eye-hand coordination as suggested by performance on the Wechsler Digit Symbol test and on handwriting tasks. Vernon, McGill, Gulick, and Candland (1959) report rotary pursuit performance to be poorer than normal after forty-eight hours of deprivation but not after seventy-two. Results were similar for mirror tracing, although Scott, Bexton, Heron, and Doane (1959) failed to find any decrement in a similar study. Performance of a rail-walking task showed progressive deterioration throughout a seventy-two hour confinement period. Recovery of motor skills following isolation appears to be rather rapid.

Intellectual Functioning and Stimulus Impoverishment. The Bexton, Heron, and Scott (1954) study has set the pattern for the study of deprivation effects upon intellectual functioning. With the exception of two or three experiments involving a laboratory task (rote learning of meaningful words), the approach has been to administer psychometric tests (for the most part standardized tests) before, sometimes during, and after confinement. While the data are not highly consistent, the initial impression is one of seemingly progressive decrement with increasing confinement. In this original study, with periods of confinement up to forty-eight hours, performance on the Kohs Block and Wechsler Digit Symbol tests was poorer in confined subjects after confinement than in unconfined control subjects. In an experiment on the learning of adjectives by the anticipation method, Vernon and Hoffman (1956) reported facilitation during a forty-eight-hour period of confinement with no difference in performance from controls either before or after isolation. The pattern of data was essentially the same with seventy-two hours of deprivation (Vernon and McGill, 1957), although the effects were not statistically reliable. Arnhoff, Leon, and Brownfield (1962) found no effect of isolation upon the learning of adjectives, when the isolation conditions were those of the McGill laboratory. Zubek, Sansom, and Prysiazniuk (1960), while they observed no significant facilitation of rote learning, also found no evidence of decrement in this function over six daily tests during isolation, and Zubek, et al. (1962) reported facilitation in a more elaborate repetition of the 1960 experiment.

The studies involving psychometric measures of intellectual functioning have employed periods of isolation varying from eight hours to fifteen days. Eight hours of isolation appears not to have any effect on simple cognitive tasks, although there is indication that it detrimentally affects logical deduction and performance that requires holding several things in mind at once (Goldberger and Holt, 1958; Holt and Goldberger, 1960). Ormiston and Finklestein (1961) report that forty-eight hours of confinement in a small simulated aircraft escape capsule had no effect upon performance on a large battery of tasks including logical reasoning. However, Scott, Bexton, Heron, and Doane (1959), employing the McGill isolation situation, describe poorer performance in isolated subjects on Koh's Blocks, the Wechsler Digit Symbol test, Thurstone-Gottschaldt figures, copying a passage, the McGill Picture Anomaly test, and the Delta Blocks. In contrast, Zubek, Sansom, and Prysiazniuk (1960) with daily measures through six days of individual isolation, found no differences in performance on tests of verbal fluency, verbal reasoning, number facility, numerical reasoning, abstract reasoning, and spatial relations. Only recent recall and recognition were adversely affected, and this persisted for at least one day following isolation. The 1962 study of Zubek et al., however, yielded evidence of impairment on cancellation performance, dexterity, number facility, numerical reasoning, abstract reasoning, spatial relations, verbal fluency and recognition with no clear loss in recall and verbal reasoning. The Lockheed crew-confinement study of fifteen days, meanwhile, found impairment in arithmetic computation and probability monitoring (Adams and Chiles, 1961).

Emotional Behavior during Stimulus Impoverishment. Probably next to hallucinations the most prominent effect of sensory isolation is confusion and irritability. Bexton, Heron, and Scott described this in the early study, and it has been frequently reported since that time. The initial effect of confinement is usually one of relaxation and pleasantness. However, this response gradually shifts to one of tension and irritability. Berlyne (1960, p. 188ff.) identifies this with boredom, on the assumption that low levels of stimulation, like high, may be aversive. Irritability is thought to result from a rise in arousal level associated with internal factors, while the lack of stimulation during sensory isolation is assumed to render the cortex incapable of controlling arousal. Emotional distress may reach pathological proportions as it did in the claustrophobia of three of four subjects during simulated solo thirty-six-hour orbital flights (Flaherty, Flinn, Hauty, Steincamp, 1960). There are, of course, individual differences in susceptibility to emotional disturbance. Goldberger and Holt (1961) identify the positive and negative effects of isolation as independent reaction patterns—rather than response phases—associated with differences in personality organization. Subjects experiencing positive affect were described as resourceful, flexible, and adaptive; those who experienced negative affect were described as passive and lacking in ego strength.

Some interest has been evidenced in the prediction of emotional reaction to the isolation situation. Vernon and McGill (1960) report that subjects, who during confinement spent more time pressing a button to see a simple geometrical configuration in dim illumination, were able to tolerate confinement for considerably less time. Petri, Collins, and Solomon (1960) have hypothesized that the isolated subject is starved for sensation and any such response, including pain, will alleviate his stress. They report that subjects most tolerant of deprivation judge pain to be more intense than those less able to tolerate deprivation. In addition, only subjects without pain during isolation experienced hallucinations. A more recent study (Peters, Benjamin, Helvey, and Albright, 1963), however, failed to confirm this hypothesis. Although no verifiable correlation was found, the data suggest a positive relationship between pain and isolation tolerance. Camberari (1958) has reported that suggestible persons reported affect during water immersion, while nonsuggestible subjects did not, and Jackson and Pollard (1963) point out the similarity between the isolation experience, as described by subjects during their isolation, and pre-isolation conceptions of the nature of the isolation experience. That the situation itself is, however, capable of enhancing suggestibility is indicated by the Scott, Bexton, Heron, and Doane (1959) finding that susceptibility to propaganda increases with confinement.

Group Performance During Stimulus Impoverishment. The matter of group performance under conditions of confinement has commanded attention for practical reasons. Long-range submerged missions are already a reality for submariners, and there is confident expectation that multiple-man crew space flights will be possible within a few years. Ruff, Levy, and Thaler (1959) have studied the performance of five-man crews under simulated five-day flight missions. Beginning with highly motivated air crews they report that this experience is no more than moderately stressful. While there were transient signs of ego impairment, some subjects appeared more mature and capable of handling conflict during than they did before the experiment began. Similarly, Adams and Chiles (1961), although they found some performance decrement, conclude that with well-motivated crews and properly planned duty-rest cycles, successful fifteen-day missions with multiple-man crews are possible. Indeed, a majority of the Lockheed subjects felt capable of an additional fifteen days of confinement if necessary. However, Gunderson and Nelson (1963) report, from observations of groups isolated over the winter in the Antarctic, an increase in the frequency and severity of emotional distress with prolonged isolation. There was a general decline in work satisfaction, social relations, and group accomplishment displayed by their subjects, and it was concluded that, while possible, the maintenance of group harmony and efficiency was at best difficult.

Theoretical Implications of Stimulus Impoverishment. The phenomena of sensory deprivation or stimulus impoverishment have proved fascinating not only to the experimentalist but also to the theorist. Many theorists of widely different persuasions and focal interests have turned their attention to the variety of data summarized above. Their language has been both psychological and neurophysiological. Hebb's theoretical orientation has emphasized the elaboration of structure through function, and he has seen the isolation data as indicating the importance of stimulation for this elaboration (1955*a*). It has provided a background for his discussion of the neurological basis of the awareness of self (1960). Heron (1957) attributes the impairment of behavior through isolation to the effect of reduced input upon the reticular activating system of Magoun. Lindsley (1961) takes a similar view. In contrast, Lilly (1956) speaks of the build-up of body libido during the quiescent state associated with the sensory deprivation situation. He hypothesized a failure to discharge somatically under this condition with a discharge into fantasy. Both Held (1961) and Teuber (1961) also have given attention to motor processes and talked in terms of a relatively simple neural mechanism adapted from the feed-back model of von Holst that correlates, stores, and uses signals in its comparator system. In deprivation, they assume that internally generated signals replace exafferents.

The psychological theories of sensory deprivation effects have been functional in orientation. Some have stressed the role of internal disorganization in deprivation behavior (e.g. Bruner, 1961; Freedman, 1961). Kubzansky (1961) associates the deprivation effects with the breakdown of internal norms and points out the importance of developing both spatial and temporal anchors in the external environment. Others stress the organizing role of internal processes. Robertson (1961) believes that with the detachment from context related to sensory deprivation, there is a heightened projection of thoughts and feelings and a freer reaction to residual input, and Jackson and Pollard (1962) point out the role of the subject's expectations of the isolation experience, his motivation for the experience, and the technique of reporting his experience as influences upon the isolation data. Some of the psychological interpretations are psychoanalytic. Goldfried (1960) describes the isolation condition as one in which the environmental supports for reality testing are removed and the ego-environment distinction weakened. Kubie (1961) refers to an influence of isolation upon the constant stream of afferents between the conscious and the pre-conscious, and Goldberger and Holt describe their data on individual differences in response to isolation in terms of the manner in which the primary process is handled.

The state of our understanding of sensory deprivation, however, is best characterized by Hebb's concluding remark at a 1961 symposium: "I am still unable to explain the effects" (p. 43). Furthermore, competent explanation must necessarily wait upon the resolution of the frequent contradictions that exist within the empirical literature.

WEIGHTLESSNESS

The concept of weightlessness became psychologically meaningful when German fighter pilots during the Second World War developed an attack maneuver that included flying a Keplerian trajectory. This involved the aircraft's describing a parabolic path that in fact is a section of a very elongated ellipse with one focal point at the earth's center. In this segment of the ellipse the force propelling the aircraft away from the earth is balanced by the force of gravity, and a brief period of weightlessness results. Actually, there are several ways of producing weightlessness (cf., e.g., Loftus and Hammer, 1961; Gerathewohl, 1961). First, there are ballistic devices. These include, in addition to the airplane, drop towers and elevators, acceleration sleds, and ballistic missiles. In all of these the state of weightlessness is achieved by placing the subject in a condition of free fall. There are also frictionless devices like the air-bearing platform that simulates weightlessness in the horizontal plane by allowing for inertial movement of the subject. And there is fluid immersion in which weightlessness is simulated by placing the subject in a medium with a density approximately equal to that of the human body. This provides for a distribution of the body's support over its entire surface. None of these are satisfactory. The ballistic devices provide for periods of weightlessness that are too short to allow for much in the way of controlled observation. And the simulation methods do not provide for full simulation. Only orbital and interplanetary flights include the conditions necessary for a definitive study of weightlessness, and observations thus far cast doubt on the probability that its effects are as profound as had been earlier expected.

The empirical literature on weightlessness to date has been both sparse and spotty, and it has consisted primarily of exploratory observations directed toward determining if weightlessness has an effect, especially a deleterious effect, upon this or that specific performance variable. Meanwhile, speculation about it has been identified with two points of view: that it has no significant relationship to behavior or performance efficiency, and that it poses a major threat to comfort, performance efficiency, and even biological integrity (Flickinger, 1959). Accumulating data appear to support the first alternative. Introspective reports following early flights inducing brief periods of weightlessness indicated confusion and disorientation (Gerathewohl, 1959). Gerathewohl (1956) summarized the experience of sixteen subjects during periods of weightlessness in a T-33 jet trainer. Most experienced no confusion or discomfort, but a few had sensations of motion. A few reported nausea and the symptoms of motion sickness. Such discomfort, when it occurs, may be attributed reasonably to emotional distress or the maneuvers attendant upon the induction of weightlessness, rather than to the condition of weightlessness itself. Colonel Glenn reported weightlessness to be a pleasant experience with no nausea or vertigo, even when he attempted to induce these symptoms by shaking his head or rapidly moving his eyes (1962). However, Gazenko and Yadovsky (1961) indicate the first Russian in orbit to have experienced an increasing discomfort after the sixth orbit. These individual differences may be reconciled, when we understand better the reasons for the wide individual differences associated with susceptibility to sea sickness and other forms of motion sickness. At the same time, adaptation, even in the free-floating state, appears to occur rapidly and a long-term habituation regimen seems unnecessary.

Sensory Function and Weightlessness. Although there has been concern for vision as a variable in orientation, little attention has been directed to a study of the basic visual functions during weightlessness. Pigg and Kama (1961) have measured visual acuity during fourteen-second periods of weightlessness in a C-131 aircraft. There was a reliable six per cent loss in acuity when measurements were compared to measurements at one g in the aircraft, and a ten per cent loss when compared to laboratory measurements. However, such a loss is not regarded to be of practical significance. Hammer (1962) had subjects make judgments of the visual vertical under in-flight conditions varying from zero gravity to 1 g. The errors observed were small but increased (1.8 to 3.5°) as the g-level decreased.

Greater attention has been given to vestibular functions, and experiments have involved both animal and human subjects. Early ballistic rocket shots carried both mice and monkeys (Henry et al., 1952). While normal animals displayed signs of disorientation, labyrinthectomized animals seemed not to be disturbed at all during exposure to zero g. Von Beckh (1954) observed the accuracy of water turtles in striking at bait while exposed to zero-g conditions. Here again, normal animals displayed disturbance, while an animal with labyrinthine damage struck normally. Both Gerathewohl and Stallings (1957) and Schock (1959) have reported on the cat's righting reflex during weightlessness. When the animal was released during the first few seconds of the weightless state, the reflex appeared. When, however, the animal was restrained for the first fifteen or twenty seconds of weightlessness, it failed to right itself. This was the case whether or not the animal was blindfolded and contradicts the pre-experimental assumption that vision would be crucial for orientation during the zero-g state. In addition, Schock found that damage to the labyrinths or the vestibular cortex failed to alter these results. The failure of pigeons to make compensatory head movements following rotation has led King and Wade (1961) to conclude that reticular stimulation fails to occur during weightlessness. Brown (1961*b*) tested orientation in human subjects submerged to a depth of neutral buoyancy, and he found that this was poorest when the head was down or tilted back. However, head movements aided orientation. Again, the results suggest the importance of the utricles for normal orientation. In contrast, Roman, Warren, and Graybiel (1963*a*) describe no change in sensitivity of the semicircular canals to stimulation during periods of in-flight weightlessness.

Gerathewohl and Stallings (1958) have described what they call the oculoagravic illusion. This is a shift in the location of the visual afterimage of a spot of light with a shift from positive g to weightlessness. In the former condition the afterimage moves downward; during weightlessness it shifts upward in the visual field. Gerathewohl (1952) had predicted this illusion from his knowledge of visual displacement with accelerations greater than one g and Schock (1958) also confirmed it. Simons (1961) has reported a similar shift of a luminous disc in a dark field with free-floating weightless subjects. Both the oculogravic (displacement under conditions of acceleration) and oculoagravic illusions are thought to result from utricular stimulation (Graybiel, 1952; Yuganov, Kas'ian, and Yazdovskii, 1960; cited by Loftus and Hammer, 1961). However, the significance of visual illusions in the weightless condition is yet to be clearly determined. Schock's data indicate that while target displacement occurred with the shift from positive acceleration to the zero-g condition, it stabilized or oscillated only slightly during the latter. He, therefore, concluded that the illusion might not occur during weightlessness. Meanwhile, Roman, Warren, and Graybiel (1963*b*) have reported that when weightlessness was induced following a period of negative acceleration, the apparent target position during zero gravity did not differ from that during negative g, and they concluded that neither angle of entry nor positive g can be responsible for the observed shift. They suggest steady-state subgravity to be the cause.

While vision appears to be largely responsible for orientation during the free-floating state, the importance of contact stimulation is seen in Simons' (1959) observations of walking behavior during weightlessness. As soon as a subject's feet contacted a surface, this appeared as "down," regardless of the subject's physical orientation or part of the airplane configuration contacted. The ceiling was "down" when walked upon. At the same time, weightlessness means the loss of many important cues of weight and friction. This is borne out in Rees and Copeland's (1960) study of the discrimination of differences in mass. During simulated weightlessness, to be discriminable, differences in mass had to be twice as great as those required under the usual one-g weightlifting situation.

Psychomotor Performance During Weightlessness. A possible decrement in psychomotor performance during zero g was of deep and continued concern to aeromedical scientists throughout the preorbital history of space flight. However, because of the problems of effective simulation, the literature is small and deals primarily with simple and routine tasks for human subjects. Hartman, McKenzie, and Graveline (1960) recorded vigilance responses, responses on binary matching, and performance on a complex operator task in a single subject during and after a seven-day period of water immersion. Performance on all three showed a slight decrement during immersion. However, there were striking changes in performance behavior in the post immersion period. Responses were fractionated and characterized by gross errors in spatial orientation. Whether this disintegration resulted from the change from the immersion to the normal environment or from the prolonged period of restriction during immersion is not clear. Kama

(1961) used the frictionless table to observe the speed and accuracy of positioning movements during weightlessness. Subjects made blind positioning movements by sliding two different masses different distances in particular directions. Subjects showed a tendency to undershoot target locations. Accuracy and response time varied with the distance of the target from the subject. Wade (1962) recorded response times for the operation of push-button, toggle, and rotary switches during zero-gravity trajectories. Significant decrements were observed, but again these were small. Performance was most efficient with the rotary switch and poorest with the toggle. In an earlier experiment, Gerathewohl, Strughold, and Stallings (1957) had subjects make aiming and hitting responses at the center of a bull's-eye test chart during conditions of zero g, one g, and three g's. Accuracy was slightly poorer during both weightlessness and the three-g condition. In addition, while there was a random scatter about the bull's-eye for the normal environment, responses tended to cluster in the bottom half of the target during acceleration and in the upper half during weightlessness. In a similar experiment, von Beckh (1954) had subjects draw crosses in squares on a response sheet. When this was performed with the eyes closed during weightlessness, responses tended to be oriented toward the upper right of the response sheet.

None of the observations of performance during the Mercury flights indicated any adverse effects of weightlessness upon the stationary subject. Henry et al. (1962) have reviewed the results of a number of the Mercury flights (MR-2, MR-3, MR-4 and MA-5, MA-6, and MA-7). On flight MR-2, for example, the animal subject performed a Sidman avoidance schedule quite normally during weightlessness. On flight MA-5, no major changes occurred in performance on a five-component schedule involving both continuous and discrete avoidance, operant responses to food and to water, and oddity problems. Observations of the human subject on MA-6 and MA-7 during his manual-control systems check revealed normal performance during weightlessness.

While the environment of the weightless condition constitutes a phenomenologically unusual environment, the concern earlier felt for it as a behaviorally relevant variable of major significance seems not to have been warranted. Subjects appear to adapt rapidly to it, and it seems not to create any special problem in spatial orientation. Man, as far as is discernible, is fully capable of behaviors that are characteristic of his normal (one-g) terrestrial environment. Certainly, there is no evidence that he will have difficulty as a space traveler and observer. There is, of course, the likelihood that more elaborate and extensive studies than are at present possible will reveal subtle behavior effects of both practical and theoretical significance. It is clearly apparent that interest in weightlessness has been essentially empirical. In an early theoretical paper, however, Haber and Gerathewohl (1951) speculated that the Weber-Fechner law of classical psychophysics would define the intensive aspects of experience in the subgravity field. The null value for a scale of weightless sensation would, of course, be the one g of the terrestrial environment. Gougerot (1953) took another view, contending that the g-level for null sensation would change with reduced g and thus that the organism would quickly adapt and set a new null value coincident with the prevailing g level. This latter view is consistent with adaptation level theory and contrasts with classical psychophysics. It is confirmed by all of our information to date.

VIGILANCE BEHAVIOR

Vigilance behavior is concerned with the detection of weak, infrequent, and irregularly occurring signals. In the laboratory, while the signal may be the absence of a stimulus or a slight shift in stimulus intensity, the vigilance setting most commonly used involves the presentation of a small number of near-threshold positive signals —pips on an oscilloscope screen or brief tones through a headset—randomly spaced over a period of an hour or longer. The stimulus surround is otherwise static and relatively impoverished. Vigilance functions display one or more representative characteristics. First of all, performance level varies with a wide range of variables. For example, Jerison and Wallis (1957) compared performance on one-clock and three-clock versions of the Mackworth Clock test. In this task the subject is instructed to signal whenever the hand on the clock-dial makes a double, in contrast to a single, jump. When the subject was required to monitor three clocks simultaneously, the average number of correct detections was markedly below that for the one-clock condition. Secondly, vigilance functions very often display decrement. In laboratory studies this usually appears within the first fifteen to thirty minutes. Then performance levels off. In the Jerison and Wallis study just mentioned, the performance curve for the one-clock condition dropped about 20 per cent in eighty minutes before leveling off. The curve for the three-clock condition showed no decrement. The experimenters, however, are of the opinion that a decrement occurred very early and was masked by the grouping of their data. Finally, vigilance curves may display end spurt. Thus, Bergum and Lehr (1963*c*) report significant improvement in the final period of testing for a group of subjects who knew how long the period of vigil would be. A variety of measures have been used to describe vigilance performance, but the most frequently used are the proportion of the signals correctly detected and the latency of the detection response.

The vigilance problem has had a rather long past but a short history. Current interest resulted from the practical needs of the Second World War military scientists to improve the generally poor performances of watchkeepers, and the major portion of the recent literature has emanated from military laboratories or from the laboratories of military contractors. The chief concern of these agencies has been empirical, and such theory as has been generated has been directed toward accounting for the performance efficiency of the human observer or that of the man-machine monitoring system.

The introduction of the term "vigilance" is attributable to Dr. Norman Mackworth (1950), who borrowed it from Sir Henry Head and used it to refer to the state of response readiness underlying efficient observing behavior. But the concept is related to the general concepts of attention and set, and there are interesting comparisons in the literature indexed under these several terms. For example, Baker's 1958 study of the effect of biasing attention during a vigilance task employed a paradigm used by Mowrer, Rayman, and Bliss in 1940 to study the central locus of set. Indeed, the vigilance experiment is the reaction-time experiment with certain important differences. Both deal with signal detection. The former is concerned with response efficiency over trials and an extended period of time. It typically asks about performance efficiency in the simplest possible terms: Was the signal detected or not detected? The latter is concerned with response efficiency on a single trial or a replicated single trial. Efficiency under these circumstances is most reasonably expressed by a measure of response latency. The vigilance experiment, with its origin in a practical need for maximum efficiency outside the laboratory, examines detection in a generally adverse setting—weak, near-threshold signals, occurring without cue, irregularly spaced, and at comparatively long intervals. The reaction-time experiment, concerned with representative indices of responsivity, provides for observation under optimal conditions—clearly detectable stimulus magnitudes, presented, with premonitory cue, at a constant and relatively more rapid rate and in a shorter period of time. The reaction-time experiment, popular in an earlier, response-oriented era, has been identified largely with the study of peripheral mechanisms. In contrast, the theories that have been generated in the context of the vigilance experiment have been, for the most part, central in orientation. It is thus curious that, while the importance of central variables is generally admitted, the major portion of the experimental literature on vigilance has involved the manipulation of situational variables. A cursory review of the findings of these studies follows. Meanwhile, the reader is referred to several excellent substantive reviews that have appeared recently (Bergum and Klein, 1961; Frankman and Adams, 1962; McGrath, Harabedian, and Buckner, 1959).

Vigilance and the Conditions of Stimulation. Relatively few experiments deal with the relation of primary stimulus properties to detection efficiency. When visual signals are used, performance generally is better with increases in signal intensity, in signal duration, and in signal to noise ratio (Mackworth, 1950; Adams, 1956; Jenkins, 1958). While auditory signals do not differ from visual signals in ease of detection (cf., e.g., Baker, Ware, Sipowicz, 1962*a*), Loeb and Hawkes have shown in several studies (1961, 1962) that performance is clearly better with auditory than with cutaneous inputs.

More attention has been directed to the effects upon performance of different characteristics of the input program. Generally, performance improves as the rate of signal presentation increases. For example, Deese and Ormond (1953) have described a positive correlation between rate and efficiency for a range of ten to forty simulated radar signals per hour. The relation to performance of the duration of the interval between any two successive signals is less clear. When this is defined as the time between signal presentations, no relationship is apparent (Deese and Ormond, 1953); when, however, interval is defined as the time between detections, the correlation is negative (Jenkins, 1958). In addition, McGrath and his colleagues (Harabedian, McGrath, and Buckner, 1960; McGrath, 1960; and Buckner and McGrath, 1961) have shown that the probability of detection decreases when the intervals are positively skewed and increases when the distribution is rectangular.

An interest in the patterning of stimuli as a significant factor in vigilance performance results from a concern for vigilance theory. Central to Baker's expectancy interpretation of vigilance is his report (1959*a*) of a change in reaction time with variation in the intersignal interval and his association of this variability with the vigilance decrement (1959*b*). Especially important is his demonstration (1962) that detection probability decreases as the interval preceding a test signal deviates from the mean of an adaptation series. The task was a clock test and eight adaptation signals were presented over a thirty-two-minute period. The test signal was detected most frequently after a four-minute interval, even though this interval never actually occurred in the adaptation series. While other studies have been in general agreement with Baker's findings (e.g., Klemmer, 1956; McCormack and Prysiazniuk, 1961; Adams and Boulter, 1964), others have failed to confirm a relationship between interval variability and performance (e.g., Karlin, 1959; Wilkinson, 1961; Boulter and Adams, 1963) and the basis of this conflict is at present unclear.

Of less theoretical significance but of more practical concern is duration of vigil as a determinant of performance, for the duration of a watch has probably more often been set by military tradition than by good human-factors practice. Studies with a variety of vigilance tasks indicate that the most common result is a deterioration in performance with the passage of time within sessions. Representative is the Baker, Ware, and Sipowicz (1962*c*) report that efficiency gradually declined during a twenty-four-hour watch, reaching its nadir at sixteen to eighteen hours duration. This may be counteracted by changing watchkeepers at a rate that will keep performance at the desired level. Another device involves the appropriate manipulation of expectation and knowledge of duration, for performance is better when expectation is for a short vigil (Jerison, 1958), and knowledge of the task duration induces end spurt (Bergum and Lehr, 1963*c*).

Vigilance performance during successive vigils is another matter. Although Adams and Chiles (1961) describe poorer performance for consecutive daily periods during fifteen days of confinement in the simulation of an advanced-design aerospace vehicle, a number of contemporary studies found no such consistency. For example, Adams, Humes, and Stenson (1962) report no between-days effect with nine consecutive daily sessions. Here confinement may be the differentiating condition, and the failure for decrement to appear across sessions implies the beneficial effect of rest and/or change in activity. Where rest, even for a matter of seconds, has been introduced, the result has been the maintenance or facilitation of efficiency. Thus Bergum and Lehr (1962), with ten-minute rest pauses every thirty minutes, maintained almost perfect visual detection performance with both high and low rates of input.

In the past several years some interest has been directed toward task complexity as a determinant of performance. This has prompted the use of multiple displays, input to several modalities, and the use of artificial signals. By way of example, there is Jerison and Wallis' observation (1957) that performance on the Mackworth Clock test with a single clock was clearly superior to performance with three clocks. After decrement, the former leveled off at 50 per cent correct detections, while the latter never reached more than 30 per cent. While the available studies are not uniformly in agreement, it seems fair to conclude that multimodal presentations—at least, when the signals are synchronized—facilitate detection (Loveless, 1957; Buckner and McGrath, 1961; Osborn, Sheldon, and Baker, 1963). The data on stimulus load and false signals are less clear. At any rate, it would appear that whether or not the latter influence vigilance depends upon how the subject defines the task for himself and the way in which the objective structure of the task influences that definition.

Vigilance and the Observer. It is curious that while the strong conviction exists that the significant determinants of vigilant behavior are central variables, experimental investigations most frequently have involved situational variables.

Where observer variables have received systematic attention, most commonly these have been examined by means of the manipulation of situational variables. Here reference, of course, is to the influence of reinforcement, knowledge of results, set, etc., upon vigilance. A small number of studies concerning social and personality determinants of vigilance and the character of individual differences now have begun to make their appearance in the literature.

Reasoning from the assumption that the detection event is reinforcing, Deese (1957) predicted and confirmed that performance is more efficient for higher than for lower signal rates. Going farther, Holland (1958) identified observing responses (orienting and other behaviors prerequisite to detection), as the reinforced events. Using the paradigm of operant conditioning, he arranged a situation in which the subject pressed a key to illuminate the task display briefly and obtained a decrease in observing responses following detection and an increased rate of key pressing. However, a subsequent study of Baker (1960), which included photographs of eye fixations on the display as observing responses occurred, indicated that where decrement did not occur, the rate of nonobserving behavior and general activity increased over time, and Jerison and Wing (1961) have suggested that neither Baker's eye fixations nor Holland's observing responses are true observing responses. They further have contended that Holland's results are artifacts of procedure, resulting from the fact that detection required the simultaneous occurrence of signal and illumination responses.

Vigilance performance is facilitated by reinforcement. Monetary reward for correct detections and withdrawal of reward, blast of a truck horn, and electric shock for errors have all been shown to improve performance (Pollack and Knaff, 1958; Bergum and Lehr, 1963*a*; Bevan and Turner, 1965). Recent experiments in the present writer's laboratory (Bevan and Turner, 1965 and 1966), have been concerned with the effect of a qualitative shift in reinforcement upon detection. They have indicated that this procedure is more effective than the use of either reward or punishment alone and, when the shift is unanticipated by the subject, a qualitative contrast effect, similar to that induced by a shift in reinforcement magnitude, is in evidence.

An interest in knowledge of results in relation to vigilance dates from Mackworth's 1950 work, but it has been intensified during the past few years. Performance with knowledge of results, even partial (e.g., Bergum and Lehr, 1963*a*) or false (e.g., Weidenfeller, Baker, and Ware, 1962) knowledge of results, is clearly better than when knowledge of results is not supplied. It is more effective when given verbally by the experimenter than when supplied by means of a system of signal lights. Hardesty, Trumbo, and Bevan (1963), found this to be the case even when the experimenter was not physically present but delivered the information over an intercom system. The verbally presented knowledge of results is probably more effective if provided in the form of evaluative comments than as quantitative information (Hardesty and Bevan, 1964). Conceptually related to the problem of knowledge of results is an interest in the influence of set upon performance efficiency and it is clear that when the subjects may anticipate with reasonable certainty when and/or where a signal will occur, detection efficiency is of a high order and immune to decrement (Baker and Harabedian, 1962; Jane Mackworth, 1963).

Individual differences in vigilance are in evidence in a large number of studies, but data are largely unsystematic and provide little in the way of a clear picture of the dimensions of these phenomena. There is evidence of a relationship between performance and general intelligence (Kappauf and Powe, 1959), general activity level (Baker, 1959*b*) and introversion-extraversion (Bakan, 1959), and it seems reasonable to expect that a classification of the nature of individual differences will carry us a long way toward an understanding of the inconsistencies that are everywhere apparent in the vigilance literature.

Recent attention has been directed toward the enhancement of effectiveness through the use of multiple monitors. Some data suggest that the effect of adding monitors is to increase the num- of signals detected (Schafer, 1949; Baker, Ware, and Sipowicz, 1962*b*). The mechanism of enhancement requires clarification. Meanwhile, the study of multiple observer systems has obvious economic and manpower implications for agencies, military and otherwise, that employ large warning networks.

Theories of Vigilance Behavior. Theories of vigilance to date have been broadly drawn thematic statements. They show, with psychological theories in other areas, a lack of articulation in formal structure that marks theories in the more highly developed sciences. Vigilance theories have been concerned, for the most part, with the nature of vigilance mechanisms and, more recently, with the energetics of vigilance. As has been the case with most psychological theory making, one or the other of two models has been employed: the *S-R* model here identified with conditioning theory and the cognitive model here employed in expectancy theory. Theoretical formulations have been elaborated to account for the action of specific variables upon performance efficiency. Given a systematic orientation, much energy has been expended by its proponents on identifying the systematic weaknesses of the alternative approach.

Mackworth (1950), in the first attempt at systematization, described vigilance in terms of Pavlovian conditioning. His main purpose was to account for performance decrement on the Clock test. This he identified with the growth of internal inhibition. The rapid recovery of performance following task interruptions he explained as disinhibition. The fact that nothing approaching total extinction occurs for the vigilance response forced the additional assumption that expectancy and self-instructions supplement the reinforcing function of the UCS. A major weakness of Mackworth's formulation is that it leads to the prediction that high signal rates are associated with greater performance decrement than low signal rates, a relationship denied by empirical data. Further, it is doubtful that the occurrence of task interruption in Mackworth's setting could qualify as a disinhibitor.

Another application of conditioning is Broadbent's (1953) explanation of vigilance in terms of attention. According to this view the monitor is a stimulus-selection system, stimuli commanding responses as a function of their physical intensity, biological importance, and/or novelty. Decrement is attributed to reduced novelty. Recovery following stimulus interpolation is due to the same reason. The failure for complex tasks to display decrement is explained by shifts in stimulus priority and the maintenance of novelty. This, however, does not explain the lower level of performance associated with complex tasks and, as Frankman and Adams (1962) argue, many of his explanations of specific data appear as ad hoc explanations rather than as deductions from the original formulation.

McCormack (1962) has dealt with vigilance in terms of response latency data and has proposed an inhibition-motivation theory to account for it. His specific concern is to account for the decrement in performance associated with the withholding of knowledge of results. To achieve this he assumes the operation of three intervening variables: inhibition (Spencian rather than Pavlovian) that develops in the absence of reinforcement (*In*), drive level (*D*), and constitutional factors (*C*). Reaction time is a function of a difference between the strength of excitatory processes associated with *C* and *D* and the strength of the inhibitory process. The fact that reaction time is an increasing function of time on task with no knowledge of results is explained by the development of inhibition. The reduction of reaction time following rest (the interpolation of extraneous stimulation) is identified with the dissipation of inhibition, and shorter reaction times with fixed-interval stimulation are explained by an assumed elevation of drive level with regularity of presentation.

Bergum and Klein (1961), dissatisfied with Pavlovian conditioning theory because of its failure to deal with motivational variables, like McCormack have turned to Spencian learning theory. Vigilance behavior is viewed as consisting of chains of serially dependent *rg*'s (fractional responses) and *R*'s (responses), with each serving as the stimulant for the next response. Nonreinforced occurrences of any response would tend to inhibit the *rg*'s with an ensuing reduction of the probability of *R*'s or a generalization of inhibition from *rg*'s to *R*'s. Furthermore, amount and frequency of reinforcement may directly affect performance level.

The cognitive approach is represented by the expectancy theories of Deese (1955) and Baker (1959*a*). While they are scarcely more adequately articulated than the applications of conditioning theory, they have had the heuristic value of having generated a number of interesting experiments. Deese's formulation was prompted by the problem of relating the temporal distribution of signals to detection performance. Deese recognizes two classes of variables in the determination of vigilance performance, motivational level and expectancy, and he directs his attention to the operation of the latter. Expectancy derives from the course of events that constitutes the subject's past experience with the task. It is reasoned that expectancy, and thus detection probability, should be higher for faster signal rates. This is borne out by the literature. Meanwhile, Deese's expectancy concept provides little guidance in explaining decrement. Recourse must be had to motivational determinants, for otherwise one would expect that after the pattern of stimulation becomes clear to the observer, expectancy should be stable throughout. Regular schedules should yield higher levels of expectancy and thus better performance than variable schedules. This is supported by Hardesty's (1964) report that response latencies are shorter for regular than for variable schedules, although in both instances response time increases as the presentation interval preceding the signal differs from the *mean* presentation interval. Deese predicts that detection probability will be low when the intersignal interval is less than the average for the series, but that it will be equal to or exceed the average probability for the series when the interval equals or exceeds the average duration. The empirical literature on intersignal interval is conflicting on this point, and Harabedian, McGrath, and Buckner (1960) have demonstrated that the relationship of these variables depends upon the way the intersignal interval is defined. Meanwhile, Baker has modified this prediction to maintain that performance efficiency is maximal for the mean interval and decreases as the interval deviates from this value. He (1962) has confirmed this for probability of detection, and Hardesty (1964) has obtained similar results for response latency.

Baker (1959*a*) has expanded Deese's view to deal formally with five classes of variables: detection probability (1) varies directly with average signal rate; (2) is enhanced by the regularity of the intersignal interval; (3) is facilitated by knowledge of results, because this variable aids in the observer's perception of the structure of the stimulus sequence; (4) improves with knowledge of the signal's location since this aids in confirmation of the signal's occurrence; and (5) is positively related to the intensity of the signal. Data are available to support each of these predictions. However, no explanation is provided for decrement over time, a common—if not inevitable—characteristic of vigilance functions. Nor does it accommodate other factors like rest, interpolated stimuli, etc., that conditioning theory explicitly covers. Bergum and Klein (1961) offer the further criticism that, since expectancy is related to the mean rate of presentation, the prediction of perfect performance is impossible. This overlooks the case of the constant interval and ignores the possibility of a range of equivalence about the series mean. It is conceivable that there are variable interval or rate schedules that are perceptually and behaviorally equivalent to constant interval or rate schedules.

The activation theory of vigilance is concerned with its energetics. It is also identified with Deese (1955) and more recently with Scott (1957). It maintains that vigilance reflects the general excitation level of the observer. This last in turn is intimately a function of the degree of variation in sensory input. Invariant input leads to adaptation and a reduction in vigilance. Carried to an extreme the result is sleep. His view receives neurophysiological support from Hebb's theorizing about the nature of central nervous system organization (1955*a*, *b*), theorizing about the studies of EEG, the reticular activation system, and the literature of sensory deprivation on which it is based. It is Deese's answer to the problem of decrement. It also accounts for the absence of decrement in the literature on multiple-task monitoring following rest and with the introduction of irrelevant stimuli. Recently, attention has turned to experimental tests of the activationist hypothesis. Adams and Boulter (1962) induced head and eye movements during monitoring, but they found no reduction in vigilance decrement. However, Kirk and Hecht (1963) have reported performance facilitation in the presence of a variable-intensity ambient noise. In contrast, Bakan and Manley (1963) have described better performance on an auditory monitoring task by blindfolded subjects than by controls with normal opportunities for vision. And Alluisi and Hall (1963) take as evidence against the activation hypothesis the appearance of the vigilance decrement, even though their subjects were simultaneously engaged in a total of six tasks.

It is the present writer's view that the current literature on vigilance can be conceptually accommodated by an extension of Helson's adaptation-level (AL) theory to this problem area. Bevan and Adamson have already performed such an exercise for the concept of reinforcement (Bevan and Adamson, 1963; Bevan, 1963), and the AL treatment of vigilance resembles closely that accorded reinforcement. Such an approach has three outstanding advantages. First, it has the psychological value of being metaphysically neutral—it bypasses the emotional bias frequently aroused by the opposition of the *S-R* and cognitive points of view. Secondly, it provides a basis for the quantitative description of the vigilance response. Already, AL theory has been an effective device for the quantitative prediction of sensory judgments and, as Woodworth and Schlosberg (1954) have already indicated, it is a simple logical step from sensory measures to response latencies. Finally, it is comprehensive, explaining with a single functional principle both the directive and energetic aspects of vigilance and identifying both the role of situational and of personal determinants of performance efficiency.

According to this view, vigilance is a response process defined by the measures that indicate detection efficiency. Like the processes underlying sensory judgment and reinforcement, it is continuous over time, susceptible to change from moment to moment, but best estimated in terms of a normative value, derived from an integration of relevant stimulus input (focal stimuli), situational variables (background), and personal determinants (residual stimulation). It is possible then to conceive of a vigilance AL in the same terms as a reinforcement AL or an AL for sensory judgment. Furthermore, vigilance must be viewed as the product of both directive and dynamic processes. Each set of processes is assumed to be continuous in character and identified by a pervasive level that reflects the pooling of the three classes of variables just mentioned.

The arousal processes are energetic or motivational in nature. They are assumed to stand in a curvilinear relationship to performance efficiency. When arousal level is low, efficiency is poor. When arousal level is high, efficiency is also poor. Maximum efficiency is identified with some intermediate arousal level. There may, of course, be qualitative differences in the performance associated with different arousal levels: low arousal may be identified by a high proportion of missed signals and few false starts; high arousal, in contrast, may be associated with a high percentage of commissive errors, relatively fewer omissive errors.

Arousal is viewed as the product of some combination of focal, background, and residual stimulation. In the last category are constitutional determinants: some organisms are marked by a continuing high level of arousal; others generally by a low one. This is the contrast between the bassett hound and the saluki.

The background component derives from the immediate situation. This is the main concern of the activation theory described above. Here the literature on nonspecific stimulation and the reticular system as it relates to vigilance is relevant. So also is the literature on sleep and waking, on the effects of sensory deprivation and of increased input to the sensorium. It is assumed that the relation between level of sensory input and arousal level is monotonic, and this appears to be confirmed by some of the experiments prompted by the activation hypothesis. Since, however, arousal is assumed to conform to the AL principle, what is important for the establishment and maintenance of arousal level is variation over time in the level of sensory input, rather than the persistence of some particular physical magnitude of stimulation, for when arousal level matches input level, adaptation may be assumed to be perfect and arousal minimal.

Focal stimulation, if of sufficient magnitude, should enhance arousal. Accordingly, arousal level is expected to be higher with higher rates of stimulation and more intense stimulation. If silent periods between successive stimuli are assumed to be integrated into the arousal AL as zero values, lengthening the interval between signals should result in reduced arousal. However, the effect of individual signals in enhancing arousal should be increased. At the same time, as just noted, because the arousal AL regresses to the mean level of stimulation, signals at the mean intensity level become increasingly less effective in maintaining arousal. Stimulation is thus a self-depreciating process. Assuming that the organism has a characteristic period of integration, whether or not there is regression of the general arousal level toward zero will depend upon the potency of the signal and the average signal rate. For a given intensity and duration of signal, signal rates higher than the period of integration will result in loss of arousal and those slower will make for the preservation of a representative arousal level, depending upon intersignal interval and stimulus potency. Since arousal is defined in terms of mean levels, shifts in level will lag behind momentary changes induced by individual signals. Finally, the relation between rate and intensity of stimulation is assumed to be reciprocal. A high rate of weaker signals may be expected to exert the same influence upon arousal level as a lower rate of intense stimuli.

Expectancy here refers to the strength of the observer's estimate of the likelihood that a signal will occur under a given set of conditions. It is assumed to stand in a direct monotonic relationship to performance efficiency. As an intervening process it shares with arousal many of the same formal properties. It is conceived to be continuous over time. It reflects the influence of the same broad classes of variable: residual, background, and focal stimulation. Ordinarily, it may be expected that residual stimulation constitutes a class of determinants of major importance, representing the patterns of past experience. Specific backgrounds are assumed to be identified with particular levels of expectancy. In the evolution of expectancy AL's, as certain studies stimulated by the expectancy theorists have already shown, the important characteristics of the focal stimuli are relative regularity of their occurrence and the permanence of their spatial locality. If intersignal intervals are constant over a series of signals, expectation, after the minimal number of trials needed for the generation of the expectancy AL, will approach certainty that the signal will reoccur following the estimated duration of the fixed interval. If the rate of presentation is randomly variable, expectation is that a signal will reoccur after the *mean* interval duration. The strength of expectation will vary inversely with the extent of the variability among the intersignal interval durations. If the distribution is rectangular, expectation for any given interval will approach zero. For any expected duration of interval, expectation, as Baker (1959*a*) has already assumed, may be expected to approach its maximal value, and be lowest immediately following the occurrence of the signal on a given presentation. The expectation of the locus of the signal is assumed to have properties similar to the expectation of its temporal occurrence. Expectancy ALs are assumed to maximize at subjective certainty or near certainty, although this may represent a wide range of objective probabilities. The possibility that expectancy may exceed psychological certainty (what provokes a judge to ignore persistently the negative cases?) provides for some interesting speculation.

Arousal would appear logically to stand in a curvilinear relationship to expectancy. Given a specific arousal level, both low expectancy and certainty will be associated with minimal increases in arousal. Maximal enhancement of arousal is identified with ambiguous expectations. The persistence of either low expectancy or certainty will be accompanied by a regression of the AL toward some minimal value.

The experimental problems that relate to vigilance involve more than the establishment of empirical correlations between sensory input and performance efficiency. They involve working out the matrix of relationships involving input, intervening processes, and performance, including the conceptually fascinating relationships of the intervening processes to each other.

SUMMARY

This chapter has dealt with behavior in unusual environments. It has concerned itself with the properties of environments, both substantive and formal, that have characterized the psychologist's use of the term in his theoretical formulations and in his investigational practice. It has also concerned itself with the empirical relationships that exist between behavior and situations that have been of special interest as unusual environments during the past decade or so. A look back at the ground covered forces the impression that the bipartite organization of this discussion was forced by the discontinuity in the state of affairs in psychology as far as the concept of environment is concerned. While environment as a concept may have achieved some degree of formal articulation, it at the same time lacks considerably in systematic versatility, and there are few clearly established bridges between its place in the corpus of psychological theories and in the empirical practices of the psychological investigator. The environment of the laboratory or field setting is rather much the presystematic environment of common sense. But the observation of behavior in the unusual setting has in recent years provoked insights of fundamental significance for the psychological systematist, and one cannot help but feel sanguine about the value of focusing attention on environment as a variable both in the concrete context of the observational setting and also, when sufficiently well defined as a systematic concept, within the domain of the psychological theorist. Indeed, it is hoped that the study of behavior in the unusual environment will not only yield important facts and principles about the nature of behavior but also lead to a theoretically more sophisticated concept of environment.

REFERENCES

Adams, D. K. Learning and explanation. In *Learning Theory, Personality Theory, and Clinical Research; The Kentucky Symposium.* New York: John Wiley & Sons, Inc., 1954, 66-80.

Adams, J. A. Vigilance in the detection of low-intensity visual stimuli. *J. exp. Psychol.,* 1956, **52**, 204-208.

———, and Boulter, L. R. An evaluation of the activationist hypothesis of human vigilance. *J. exp. Psychol.,* 1962, **64**, 495-504.

———, and ———. Spatial and temporal uncertainty as determinants of vigilance behavior. *J. exp. Psychol.,* 1964, **67**, 127-131.

———, Humes, J. M., and Stenson, H. H. Monitoring of complex visual displays: III. Effects of repeated sessions on human vigilance. *Human Factors,* 1962, **4**, 149-158.

Adams, O. S. and Chiles, W. D. Human performance as a function of the work-rest ratio during prolonged confinement. Wright-Patterson AFB, Ohio, *ASD tech. Rep.,* 61-720, 1961.

Alluisi, E. A. and Hall, T. J. Declines in auditory vigilance during periods of high multiple-task activity. *Percept. mot. Skills,* 1963, **16**, 739-740.

Arnhoff, F. N., Leon, H. V. and Brownfield, C. A. Sensory deprivation: its effects on human learning. *Science,* 1962, **138**, 899-900.

Arnoult, M. D. Perceptual problems of a space traveler. Paper presented at So. Soc. Phil. and Psychol. meeting, 1959.

Bakan, P. Extroversion-introversion and improvement in an auditory vigilance task. *Brit. J. Psychol.,* 1959, **50**, 325-332.

———, and Manley, R. Effect of visual deprivation on auditory vigilance. *Brit. J. Psychol.,* 1963, **54**, 115-119.

Baker, C. H. Attention to visual displays during a vigilance task: I. Biasing attention. *Brit. J. Psychol.,* 1958, **49**, 279-288.

———. Toward a theory of vigilance. *Canad. J. Psychol.,* 1959*a*, 13, 35-41.

———. Attention to visual displays during a vigilance task: II. Maintaining the level of vigilance. *Brit. J. Psychol.,* 1959*b*, **50**, 30-36.

———. Observing behavior in a vigilance task. *Science,* 1960, **132**, 674-675.

———. Probability of signal detection in a vigilance task. *Science,* 1962, **136**, 46-47.

———, and Harabedian, A. A study of target detection by sonar operators. Human Factors Research, Inc., Los Angeles, 1962.

Baker, R. A., Ware, J. R. and Sipowicz, R. R. Vigilance: a comparison in auditory, visual and combined audio-visual tasks. *Canad. J. Psychol.,* 1962*a*, **16**, 192-198.

———, ———, and ———. Signal detection by multiple monitors. *Psychol. Rec.,* 1962*b*, **12**, 133-137.

———, ———, and ———. Sustained vigilance I—signal detection during a 24-hour continuous watch. *Psychol. Rec.,* 1962*c*, **12**, 245-250.

Barker, R. G. On the nature of the environment. *J. soc. Issues,* 1964, **19**, 17-38.

Bergmann, G. The contribution of John B. Watson. In Scher, J. (ed.), *Theories of the Mind.* Glencoe, Ill.: Free Press, 1962, 674-688.

Bergum, B. O. and Klein, I. C. A survey and analysis of vigilance research. HumRRO res. Rep., No. 8, 1961.

———, and Lehr, D. J. Vigilance performance as a function of interpolated rest. *J. appl. Psychol.,* 1962, **46**, 425-427.

———, and ———. Vigilance performance as a function of task and environmental variables. HumRRO res. Rep., No. 11, 1963*a*.

———, and ———. Effects of authoritarianism on vigilance performance. *J. appl. Psychol.,* 1963*b*, **47**, 75-77.

———, and ———. Endspurt in vigilance. *J. exp. Psychol.,* 1963*c*, **66**, 383-385.

Berlyne, D. E. *Conflict, Arousal, and Curiosity.* New York: McGraw-Hill Book Company, Inc., 1960.

Bevan, W. Sound-precipitated convulsions: 1947-1954. *Psychol. Bull.,* 1955, **52**, 473-504.

———. Extant perceptual theories and the perceptual world of space flight. In Whitcomb, M. A. (ed.), *Visual Problems of the Armed Forces.* Washington: NAS-NRC, 1962, 66-75.

———. The pooling mechanism and the phenomenon of reinforcement. In Harvey, O. J. (ed.), *Motivation and Social Interaction.* New York: The Ronald Press Company, 1963, 18-34.

———, and Adamson, R. Internal referents and the concept of reinforcement. In Washburne, N. F. (ed.), *Decisions, Values, and Groups.* Vol. II. New York: Pergamon Press, 1963, 453-472.

———, and Turner, E. D. Vigilance performance with a qualitative shift in reinforcers. *J. exp. Psychol.,* 1965, **70**, 83-86.

———, and ———. Vigilance performance with a qualitative shift in verbal reinforcement. *J. exp. Psychol.,* 1966, **71**, 467-468.

Bexton, W. H., Heron, W., and Scott, T. H. Effects of decreased variation in the sensory environment. *Canad. J. Psychol.*, 1954, **8**, 70-76.

BIDERMAN, A. D. AND ZIMMER, H. (eds.). *The Manipulation of Human Behavior.* New York: John Wiley & Sons, Inc., 1961.

BOULTER, L. R. AND ADAMS, J. A. Vigilance decrement, the expectancy hypothesis, and intersignal interval. *Canad. J. Psychol.*, 1963, **17**, 201-209.

BROADBENT, D. E. Classical conditioning and human watch-keeping. *Psychol. Rev.*, 1953, **60**, 331-339.

BROWN, J. F. The visual perception of velocity. *Psychol. Forsch.*, 1931, **14**, 199-232.

BROWN, J. L. (ed.). *Sensory and Perceptual Problems Related to Space Flight.* Washington: NAS-NRC Pub. 872, 1961*a*.

———. Orientation to the vertical during water immersion. *Aerospace Med.*, 1961*b*, **32**, 209-217.

BROWN, R. H. "Empty-field" myopia and visibility of distant objects at high altitudes. *Amer. J. Psychol.*, 1957, **70**, 376-385.

BRUNER, J. S. Personality dynamics and the process of perceiving. In Blake, R. R., and Ramsey, G. V. (eds.), *Perception, an Approach to Personality,* New York: The Ronald Press Company, 1951, 121-247.

———. Cognitive consequences of early sensory deprivation. In Solomon, P. et al. (eds.), *Sensory Deprivation.* Cambridge, Mass.: Harvard University Press, 1961, 195-207.

———, AND KRECH, D. (eds.). *Perception and Personality.* Durham, N.C.: Duke University Press, 1950.

BRUNSWIK, E. *Perception and the Representative Design of Psychological Experiments.* Berkeley, Calif.: University of California Press, 1956.

BUCKNER, D. N. AND MCGRATH, J. J. A comparison of performance on single and dual sensory mode vigilance tasks. Tech. Rep. No. 8, Human Factors Research, Los Angeles, 1961.

BURNS, N. M., CHAMBERS, R. M. AND HENDLER, E. (eds.). *Unusual Environments and Human Behavior.* New York: The Free Press of Glencoe, 1963.

———, AND KIMURA, D. Isolation and sensory deprivation. In Burns, N. M., Chambers, R. M., and Hendler, E. (eds.), *Unusual Environments and Human Behavior.* New York: The Free Press of Glencoe, 1963, 167-192.

CAMBERARI, J. D. The effects of sensory isolation on suggestible and non-suggestible psychology graduate students. Unpub. Ph.D. dissertation, University of Utah, 1958.

CANNON, W. B. *Wisdom of the Body.* New York: W. W. Norton & Company, Inc., 1932.

CARNAP, R. The methodological character of theoretical concepts. In Feigl, H., and Scriven, M., *Minnesota Studies in the Philosophy of Science,* Vol. 1, Minneapolis: University of Minnesota Press, 1956, 70-71.

CHAMBERS, R. M. Operator performance in acceleration environments. In Burns, N. M., Chambers, R. M., and Hendler, E. (eds.), *Unusual Environments and Human Behavior.* New York: The Free Press of Glencoe, 1963, 193-319.

CHEIN, I. The environment as a determinant of behavior, *J. soc. Psychol.*, 1954, **39**, 115-127.

COHEN, W. Spatial and textural characteristics of the Ganzfeld, *Amer. J. Psychol.*, 1957, **70**, 403-410.

———. Color perception in the chromatic Ganzfeld. *Amer. J. Psychol.*, 1958*a*, **71**, 390-394.

———. Apparent movement of simple figures in the Ganzfeld. *Percept. mot. Skills,* 1958*b*, **8**, 32.

———. Form recognition, spatial orientation, perception of movement in the uniform visual field. In Morris, A. and Horne, E. P. (eds.), *Visual Search Techniques.* Washington: NAS-NRC Pub. 712, 1960, 119-123.

COUTU, W. *Emergent Human Nature.* New York: Alfred A. Knopf, Inc., 1949.

DEESE, J. Some problems in the theory of vigilance. *Psychol. Rev.*, 1955, **62**, 359-368.

———. Changes in visual performance after visual work. *WADC tech. Rep. 57-285*, 1957.

———, AND ORMOND, E. Studies of detectability during continuous visual search. *WADC tech. Rep. 53-8*, 1953.

DEWEY, J., AND BENTLEY, A. F. *Knowing and the Known.* Boston: The Beacon Press, 1949.

DOANE, B. K., MAHATOO, W., HERON, W., AND SCOTT, T. H. Changes in perceptual function after isolation. *Canad. J. Psychol.*, 1959, **13**, 210-219.

FISKE, D. W. Effects of monotonous and restricted stimulation. In Fiske, D. W., and Maddi, S. R. *Functions of varied experience.* Homewood, Ill.: Dorsey Press, 1961, 106-144.

———, AND MADDI, S. R. *Functions of Varied Experience.* Homewood, Ill.: Dorsey Press, 1961.

FLAHERTY, B. E., FLINN, D. E., HAUTY, G. T., AND STEINKAMP, G. R. Psychiatry and space flight. *USAF Sch. Aviat. Med.* No. 60-80, 1960.

FLICKINGER, D. Biomedical aspects of space flight. In Gantz, K. (ed.), *Man in Space.* New York: Duell, Sloan & Pearce, Inc., 1959, 41-62.

FRANKMANN, JUDITH P., AND ADAMS, J. A. Theories of vigilance. *Psychol. Bull.*, 1962, **59**, 257-272.

Freedman, S. J. Perceptual changes in sensory deprivation: suggestions for a conative theory. *J. nerv. ment. Dis.*, 1961, **132**, 17-21.

———, Grunebaum, H. U. and Greenblatt, M. Perceptual and cognitive changes in sensory deprivation. In Solomon, P., et al. (eds.), *Sensory Deprivation.* Cambridge, Mass.: Harvard University Press, 1961, 58-71.

———, and Held, R. Sensory deprivation and perceptual lag. *Percept. mot. Skills,* 1960, **11**, 277-280.

Freeman, G. L. *The Energetics of Human Behavior.* Ithaca, N.Y.: Cornell University Press, 1948.

Garfinkel, H. A conception of, and experiments with, "trust" as a condition of stable concerted activities. In Harvey, O. J. (ed), *Motivation and Social Interaction.* New York: The Ronald Press Company, 1963, 187-238.

Gazenko, O. G. and Yadovsky, V. I. Some results of physiological reactions to space flight conditions. Paper read at American Rocket Society meeting, December, 1961.

Geldard, F. A. *Fundamentals of Psychology.* New York: John Wiley & Sons, Inc., 1962.

Gerathewohl, S. J. Physics and psychophysics of weightlessness visual perception. *J. aviat. Med.*, 1952, **23**, 373-395.

———. Personal experiences during short periods of weightlessness reported by sixteen subjects. *Astronautica Acta,* 1956, **2**, 203-217.

———. Weightlessness. In Gantz, K. (ed.), *Man in Space.* New York: Duell, Sloan & Pearce, Inc., 1959, 108-132.

———. *Zero-g Devices and Weightlessness Simulators.* Washington: NAS-NRC Pub. 781, 1961.

———, and Stallings, H. D. The labyrinthine posture reflex (righting reflex) in the cat during weightlessness. *J. aviat. Med.*, 1957, **28**, 345-355.

———, and ———. Experiments during weightlessness: A study of the oculo-agravic illusion. *J. aviat. Med.*, 1958, **29**, 504-515.

———, Strughold, H., and Stallings, H. D. Senso-motor performance during weightlessness. *J. aviat. Med.*, 1957, **28**, 7-12.

Gibson, J. J. *Perception of the Visual World.* Boston: Houghton Mifflin Company, 1950.

———. Perception as a function of stimulation. In Koch, S. (ed.), *Psychology: A study of a Science, Vol. 1,* New York: McGraw-Hill Book Company, Inc., 1959, 456-501.

———, and Waddell, D. Homogeneous retinal stimulation and visual perception. *Amer. J. Psychol.*, 1952, **65**, 263-270.

Glenn, J. R., Jr. Pilot's flight report. In *Results of the First U.S. Manned Orbital Space Flight,* February 20, 1962, Wash.: U.S. Gov't. Print. Off., 1962, 119-138.

Goldberger, L., and Holt, R. R. Experimental interferences with reality contact (perceptual isolation): method and group results. *J. nerv. ment. Dis.*, 1958, **127**, 99-112.

———, and ———. Experimental interference with reality contact: individual differences. In Solomon, P., et al. (eds.), *Sensory Deprivation.* Cambridge, Mass.: Harvard University Press, 1961, 130-142.

Goldfried, M. R. A psychoanalytic interpretation of sensory deprivation. *Psychol. Rec.*, 1960, **10**, 211-214.

Gougerot, L. Lois de Weber-Fechner et variations de la pesanteur apparente. *Le Medicine Aeronautique*, 1953, **8**, 119.

Graybiel, A. The oculogravic illusion. *Arch. Opthal.*, 1952, **48**, 605-615.

Gunderson, E. K. E., and Nelson, P. D. Adaptation of small groups to extreme environments. *Aerospace Med.*, 1963, **34**, 1111-1115.

Haber, H., and Gerathewohl, S. J. On the physics and psychophysics of weightlessness. *J. aviat. Med.*, 1951, **22**, 180-189.

Hammer, Lois R. Perception of the visual vertical under reduced gravity. *MRL-TDR* 62-65, 1962.

Harabedian, A., McGrath, J. J. and Buckner, D. N. The probability of signal detection in a vigilance task as a function of inter-signal interval. *Tech. Rep.* No. 3. Human Factors Research, Los Angeles, 1960.

Hardesty, D. Reaction time as a function of the spatial and temporal pattern of stimulation. Unpub. Ph.D. dissertation, Kansas State University, 1964.

———, and Bevan, W. Forms of orally-presented knowledge of results and serial reaction time. *Psychol. Rec.*, 1964, **14**, 445-448.

———, Trumbo, D., and Bevan, W. Influence of knowledge of results on performance in a monitoring task. *Percept. mot. Skills*, 1963, **16**, 629-634.

Hartman, B., McKenzie, R. E., and Graveline, D. E. An exploratory study of changes in proficiency in a hypodynamic environment. *USAF Sch. aviat. Med. tech. Rep.* 60-72, 1960.

Hebb, D. O. *The Organization of Behavior.* New York: John Wiley & Sons, Inc., 1949.

———. The mammal and his environment. *Amer. J. Psychiat.*, 1955*a*, **111**, 826-836.

———. Drives and the C.N.S. (conceptual nervous system). *Psychol. Rev.*, 1955*b*, **62**, 243-254.

———. The American revolution. *Amer. Psychologist*, 1960, **15**, 735-745.

———. Discussion. *J. nerv. ment. Dis.*, 1961, **132**, 40-43.

Held, R. Exposure-history as a factor in maintaining stability of perception and coordination. *J. nerv. ment. Dis.*, 1961, **132**, 26-32.

———, and White, B. Sensory deprivation and visual speed: an analysis. *Science*, 1959, **130**, 860-861.

Helson, H. *Adaptation-level Theory.* New York: Harper and Row, Publishers, 1964.

Henry, J. P., Augerson, W. S., Belleville, R. E., Douglas, W. K., Grunzke, M. K., Johnston, R. S., Laughlin, P. C., Mosely, J. D., Rohles, F. H., Voas, R. B., and White, S. C. Effects of weightlessness in ballistic and orbital flight. *Aerospace Med.*, 1962, **33**, 1056-1068.

———, Baillinger, E. R., Maher, P. H., and Simons, D. G. Animal studies of the subgravity state during rocket flight. *J. aviat. Med.*, 1952, **23**, 421-432.

Heron, W. The pathology of boredom. *Sci. Amer.*, 1957, **196**, 52-56.

———, Doane, B. K., and Scott, T. H. Visual disturbances after prolonged perceptual isolation. *Canad. J. Psychol.*, 1956, **10**, 13-18.

Hilgard, E. R. *Introduction to Psychology*, 3rd ed. New York: Harcourt, Brace, and World, Inc., 1962.

Hochberg, J. E., Triebel, W., and Seaman, G. Color adaptation under conditions of homogeneous visual stimulation (Ganzfeld). *J. exp. Psychol.*, 1951, **41**, 153-159.

Holland, J. G. Human vigilance. *Science*, 1958, **128**, 61-63.

Holt, E. B. *Animal Drive and the Learning Process.* New York: Henry Holt and Company, Inc., 1931.

Holt, R. R., and Goldberger, L. Personological correlates of reactions to perceptual isolation. *WADC tech Rep. 59-735*, 1959.

———, and ———. Research on the effects of isolation on cognitive functioning. *WADD tech. Rep. 60-260*, 1960.

Hull, C. L. *Principles of Behavior.* New York: D. Appleton-Century Company, Inc., 1943.

Ittelson, W. H. The constancies in perceptual theory. *Psychol. Rev.*, 1951, **58**, 285-294.

———. *Visual Space Perception.* New York: Springer, 1960.

———, and Ames, A., Jr. Accommodation, convergence and apparent distance. *J. Psychol.*, 1950, **30**, 43-62.

Jackson, C. W., Jr., and Kelly, E. L. Influence of suggestion and subject's prior knowledge in research on sensory deprivation. *Science*, 1962, **132**, 211-212.

———, and Pollard, J. C. Sensory deprivation and suggestion: a theoretical approach. *Behav. Sci.*, 1962, **7**, 332-342.

———, and ———. Some non-deprivation variables which influence the "effects" of experimental sensory deprivation. Paper read at Midwest Psychol. Assoc. Meeting, 1963.

Jenkins, H. M. The effect of signal-rate on performance in visual monitoring. *Amer. J. Psychol.*, 1958, **71** 647-661.

Jerison, H. J. Experiments on vigilance: duration of vigil and the decrement function. *WADC tech Rep. 58-369*, 1958.

———, and Wallis, R. A. Experiments on vigilance. II. One-clock and three-clock monitoring. *WADC tech. Rep. 57-206*, 1957.

———, and Wing, J. F. Human vigilance and operant behavior. *Science*, 1961, **133**, 880-881.

Kama, W. N. Effects of simulated weightlessness upon positioning responses. *ASD tech. Rep. 61-555*, 1961.

Kandel, E. J., Myers, T. I., and Murphy, D. B. Influence of prior verbalization and instructions on visual sensations reported under conditions of reduced sensory input. *Amer. Psychologist*, 1958, **13**, 334.

Kappauf, W. E., and Powe, W. E. Performance decrement on an audio-visual checking task. *J. exp. Psychol.*, 1959, **57**, 49-56.

Karlin, L. Reaction time as a function of foreperiod duration and variability. *J. exp. Psychol.*, 1959, **58**, 185-191.

King, B. G., and Wade, J. E. Vestibular functions. In Hammer, L. (ed.), *Aeronautical Systems Division Studies in Weightlessness, 1959-60. WADD tech. Rep. 60-715*, 1961, 35-36.

Kirk, R. E., and Hecht, Elizabeth. Maintenance of vigilance by programmed noise. *Percept. Mot. Skills*, 1963, **16**, 553-560.

Klemmer, E. T. Time uncertainty in simple reaction time. *J. exp. Psychol.*, 1956, **51**, 179-184.

Koffka, K. *Principles of Gestalt Psychology.* New York: Harcourt, Brace and Company, Inc., 1935.

Köhler, W. *Gestalt Psychology.* New York: Liveright Publishing Corporation, 1929.

Krech, D. Dynamic systems as open neurological systems. *Psychol. Rev.*, 1950, **57**, 345-361.

Kubie, L. Theoretical aspects of sensory deprivation. In Solomon, P. et al. (eds.), *Sensory Deprivation.* Cambridge, Mass.: Harvard University Press, 1961, 218-220.

Kubzansky, P. E. The effects of reduced environmental stimulation on human behavior: a review. In Biderman, A. D. and Zimmer, H. (eds.), *The Manipulation of Human Behavior.* New York: John Wiley & Sons, Inc., 1961, 51-95.

Lewin, K. *Principles of Topological Psychology.* New York: McGraw-Hill Book Company, Inc., 1936.

Lilly, J. C. Mental effects of reduction of ordinary levels of physical stimuli on intact, healthy persons. *Psychiat. res. Rep.*, 1956, **5**, 1-28.

Lindsley, D. B. Common factors in sensory deprivation, sensory distortion and sensory overload. In Solomon, P., et al. (eds.), *Sensory Deprivation.* Cambridge, Mass.: Harvard University Press, 1961, 174-194.

Loeb, M., and Hawkes, G. R. The effect of rise and decay time on vigilance for weak auditory and cutaneous stimuli, *USA Med. res. Lab. Rep.*, No. 491, 1961.

———, and ———. Detection of differences in duration of acoustic and electrical cutaneous stimuli in a vigilance task. *J. Psychol.*, 1962, **54**, 101-111.

Loftus, J. P., and Hammer, Lois R. Weightlessness and performance. A review of the literature. *ASD tech. Rep. 61-166*, 1961.

Loveless, N. E. Signal detection with simultaneous visual and auditory presentation. Air Ministry, London, Flying Personnel Research Committee, Rep. 1027, 1957.

Mackworth, Jane F. The effect of intermittent signal probability upon vigilance. *Canad. J. Psychol.*, 1963, **17**, 82-89.

Mackworth, N. H. *Researches on the Measurement of Human Performance.* Med. Res. Council. Special Report No. 268. London: His Majesty's Stationery Office, 1950.

McCormack, P. D. A two-factor theory of vigilance. *Brit. J. Psychol.*, 1962, **53**, 357-363.

———, and Prysiazniuk, A. W. Reaction-time and regularity of interstimulus interval. *Percept. mot. Skills*, 1961, **13**, 15-18.

McGrath, J. J. The effect of irrelevant environmental stimulation on vigilance performance. Human Factors Research, Los Angeles, tech. Rep. No. 6, 1960.

———, Harabedian, A., and Buckner, D. Review and critique of the literature on vigilance performance. Human Factors Research, Los Angeles, Tech. Rep. No. 1, 1959.

Mead, G. H. *Mind, Self, and Society.* Chicago: University of Chicago Press, 1939.

Metzger, W. Optische Untersuchungen am Ganzfeld. II. Zur Phanomenologie des homogenen Ganzfelds. *Psychol. Forsch.*, 1930, **13**, 6-29.

Michotte, A. *The Perception of Causality*, New York: Basic Books, 1963.

Miller, G. A., Galanter, E., and Pribram, K. H. *Plans and the Structure of Behavior.* New York: Holt, Rinehart, and Winston, Inc., 1960.

Miller, J. W., and Hall, R. J. The problem of motion perception and orientation in the Ganzfeld. In Whitcomb, M. A. (ed.), *Visual Problems of the Armed Forces.* Washington: NAS-NRC, 1962, 14-20.

———, and Ludvigh, E. J. Visual detection in a uniformly luminous field. *J. aviat. Med.*, 1958, **29**, 603-608.

———, and ———. Time required for detection of stationary and moving objects as a function of size in homogeneous and partially structured visual fields. *USN Sch. aviat. Med. Rep.*, 1959, No. 15.

Morgan, C. T. *Introduction to Psychology.* New York: McGraw-Hill Book Company, Inc., 1961.

Mowrer, O. H., Rayman, N., and Bliss, E. L. Preparatory set (expectancy)—an experimental demonstration of its central locus. *J. exp. Psychol.*, 1940, **26**, 357-361.

Munn, N. L. *Introduction to Psychology.* Boston: Houghton Mifflin Company, 1962.

Murphy, G. *Personality.* New York: Harper & Brothers, 1947.

Ormiston, D. W. The effects of sensory deprivation and sensory bombardment on apparent movement thresholds. *Dissertation Abstr.*, 1958, **18**, 2200-2201.

———, and Finklestein, B. The effects of confinement on intellectual and perceptual functioning. *ASD tech. Rep.* 61-577, 1961.

Osborn, W. C., Sheldon, R. W., and Baker, R. A. Vigilance performance under conditions of redundant and nonredundant signal presentation. *J. appl. Psychol.*, 1963, **47**, 130-134.

Peters, J., Benjamin, F. B., Helvey, W. M., and Albright, G. A. Study of sensory deprivation, pain and personality relationships for space travel. *Aerospace Med.*, 1963, **34**, 830-837.

Petrie, A., Collins, W., and Solomon, P. The tolerance for pain and for sensory deprivation. *Amer. J. Psychol.*, 1960, **73**, 80-90.

Pigg, K. D., and Kama, W. N. The effect of transient weightlessness on visual acuity. *WADD tech. Rep. 61-184*, 1961.

Pollack, I., and Knaff, P. R. Maintenance of alertness by a loud auditory signal. *J. acoust. soc. Amer.*, 1958, **30**, 1013-1016.

Pollard, J. C., Uhr, L., and Jackson, C. W. Studies in sensory deprivation. *Arch. gen. Psychiat.*, 1963, **8**, 435-454.

Rees, D. W., and Copeland, Nola K. Discrimination of differences in mass of weightless objects. *WADD tech. Rep. 60-601*, 1960.

RIESEN, A. H. Arrested vision. *Sci. Amer.*, 1950, **183**, 16-19.

ROBACK, A. A. *History of American Psychology.* New York: Library Publishers, 1952.

ROBERTSON, M. H. Theoretical implications of sensory deprivation. *Psychol. Rec.*, 1961, **11**, 33-42.

ROMAN, J. A., WARREN, B. H., AND GRAYBIEL, A. The function of the semicircular canals during weightlessness. *Aerospace Med.*, 1963*a*, **34**, 1085-1089.

———, ———, AND ———. Observations of the elevator illusion during subgravity preceded by negative illusions. USN Bu. Med. Surg., Proj. MR005. 13-6001, subtask 1, Rep. No. 83, 1963*b*.

ROSENBAUM, G., DOBIE, SHIRLEY I., AND COHEN, B. D. Visual recognitive thresholds following sensory deprivation. *Amer. J. Psychol.*, 1959, **72**, 429-433.

RUFF, G. E., LEVY, E. Z., AND THALER, V. H. Studies of isolation and confinement. *Aerospace Med.*, 1959, **30**, 599-604.

———, ———, AND ———. Factors influencing reactions to reduced sensory input. In Solomon, P. et al. (eds.), *Sensory Deprivation.* Cambridge, Mass.: Harvard University Press, 1961, 72-90.

SCHAFER, T. H. Detection of a signal by several observers. USN Electronics Lab. Rep. 101, 1949.

SCHOCK, G. J. D. Apparent motion of a fixed luminous target during subgravity trajectories. *AFMDC tech Note 58-3*, 1958.

———. A study of animal reflexes during exposure to subgravity and weightlessness. *AFMDC tech Note 59-12*, 1959.

SCOTT, T. H. Literature review of the intellectual effects of perceptual isolation. Def. Res. Bd., Canada, Rep. No. HR66, 1957.

———, BEXTON, W. H., HERON, W., AND DOANE, B.K. Cognitive effects of perceptual isolation. *Canad. J. Psychol.*, 1959, **13**, 200-209.

SELLS, S. B. *Essentials of Psychology.* New York: The Ronald Press Company, 1962.

SIMON, H. A. Rational choice and the structure of the environment. *Psychol. Rev.*, 1956, **63**, 129-138.

SIMONS, J. C. Walking under zero-gravity condition. *WADC tech Note 59-327*, 1959.

———. Personal communication. Cited in Loftus, J. P., and Hammer, Lois R., Weightlessness and performance. A review of the literature. *ASD tech Rep. 61-166*, 1961.

SOLOMON, P., KUBZANSKY, P. E., LIEDERMAN, P. H., MENDELSON, J. H., TRUMBULL, R., AND WEXLER, D. (eds.). *Sensory deprivation.* Cambridge, Mass.: Harvard University Press, 1961.

STERN, W. *General Psychology from the Personalistic Standpoint.* New York: The Macmillan Co., 1938.

TEUBER, H. L. Sensory deprivation, sensory suppression, and agnosia: notes for a neurologic theory. *J. nerv. ment. Dis.*, 1961, **132**, 32-40.

TOLMAN, E. C., AND BRUNSWIK, E. The organism and the causal texture of the environment. *Psychol. Rev.*, 1935, **42**, 43-77.

VERNON, J. A., AND HOFFMAN, J. Effects of sensory deprivation on learning rate in human beings. *Science*, 1956, **123**, 1074-1075.

———, MARTON, T., AND PETERSON, E. Sensory deprivation and hallucinations. *Science*, 1961, **133**, 1808-1812.

———, AND MCGILL, T. E. The effect of sensory deprivation upon rote learning. *Amer. J. Psychol.*, 1957, **70**, 637-639.

———, AND ———. Utilization of visual stimulation during sensory deprivation. *Percept. mot. Skills*, 1960, **11**, 214.

———, AND ———. Sensory deprivation and pain threshold. *Science*, 1961, **133**, 330-331.

———, ———, GULICK, W. L., AND CANDLAND, D. K. Effect of sensory deprivation on some perceptual and motor skills. *Percept. mot. Skills*, 1959, **9**, 91-97.

———, ———, ———, AND ———. The effect of human isolation upon some perceptual and motor skills, In Solomon, P., et al. (eds.), *Sensory Deprivation.* Cambridge, Mass.: Harvard University Press, 1961, 41-57.

VON BECKH, H. J. Experiments with animal and human subjects under sub- and zero-gravity conditions during the dive and parabolic flight. *J. aviat. Med.*, 1954, **25**, 235-241.

WADE, J. E. Psychomotor performance under conditions of weightlessness. *AMRL TDR*, 62-73, 1962.

WATSON, J. B. *Psychology from the Standpoint of a Behaviorist.* Philadelphia: J. B. Lippincott Company, 1919.

———. *Behaviorism.* New York: W. W. Norton & Company, Inc., 1925.

WEIDENFELLER, E. W., BAKER, R. A., AND WARE, J. R. Effects of knowledge of results (true and false) on vigilance performance. *Percept. mot. Skills*, 1962, **14**, 211-215.

WESTHEIMER, G. Accommodation measurements in empty visual fields. *J. op. soc. Amer.*, 1957, **47**, 714.

———. Accommodation levels in empty visual fields. In Horne, E. P., and Whitcomb, M. A. (eds.), *Vision Research Reports.* Washington: NAS-NRC Pub. 835, 1960, 21-23.

WEXLER, D., MENDELSON, J., LEIDERMAN, P. H., AND SOLOMON, P. Sensory deprivation, a technique for studying psychiatric aspects of stress.

AMA Arch. Neurol. and Psychiat., 1958, 79, 225-233.

WHITESIDE, T. C. D. *The Problems of Vision in Flight at High Altitudes.* London: Butterworth & Co. Ltd., 1957.

WILKINSON, R. T. Comparison of paced, unpaced, irregular and continuous displays in watchkeeping. *Ergonomics*, 1961, **4**, 259-267.

WOODWORTH, R. S. *Dynamics of Behavior.* New York: Henry Holt and Company, Inc., 1958.

———, AND SCHOLSBERG, H. *Experimental Psychology*, New York: Henry Holt and Company, Inc., 1954, 261-265.

YUGANOV, E. M., KAS'IAN, I. I., AND YAZDOVSKII, V. I. Muscle tone during conditions of weightlessness. Cited by Loftus, J. P., and Hammer, Lois R. Weightlessness and performance. A review of the literature. *ASD tech. Rep.*, 61-160, 1961.

ZISKIND, E., AND AUGSBURG, T. Hallucinations in sensory deprivation—method or madness? *Science*, 1962, **137**, 992-993.

ZUBEK, J. P., AFTANAS, M., HASEK, J., SANSOM, W., SCHLUDERMANN, E., WILCOSH, L., AND WINOCUR, G. Intellectual and perceptual changes during prolonged perceptual deprivation: low illumination and noise level. *Percept. mot. Skills*, 1962, **15**, 171-198.

———, SANSOM, W., AND PRYSIAZNIUK, A. Intellectual changes during prolonged perceptual isolation (darkness and silence). *Canad. J. Psychol.*, 1960, **14**, 233-243.

CHAPTER 11

Some Theoretical Views of Creativity

J. P. GUILFORD

University of Southern California

No phenomenon or subject that is uniquely a responsibility of psychologists was so seriously neglected for so long or came to life so suddenly as that of creativity. In terms of publications on the subject, beginning with Galton's *Hereditary Genius* in 1869 and ending nine decades later, scientific interest has shown an exponential growth. After a panifully slow rate of increase in publications, in the late 1930's things began to stir in terms of empirical studies. But it is in the 1950's that the output on the subject virtually exploded. It can be said that at the end of that decade, the subject of creativity had been brought down from the clouds of mystery that had surrounded it and that the mists of hazy conceptions have to some extent been cleared away.

The postwar state of international tension, and particularly the emergence of Russia's first Sputnik, jolted us into a realization of critical needs for larger numbers of skilled, inventive people, to meet the many challenges. Rockets, guided missiles, men in orbit, trips to the moon, all have stirred our imaginations as nothing that has happened for centuries. New ideas are not merely tolerated but welcomed and demanded. The formerly anti-intellectual climate is fortunately giving way to a new respect for brains.

The many demands for creative people—scientists and engineers, especially—have called for knowing much more about the phenomena of creativity. This is where the psychologist comes in. We cannot do much about identifying creative talent, before that talent emerges in the production of creative products of recognized social merit, unless we know what enduring dispositions of children and youths are prognostic. We cannot do much in the way of promoting development of creative habits and attitudes unless we know some basic facts about the nature of creative thinking and its motivation.

MEANINGS OF CREATIVITY Different authors and speakers use the term "creativity" in many ways; one often has to infer from the context which meaning is probably intended. A difficulty is that, like "love," "creativity" is a "many-splendored thing."

feeling

Often the term is applied very broadly to a whole field of information, a subject pertaining to inventive people, inventive acts, and novel products. Some insist that there must be a tangible product, such as a poem, a work of art, or a scientific theory (Rogers, 1962; Arnold, 1962*b*), and some go so far as to say that the result must be useful (Mednick, 1962). Although the demand for a tangible product that merits social recognition, also utilitarian value, may be quite meaningful and appropriate for the purposes of technologies, it is not appropriate for a definition in basic psychology.

Two kinds of products must be distinguished. The kind just mentioned is borrowed from the commonsense view of creative people and their outputs. From the point of view of basic psychology, there is a much larger class of products, which also includes marks of pencil on paper, uttered words, expressed ideas, and even ideas merely thought of. The latter may be called "*psychological* products." The varieties of recognized psychological products will be described in the next section. The criterion of "usefulness" involves a value judgment in a way that science cannot tolerate or manage. Many a useless invention is an invention nonetheless. Psychologically, the kinds of mental activity that go into inventions, useful or useless, are essentially alike. The same may be said when the product is merely a psychological one. This writer cannot agree with Carl Rogers (1962) when he states that phantasies are not creative unless expressed in a tangible product. What difference is there psychologically in the kind of thinking, whether or not a tangible product emerges?

This writer will not attempt to give a formal definition of "creativity" for the same reason that authors often avoid defining concepts like "learning" or "emotion." The meanings of such concepts cannot be adequately boiled down into one simple statement. But a few important distinctions should be pointed out, for the sake of comprehending more fully what is to follow. "Creativity" sometimes refers to *creative potential,* sometimes to *creative production,* and sometimes to *creative productivity.*

Creative potential is the personal disposition of an individual that is describable in terms of a collection of relatively enduring qualities that he possesses and that prepare him for creative thinking. Creative thinking culminates in novel ideas, which may or may not emerge in the form of tangible products, as stated before.

Novelty need apply only within the frame of reference of the person himself. If we say that an idea is novel only if no one before has ever achieved it, we are completely blocked; for we could never hope to establish the fact, one way or the other. We have some possibility of establishing whether or not the idea is novel for the individual by knowing his past history. We rarely have enough information regarding his past history, however, to be certain. Operationally, indirect evidence can be found by demonstrating that a reasonably large sample of persons of similar background do not have the idea. In other words, we can establish the fact that his idea is unique in his population, where his population can be sampled in reasonable number.

The term "creative production" does not necessarily mean tangible products. It refers to the processes of productive thinking, a subject which will be treated in some detail in a later section. For the purposes of scientific investigation, we hope and expect that there will be enough observable evidence in the way of overt behavior and behavioral effects. One of the greatest difficulties has been that there is an insufficiency of such observable data. This has misled many thinkers and investigators to attribute a large part of creative thinking to unconscious or preconscious regions, beyond the inspection even of the thinker himself.

For those concerned with applied fields, "creative productivity" means quantity of output in the form of socially recognized products. In terms of basic science, it means another kind of quantity of output, namely, number of responses of certain kinds, e.g., number of words, ideas, sentences, or other products of generalized, psychological types. These products are at a much less spectacular, commonsense level, of course, than those of a poet, a composer, or an inventor.

A PREVIEW OF WHAT IS TO COME Much of the advance in the empirical investigation of creativity during the past decade has come about through new ways of looking at the problem of creativity and through the applications of new experimental techniques. As the next section will explain, the major approach, in one way or another, has been through studies of individual differences.

One of the first needs in exploring any new field, is to map it in terms of a system of concepts. Application of the procedures of factor analysis to the problem has provided much toward a mapping of the field of creative potential. In doing so, it has also led to understandings of creative thinking. Having the new concepts available, it is possible to initiate experiments of more usual types with fruitful results. Some of these findings will be utilized in later sections.

The heart of this chapter is concerned with the nature of the psychological operations involved in the activity of creative thinking and the conditions affecting that thinking. A quick review will be given of theories that have been held regarding creative processes, and a new general theory will be proposed, based upon the findings mentioned in the next section, and other findings.

Among the conditions affecting creative output are some social factors in the milieu within which the creative thinker finds himself. Creative thinking of individuals in groups presents some special problems. Various kinds of individuals and what they do often affect the creative thinker, favorably or unfavorably.

There are many, excepting extreme hereditarians, who want to know to what extent steps can be taken to augment creative potential or to help individuals realize the fruition of their full potential and, if improvement can be fostered, they want to know what those steps are. There is the usual genetic problem of how creative performances change over the life span of the average individual, particularly during the growing years. Numerous experiments have demonstrated that creative performances can be improved by the application of various training procedures. There is still much to be learned regarding the best ways in which to educate for creativity.

A MULTIVARIATE VIEW OF CREATIVITY

As indicated earlier, the field of creative potential has been extensively mapped through an application of factor analysis. Factor analysis comes in a general family of research methods known as multivariate analysis. To start with a broad view, let us see what the significance of this category of research is.

NATURE OF MULTIVARIATE METHODS Since it became the science of behavior, traditional experimental psychology has cast its problems in terms of stimulus-response relationships. Independent variables are found in certain stimulus conditions (S) and dependent variables are observed in certain aspects of responses (R). The traditional design has been to vary one stimulus condition, e.g., number of applications of S, holding constant other stimuli, and possibly also other aspects of the individual O, while varying the independent variable systematically. Modern experimental designs have permitted systematic variation of more than one S condition in the same experiment, but this elaboration cannot conveniently be carried very far without undesirable complications. The end result is in terms of stating descriptively some orderly relationship between dependent and independent variables. In contrast to a procedure like factor analysis, such experimentation is commonly known as "bivariate analysis."

The Experimental Application of Factor Analysis. The multivariate methods, although applying stimuli and thus instigating responses, usually focus attention upon relationships among the response variables only. In the usual type of factor analysis, responses would be obtained from a large number of individuals, each presumably behaving in response to the same experimental situation, e.g., a psychological test. If under such conditions there are differences among individuals in the responses (scores) they make, something in the dispositions of those individuals is thought to be responsible. It is that "something" within the nature of the individuals in which the factor analyst is interested.

Although not denying that it is important to know the principles by which variations in stimulus conditions produce variations in responses in the typical member of a species, the factor analyst insists that conditions within the individual, conditions of either temporary or of more enduring nature, help to determine each individual's response. The more enduring conditions may be regarded as personality traits.

The study of S-R relationships is handicapped in segregating traits and in getting at the nature of those traits. The S-R approach can manipulate temporary personal conditions, such as motivation, fatigue, and relaxation. It cannot manipulate the enduring traits within individuals without the experimenter devoting intolerable amounts of time and also more know-how than is yet available. The multivariate psychologist can take his individuals as they come, provided they are within a population that is homogeneous in certain ways that he desires, such as age, sex, and formal education. They may vary simultaneously in quite a number of traits that affect behavior in the experiment. The method of factor analysis, when properly applied, is adequate to the task of determining how many pertinent traits are having effects and also something about the nature of those traits.

To be more specific, and to apply this abstract picture to investigation of creativity, let us make two assumptions. The first assumption is that creativity is not something limited to the socially recognized few geniuses, but that creative potential is distributed continuously throughout the human race. The geniuses are merely at the upper tail of the distribution. Incidentally, this one assumption opens up the possibility of investigating creativity without our being dependent upon the very rare shining lights in the population.

The second assumption is that creative potential is not just one, uniform ability wherever it is found but a composite of a number of contributing abilities and other traits. This assumption appears to be in flat contradiction to the stated views of two respected psychologists. Rogers (1962) asserts that creativity is the same wherever we find it; in painting a picture, composing music, inventing a new instrument of killing, devising a new scientific theory, or developing a new procedure for handling people. Thurstone (1952, p. 55) stated that "creative talent is qualitatively the same at all levels," meaning by "levels" such distinctions as between trades and professions.[1]

We shall soon see that if these authors meant that there is but one factor or primary ability of creativity that functions equally in all areas, they are wrong. We shall see that there are a surprisingly large number of creative abilities, each limited in scope, in properties, and undoubtedly in its applications. It is only fair to say, however, that in spite of the differentiation of these abilities, there are definite parallels among them; there are *classes* of similar abilities, which gives some superficial basis for expecting unity.

In applying factor analysis, the observations are usually in terms of test scores. These are the raw data. The tests all differ from one another in various ways. In the best experimental use of factor analysis, one starts with a hypothesis concerning the nature of each factor; sometimes there are two or more alternative hypotheses (Guilford, 1950). One then constructs or selects three or more different tests that should measure the hypothesized trait in common. For example, for Thurstone's well-known factor of word fluency, one test asks the examinee to list as rapidly as he can all words he can think of beginning with the letter S; a second calls for listing words ending in -sion; and a third calls for listing words each containing the letters E and M. All of these tests would intercorrelate substantially, while not correlating to any extent with those of different character. We know this now from much experience in factor-analyzing such tests; it was not known before analysis.

The experimental controls available to the factor analyst reside in the nature of the tests constructed and in the kind of population to whom the tests are given. The test instructions are intended to arouse similar high levels of motivation in all examinees and to control the way in which examinees go to work on the items. Efforts should be made to construct items in such ways that they can be solved correctly only if examinees all do them in the manner intended. Or examinees have to do the items in that way if they are to achieve high scores. The tested population should be of closely similar age (the younger the group the truer this is), similar educational level, and sometimes of the same sex.

A factor-analytic treatment of data begins with a correlation matrix, which includes all the coefficients of correlation between pairs of tests. After extractions of factors and rotations of axes, it ends with a factor matrix, which includes correlations of all the tests with each factor, the number of factors being ordinarily less than half the number of tests. The psychologist at this point again applies his intuitive powers, attempting to see for what kind of psychological variable each factor stands.

Factors and Functions. If interpretable, the factor variable is taken to stand for a trait, i.e., a mode of variation among individuals in some psychological quality. The psychological quality is inferred from the features of the tests that measure it in common; they suggest the kind of psychological function it should take to make a high score in such tests.

The last statement has further important logical implications. What is called an ability variable when we speak of individual differences becomes logically a kind of function when we speak of the behaving individual. Different individuals perform at different levels of excellence in a particular way of functioning. In this way, the results of factor analysis throw light upon the psychological *activities* of individuals. In the area of creativity, this means that we learn something about the nature of creative thinking from knowledge of the nature of the factors in that domain.[2]

---

[1] In view of Thurstone's promotion of multiple-factor theory, this statement is puzzling. He also seemed to forget his own and his students' differentiations of more than one fluency factor. One can only infer that he did not at the time link fluency factors with creative thinking. Thurstone's theory of creative thinking will be mentioned later.

[2] The role of factor analysis as an experimental approach to basic psychological research is discussed at much greater length elsewhere (Guilford, 1961).

CREATIVE-APTITUDE FACTORS Although it is recognized that creative potential involves motivational and temperamental aspects as well as aptitude aspects, most of the factor-analytic work in this area has been on abilities; only a little on interests, needs, attitudes, and temperamental traits. Aptitudes pertain to what persons *can* do, whereas other kinds of traits help to determine what they *will* do, with what zest, and in what manner. It is from the study of aptitudes that we can learn most regarding the nature of creative thinking.

Before the 1950's there had been a few precedents for believing in the hypothesis of the multivariate nature of creative potential. As early as 1927, Hargreaves had found that tests of fluency and of originality separated to indicate group factors, which were also distinct from Spearman's g. Thurstone and his students had distinguished as many as four factors in various kinds of tests of verbal fluency (Thurstone, 1938; Carroll, 1941; Taylor, 1947).

Several analyses since 1950 have served to extend the number of known factors of kinds that should most likely contribute to the success of creative performances (Wilson, et al., 1954; Guilford and Christensen, 1956; Berger, et al., 1957; Frick, et al., 1959; Kettner, et al., 1959; Guilford, et al., 1961). Major attention has been given to factors of fluency, flexibility, and originality, but there are others. We shall consider the properties of the factors in these particular areas, for this information reveals the facets of thinking activities involved in mental creation. One of the chief advantages of interpreted-factor concepts is that they are tied to observable information—the tests that represent them. They are therefore in the category of empirical concepts; their definitions are referential. Some of the typical tests for each factor will therefore be mentioned and briefly described. The conceived psychological factor derives its properties inferentially from the basic psychological nature of the tasks involved in the tests.

Fluency Factors. Earlier analyses in the 1950's verified and more clearly defined the four fluency factors, discovered three kinds of flexibility, as well as a factor that could be called "originality." The factor of word fluency (WF) has already been mentioned, with kinds of tests to which it refers. Ideational fluency (IF) may also be assessed by tests calling for listing of words, but unlike WF tests, in which the words are recalled to satisfy some spelling or letter requirement, IF tests call for meaningful verbal responses. The examinee (*E*) may be asked to list things that are hard, round, and smaller than a basketball. Other IF tests call for more than single-word responses. One calls for lists of uses for a common, wooden lead pencil; another calls for suggested titles for a short story; still another may call for ideas connected with a man climbing a ladder. All these responses, whether one word or several, may be regarded as representing units of meaning.

A factor of associational fluency (AF) calls for single, meaningful, verbal responses, not as members of a specified class as with factor IF, but words to satisfy a relationship. A test called Controlled Associations tells *E* to list synonyms for a given word, such as "black." This instruction and stimulus word may elicit responses such as "dark, lightless, hopeless," etc. The relation is similarity. Simile Insertion calls for supplying different adjectives in each item, such as "The cat's fur was as ______ as feathers."

Expressional fluency (EF) requires items calling for the production of connected discourse. The responses need not be complete sentences; even two words in appropriate phrase sequence would do, particularly with children. The tasks for EF thus require an organizing activity. A test called "Four-Word Combinations" asks for four-word sentences. It makes little difference whether or not the initial letters of the words are specified, as "W____ f____ s____ n____," repeated for a number of items. The responses might be "We found some nuts," "Who fought Sam Newton?", "Wiley Frank smiled notoriously," and so on. *E* is told not to use the same word more than once. Somewhat different tests present perhaps three to five words, all of which are to be used in sentences. Another, more complex task presents twenty words, all of which must be worked into a paragraph. As usual, the simpler the test, the more experimental control we have over what *E* does, and the more readily is the performance scored.

Flexibility Factors. In all of the fluency tests, the quantity of production within limited time is the common feature. Quality is no consideration. Only acceptability of a response within the broad restrictions of the instructions is applied as a criterion. When the scorer is uncertain, as in AF tests, he gives *E* the benefit of the doubt. Efficiency of recall is therefore the prime consideration. More will be said about the role of recall in creative thinking in a later section.

Where one factor of flexibility was expected, two have been repeatedly verified, one called "spontaneous" flexibility, the other "adaptive" flexibility. The former was first found in the Brick Uses test, which calls for listing uses of a common brick, with ten minutes allowed. A simple count of given uses is a good measure of the factor IF. When scored in terms of a "shift" score, however, the performance on Brick Uses provides a measure of spontaneous flexibility (SX). A "shift" is a going from one class of uses to another in two contiguous responses. For example, after a run of uses for building material a change to a new run of uses as a missile ("throw at a dog, throw at a cat, etc.) would constitute one shift. The upper limit of shift counts would be one less than the total number of responses; the lower limit would be zero. There is usually a very low correlation between the two scores. Another test, Alternate Uses (formerly Unusual Uses), virtually compels the examinee to shift classes for each new response. Given the instruction "A newspaper is ordinarily used for reading; give some other kinds of uses," he may respond with: "wrap garbage, swat flies, pack china, make a baseball bat, make insulation, use as an umbrella, etc." The qualifier "spontaneous" in the name of this factor arose from the fact that in the Brick Uses test the examinee is free to vary his classes or not, as he is inclined to do. Later interpretations have minimized the significance of that qualifier, as we shall see.

Adaptive flexibility (AX) was first found in the test called Match Problems. In Match Problems, based on a familiar game, *E* is given a number of contiguous squares, each side made by a match stick. He is told to take away a specified number of matches to leave a specified number of squares, with no extra matches left over. Some items also involve triangles. The task itself undoubtedly involves a ready shifting of strategy, in considerable trial-and-error behavior. With this shifting, the matches and squares must continually look different to *E*. In addition, some solutions involve unusual tactics, such as leaving one square four times the area of others. In other forms, the Match Problems test asks *E* to solve the same problem in as many as four different ways. This form provides further necessity for variation of approach on the part of the examinee. The test Planning Air Maneuvers calls upon *E* to show how an airplane should be flown in skywriting to produce two consecutive capital letters efficiently. It also involves considerable trial and error, with variations of possible solutions.

Originality. The measurement of the hypothesized factor of originality (O) was approached with three alternative principles (Wilson et al., 1953). The novel response is a rare response. Rarity of a response in a given, homogeneous population is statistically determinable by frequency counts. For some tests, in a sample of 400 young men of rather narrow range of formal education, the frequency of every response was counted. Each response could then be given a weight inversely proportional to its frequency, and this weight was used in scoring. The Quick Response test presented *E* with common word stimuli, to each of which he had five seconds to give a response. His score is a weighted sum of his responses.

Many novel responses are unusual because they are remotely associated with the stimulus; they are "way out," in the "beatnik" sense. Such a principle has been applied in the Consequences test. Each item poses a hypothetical change, such as finding a way of making it unnecessary for people to eat food any longer. *E* is to list all the consequences he can think of in a limited time. In scoring the test, a discrimination is made between obvious and remote consequences. The obvious ones are more direct and almost anyone would think of them. The remote ones are distant in time or in a chain of events or pertain to some often-overlooked aspect of the problem. The number of obvious responses given by *E* makes a good score for ideational fluency. The number of remote responses provides a score for originality.

The third principle tried out for measuring originality uses the standard of cleverness. In response to a test like Plot Titles, for example, *E* may give plain, ordinary, even banal titles, or he may give clever, perhaps humorous, or "cute" expressions for titles; most *E*s give a mixture of the two kinds. Suppose the story given is the fable of the fox and the sour grapes. Titles such as "The fox and the grapes," "The fox who gave up," or "The grapes were too high for the fox," while descriptive, are not very imaginative. On the other hand, "The out-foxed fox" and "The fox griped about grapes" are interesting as well as descriptive. The number of nonclever responses that *E* gives is his score for factor IF; the number of clever responses is his score for factor O. Other tests that have been found to measure factor O to a significant degree are Cartoons, to which *E* gives suggested "punch lines"; Riddles, to which he gives clever solutions, and Symbol Production, in which *E* gives a very sketchy line symbol in connection with each of a list of common words. Since the last-mentioned test does not obviously conform to any of the three principles of measurement of originality, we shall have to consider a broader view, which can only come later.

Elaboration. A factor of a different character, in some ways, not hypothesized at the beginning of the series of studies at the University of Southern California, has been found, first, in an analysis of abilities entering into planning. The common operations of planning should involve considerable creative thinking. The factor in question is called "semantic elaboration." The reason for the "semantic" term in the label will be explained later. It was thought that the later stages of planning should involve considerable working out of details, once the broad scheme is adopted. As a matter of fact, planning is not unique in this respect. The final steps in connection with almost any creative product requires additions of details and final touches. Such a factor was found in a test called "Planning Elaboration," for the good reason that, giving the outline of a plan, it asked *E* to fill in the details needed to make the plan work.

Some Other Kinds of Factors. In 1950 (Guilford, 1950) it was hypothesized that an important quality in which recognized creative people stand high is "sensitivity to problems." The problem-sensitive person notices and is curious about anything that stands out as being unusual, and things seem unusual to him that are passed over by those less sensitive. Torrance et al. (1962*c*) recently remarked "Thick-skinned people do no creative thinking." They go on to say that such people know everything; to them nothing is baffling.

A number of tests were developed in which the score should reflect how readily the examinee appreciates defects, deficiencies, and inconsistencies. In the Apparatus test, *E* is told to state two things that are wrong with each of several common appliances, such as a toaster or automobile, or things that need improvement. In the Seeing Problems, *E* is told to list problems that may arise in connection with the use of a common object like a candle. In Seeing Deficiencies, he is told to say what is faulty about a proposed plan of action, such as in paving a street and laying a sewer, in that order. These tests, with others, have demonstrated a factor of sensitivity to problems.

A factor of redefinition was also hypothesized; an ability to adapt an object or part of an object to some very new and different specified use. An item from Gestalt Transformations reads: Which object could be utilized in starting a fire?

A. rope
B. cabbage
C. pocket watch
D. fish
E. needle

The keyed answer is "C. pocket watch," because presumably the face cover could be adapted to use as a condensing lens to focus the sun's rays. Object Synthesis calls for combining two objects to make them serve some function. The two objects might be a pin and a string, which could become a fishing line. Picture Gestalt presents a picture of many common objects, as in an ordinary kitchen, and a list of uses to be served potentially from things in the picture. All these tests help to define the factor called "semantic redefinition," a third kind of flexibility in thinking. Later considerations have recognized the logical connection between redefinition and the commonly known concept of "functional fixedness." The one is probably at the opposite pole from the other on the same continuum. More will be said about this point later.

CREATIVE ABILITIES AND THE STRUCTURE OF INTELLECT Considerable additional significance has accrued to these factors of creative ability from the fact that they have been integrated into a single, systematic, and comprehensive model known as the "structure of intellect" (Guilford, 1959; Guilford and Merrifield, 1960). The interrelating of these factors with one another and with other intellectual abilities in a system has lent new and sometimes different meanings to them.

The structure of intellect has been described in a number of places; only minimum treatment will be given here. The classification of the known intellectual-ability factors is done in three intersecting ways; according to the kind of operation, the kind of content or information, and the kind of psychological product involved. When these three aspects of a factor are specified, the unique character of the factor is essentially covered. The three sets of categories (with letter symbols often used in place of them) are as follows:

Operations
(C) Cognition
(M) Memory
(D) Divergent production
(N) Convergent production
(E) Evaluation

Contents
(F) Figural
(S) Symbolic
(M) Semantic
(B) Behavioral

Products
(U) Units
(C) Classes
(R) Relations
(S) Systems
(T) Transformations
(I) Implications

Space will not be taken to define every concept here, except as needed. But it can be seen that by combining every kind of operation with every kind of content and every kind of product, a total of 5 × 4 × 6 unique combinations are available. Of the 120 possibilities, 80 have already been realized in terms of demonstrated factors, and many of the predicted but unknown factors are under investigation.

Divergent-Production Abilities. We are most concerned here with the place of the more clearly recognized creative-potential factors in the system and what new significances they acquire by reason of their places within the system. The factors of fluency, flexibility, originality, and elaboration fall in the operation category of divergent production. The "production" part of this label means that in tasks of the abilities in this category, when the examinee is given an item of information, he produces other items of information according to specifications of the test instructions. The "divergent" part of the label means that *E* must produce several items of information and they must all be different.

Divergent production (DP), as a category concept, is parallel to the convergent-production category. Abilities in the latter category deal with tasks in which there is one right answer; the given information fully determines it, or convention dictates it in response to the given information. Parallel to the DP factor of ideational fluency, there is a naming-ability factor, found in naming tasks in which there is a unique name for everything presented, whether the thing is concrete or abstract. Parallel to the factor of associational fluency is an ability to complete logic-tight analogies. The difference can be illustrated by the two kinds of questions: "What word means *exactly* the opposite of wet?" (a convergent-production item) and "What words mean *almost* the opposite of wet?", a divergent-production item. In the latter case the multiple-response list might include: arid, desiccated, sec, wry, uninteresting, etc.

Most of the DP factors mentioned above involve meaningful words. They belong in the content category called "semantic." Two of the factors are not in the semantic category—word fluency, whose content is symbolic, and adaptive flexibility, whose content is figural. Within the semantic area of divergent-production abilities, the factors of fluency, etc., are distinguished in terms of the kinds of products involved. Ideational fluency pertains to ideas, expressible in single words or a small number of words; in other words, the product is a unit. In terms of its place in the structure of intellect, factor IF is the ability to produce divergent, verbally meaningful units of information. To use the category letter symbols, ideational fluency is factor DMU. Word fluency is concerned with symbolic units, hence it is factor DSU. There should be a factor DFU, and that factor has recently been demonstrated. A test of it asks *E* to use three given lines to make as many different unitary combinations as he can. There should be a factor DBU, and such an ability has now been demonstrated. Behavioral units are feelings, attitudes, intentions, etc., of individuals, as portrayed in their expressive behavior.

Spontaneous flexibility, which, as stated before, is measured by the readiness with which *E* shifts from class to class, is recognized as factor DMC, the divergent production of semantic classes. It can be said that at least some uncreative persons are suffering from a disease that might be called "hardening of the categories." This describes a low status on factor DMC.

Associational fluency, it may be remembered, was interpreted as dealing with relations. The items for tests of AF specify or imply that a certain relation—similarity, opposition, and so on—is to be satisfied by producing a word or words. Factor AF is therefore identified as DMR.

Expressional fluency is the prime feature of tests of producing connected discourse. The invention of phrases and sentences is a matter of producing semantic systems. Factor EF is therefore identified in the system of intellectual factors as DMS; the divergent production of semantic systems. Many writers on creativity stress the fact that creative activity is a matter of putting old things into new combinations. Combinations are systems, and to this extent this writer can agree. But note that systems are not the only kind of psychological product that can be invented, hence the identification of all creative production with combination making (system making) is a narrow view.

The factor of originality has been identified in the structure of intellect as the divergent production of semantic transformations. A transformation is any kind of change in a psychological product. A semantic transformation occurs whenever a meaningful word is redefined. When a person sees that the word "fair" may mean "blond," "equitable," or "a carnival-type event," as he goes from one meaning to the next, he is engaging in a transformation. The application of the concept of transformation to originality tests scored for cleverness is very obvious. A twist of meaning is often the basis for making the title clever.

Transformations also provide an important basis for humor. This is illustrated by the story of two ladies, who were acquainted and who met by surprise in the waiting room of a psychiatrist. One said to the other, "Well, how nice to see you. I didn't expect to see you here. Are you coming or going?" The other replied, "Well, I really don't know. If I knew, I probably wouldn't be here." Getzels and Jackson (1961), and others, have noted the strong tendency for those who score high on verbal creative-thinking tests to appreciate humor and to produce it.

Adaptive flexibility involves transformations in *figural* material or content. In the Match Problems test, being able to revise or reinterpret combinations of lines and to change strategy accordingly appears to be the secret of success. It is interesting that adaptive flexibility turns out to be parallel to originality. It is factor DFT, whereas originality is DMT. One might say that adaptive flexibility is originality with concrete, visual content or that originality is adaptive flexibility with semantic content.

Semantic elaboration is now recognized as the divergent production of semantic implications, or factor DMI. It will now be seen why the "semantic" part of the original label was added; to distinguish elaboration with meanings from elaboration with figural or other kinds of information. A factor DFI has been found recently. Tests of it call for *E* to add lines to what is already given. The more he adds, the higher his score. There is also a known DSI factor, but not yet a DBI factor.

Abilities Differentiated by Content. Apart from the behavioral category, sixteen of the eighteen expected abilities of the divergent-production category have been explored and demonstrated. This statement implies a more general principle that should be emphasized. This principle is to the effect that ability factors are not strongly correlated. We should therefore not expect any individual to be equally able in all areas of content or information or in all product areas. If a person is relatively high in all of the figural DP abilities, he probably has good potential for success in creating with concrete materials—inventing, painting, or composing.

There is even reason to believe that the same person is not likely to be equally capable in visual-figural and auditory-figural abilities. A visual-auditory distinction has already been found in two places in the structure of intellect; in the cognition of figural units and in the memory for figural systems. There is an ability to recognize visual units separate from an ability to recognize auditory units; also a memory for visual systems separate from a memory for auditory systems. These distinctions happen to be outside the DP category, but similar distinctions should theoretically apply in that category by analogy. There has been no analysis of auditory DP tests as yet. Some observations of Agnew (1922*b*) suggest further differentiation of abilities. From experiences with five great composers, he concluded that besides using auditory imagery, which is spontaneous, composers also utilize visual and kinesthetic imagery. Thus a set of kinesthetic factors is a possibility.

We should therefore not expect a person to be equally creative in music and in painting. The very rare person *could* be, but this is improbable.

Kinds of products should be in common, it is true, and there would be the possibility of translating across from visual to auditory information. A dramatic example of this is the very rare synesthetic individual, for whom the translation is automatic. For others it probably has to be learned. (Different contents are like different languages.) Different languages have cognate terms and equivalents, but unless the person is good in both languages, there is a translation problem.

Welch (1946) has provided some striking evidence for unevenness of individuals in figural versus semantic abilities in a study designed for another purpose. He administered four DP tests to forty-eight college students and thirty professional artists. The average age of the latter group was somewhat higher, but that fact is probably irrelevant for the conclusions to be drawn here. The tasks were characterized as follows:

1. Recombine words into meaningful sentences
2. Combine given lines to form letters of the alphabet
3. Employ twenty given words in writing a story
4. Construct pieces of model furniture from ten blocks.

These descriptions clearly suggest that tests 1 and 3 are semantic in content and 2 and 4 are figural. Both kinds seem to emphasize the divergent production of systems, or possibly units, in the case of test 2. No significant differences were found in means of students and artists in the case of tests 1 and 3, but differences between the two groups in tests 2 and 4 were highly significant, with t ratios of 13.2 and 10.2, respectively. This kind of result could well be predicted on the basis of the knowledge that figural and semantic DP abilities are relatively independent. Without that information, Welch had predicted significant differences in all four tests. The same kind of expectation would probably have been true of Lowenfeld (1959), who repeatedly asserted that creativity is much the same everywhere, in art as in science.

In the fields of science and mathematics, we find additional indications of relatively independent functioning of figural, symbolic, and semantic abilities. Roe (1951) concluded that the top scientists she studied tended toward one of two classes, visual versus verbal. This difference can be paraphrased as figural versus semantic. Biologists and experimental physicists resorted more to visual thinking, whereas theoretical physicists and social scientists resorted more to verbal thinking. Besides the likelihood that problems with which the scientists have to deal in the various fields are different, there is another tenable hypothesis that each person tends to favor the use of those abilities in which he has found he is strongest. (Roe offered the hypothesis that the type of thinking to which the person is most accustomed helps determine his choice of field. Test results confirmed Roe's conclusion. Her verbally inclined scientists made higher mean scores on the verbal tests, whereas the visual thinkers did better on nonverbal tests.)

Einstein recognized distinctions between verbal and other kinds of thinking in a letter to Hadamard:

> The words of the language, as they are written or spoken, do not seem to play any role in my mechanism of thought. The psychical entities which seem to serve as elements in thought are certain signs and more or less clear images which can be "voluntarily" reproduced and combined. . . . before there is any connection with logical construction in words or other kinds of signs which can be communicated to others. (Hadamard, 1945, p. 142.)

Einstein's reference to images must mean that he engaged in some figural thinking; his reference to signs must mean that he engaged in symbolic thinking. He may have combined the two or he may have gone with great facility from one to the other. Hadamard (1945) remarked that mathematicians differ considerably in their observed use of imagery of different kinds and of verbal versus nonverbal thinking. (The personal choice, as with scientists, may have some relation to the kind of content factors in which the individual has found he can do his thinking most readily. He may then translate the final product, if necessary, into the language in which he finally intends to communicate to others.)

Transformation factors. The factors of semantic redefinition and sensitivity to problems are definitely not DP abilities. In tests of semantic redefinition, such as Gestalt Transformations, there is one right answer to each problem. This immediately suggests *convergent* production, not DP.) The fact that a redefinition is the key to success tells us that a transformation is the product involved. The factor of semantic redefinition is clearly parallel to factor DMT (originality), which is concerned with *multiple* redefinitions. Hence, the structure-of-intellect designation for semantic redefinition is NMT.

The "semantic" in the original label was to distinguish factor NMT from two other known factors NFT and NST, figural- and symbolic-redefinition factors, respectively. Tests of NFT and NST are like those with hidden figures on the one hand, and hidden words on the other. Penetration of Camouflage requires finding faces concealed in foliage, etc. Hidden Figures, based upon the Gottschaldt-figures experiment, requires the segregation of line forms from more complicated forms. Lines must be redefined and given new functions in order to succeed. Items in a test called "Camouflaged Words," are like the following:

What name of a game or sport is concealed in these sentences?

I did not know that he was ailing.
He took a Mongol for his bride.
Oh, see the coward ice skating.

The plausible pertinence of these three transformation abilities for creative potential, all outside the DP category, suggests that other transformation factors might also be relevant, for example the transformation factors in the cognition category. One of these, CFT, is more commonly known as "spatial visualization," an ability to think of movements or other changes of visual objects in space. If all transformation abilities are relevant for creative performance, we have 5 × 6 or thirty potential abilities to add to the twenty-four divergent-production abilities, except for the four factors where those two categories intersect.

Evaluation Abilities and Others. There remains the factor of sensitivity to problems to relate to the structure-of-intellect. It is neither a DP nor a transformation ability. The common conception has regarded it as an evaluative ability and has placed it in the operation category of evaluation. It appeared to be a matter of evaluation, because in tests calling for pointing out defects and deficiencies, the examinee is applying standards or criteria of what is satisfactory and what is not. The known ability is in the semantic category, the items being composed of meaningful information. It was not certain that the products involved are implications, but that had been the decision, pending further research. Implications are extrapolations from given information to what one might call "intrinsic" information. A toaster that will not automatically adjust itself to the dryness of bread, will sometimes give us a burned or an underdone product. Such outcomes are implied by the posed fact; both are unwanted outcomes. From this kind of reasoning, sensitivity to problems has been regarded as factor EMI.[3]

Thus, we have to go outside the DP and transformation areas to include all abilities that seem relevant to successful creative thinking. And while we are about it, we may as well admit that many abilities that do not seem directly applicable in the creative-thinking scheme, may nonetheless make their contributions indirectly to the success of the final outcome. For example, a creative writer would not get far without having a vocabulary of respectable size. The size of vocabulary is the unique measure of factor CMU—cognition of semantic units. A creative mathematician would not get very far without having abilities to cognize symbolic systems (equations, for example). Memory abilities must be important, for every creator draws heavily upon stored information. Being high in all such abilities would not be sufficient conditions for creative performance, but they certainly lend secondary, but necessary, aids.

CONSTRUCT VALIDITY OF FACTORS AND TESTS

Proponents of factors and factor tests are frequently challenged to cite evidence that their factor constructs are "genuine" and that the factor tests measure variables of general significance. This is quite appropriate, but the question of construct validity of tests should apply less to factorists than to others. In the view of the writer, the best possible evidence of construct validity is information regarding factorial validity, which, operationally, is given in terms of factor loadings. This view is far from widely accepted and even factorists should welcome evidences from other than factor-analytic sources, consequently information from other sources will be given.

Torrance (1962*a*) has considerable information derived from relating scores on divergent-production tests given to school children to nominations of their teachers and their peers. High-scoring children in the lower grades are nominated for having wild, silly, and sometimes naughty ideas. This is more true of boys than of girls. By grades five and six, high-DP children are nominated for having "good" ideas. High-DP children also show unusual ideas and a penchant for adding nonessential details in their drawings. They come out with ideas so unusual that they are difficult to evaluate by ordinary standards. When a high-scoring child on DP tests is planted in a group of five, the other four having lower scores, the high-DP child initiates ideas far out of proportion in group problem solving. In high school, Torrance found a correlation of .24 between DP-test scores and a nomination criterion.

---

[3] From a new analysis coming to completion after this was written, it appears that sensitivity to problems is more likely a cognitive ability; an awareness of semanitc implications, with the symbol CMI, with a new factor demonstrated that is more reasonably defined as EMI.

In studies of originality, Barron (1955) related scores from three DP tests (Unusual Uses, Plot Titles, Consequences) to various other assessments of originality in 100 Air Force Captains. The tests correlated .30, .36, and .32 with assessment-staff ratings for originality. A multiple correlation of .55 was found between the same criterion and a combination of eight tests that included the three DP tests and other apparent DP tests.

Zaccaria et al. (1956), using as a criterion a score of biographical information that stressed signs of creativity, found significant correlations with it for eleven out of fifteen scores from tests of creative factors. Drevdahl (1956) used faculty evaluations of advanced students in arts and sciences as criteria. Tests of fluency, flexibility, and originality yielded few significant correlations, the highest, for a test of originality, being .33. Predictive-validity correlations in samples of research scientists have been very low for tests of creative factors (C. W. Taylor et al., 1961), but the few validation studies have actually included very few creative-aptitude factor tests. Repucci (1962*b*) has reported a correlation of .31 between a kind of DP test and a peer-rating criterion of creativity for research chemists.

The study of Welch (1946) mentioned earlier showed a very large difference between professional artists and college students on two *figural* DP tests but not on two *semantic* DP tests. Jones (1960) made a much more systematic study along the same lines. The population was composed of sixth-grade students. The criteria of creativity were based upon productions of drawings on the one hand and samples of writing, on the other. Several judges rated each child's best product out of three in each case. The purpose of the study was to determine whether figural versus semantic DP tests would show differential validity in relation to creative drawing versus writing productions. For both the drawing and the writing samples, three variables—creativeness, freedom in use of material, and amount of detail—were rated. The correlations of corresponding criterion qualities across the two kinds of products were: .25, .14, and −.03. This result can be interpreted as indicating a high degree of independence of creative performance in the two media, figural and semantic. It should not be surprising, then, that Brittain and Beittel (1961) and Beittel (1964) should find that semantic DP tests failed to discriminate creative-rated from noncreative-rated college students in art.

Using a composite of the creativity and freedom criterion ratings in each case, Jones found that verbal-test composites correlated with the writing criterion .46 (with four tests) and .58 (with six tests). A verbal composite correlated .32 with the drawing criterion. Figural-test composites correlated with the drawing criterion .50 (six tests) and .54 (seven tests). A figural composite correlated .40 with the writing criterion. Thus the differential prediction of figural and semantic creativity tests was not great, but it appears to be genuine.

The most striking validation data for semantic DP tests were recently reported by Elliott (1962). In two public-relations consulting organizations, top executives nominated their most and least creative personnel. In another sample of forty advertising copy writers, subjects were ranked in terms of creative output. In both of these populations single DP tests and combinations of them yielded correlations as high as .60 for predictive validity.

Considerably more data could be cited on predictive validity, which would also contribute to conclusions regarding construct validity, but enough has probably been reported to support claims that can be made of construct validity of the more creative factors and the tests that measure those factors. The next section will proceed on the assumption that the concepts developed through the multivariate approach of factor analysis are relevant for general theory of creativity and can be meaningfully integrated with concepts found through other approaches.

THE NATURE OF CREATIVE PRODUCTION

SOME TRADITIONAL THEORIES In this section we shall consider briefly some of the classical theories regarding the nature of creative production, and a new theory will be presented, putting creative thinking within the context of learning and problem solving. The roles of various psychological phenomena in stages of creative problem solving will be considered, as well as a number of conditions that may contribute to or detract from creative output.

Serendipity. One nontechnical theory of creative events which is of some consequence can be disposed of quite readily: the idea of serendipity. Supporters of this hypothesis make a great deal of the apparently accidental character of many important discoveries. There are many instances in which a man, usually a scientist, has made an important discovery of one phenomenon while actually looking for something else; Galvani discovering electric effects in living tissue; Oersted discovering the relation between electricity and magnetism; Claude Bernard discovering the nervous control of blood circulation; and Pavlov discovering the conditioned response. It may be that the thing discovered is not a new phenomenon but a new interpretation of a familiar phenomenon.

Undoubtedly, all such scientists are well prepared for the discoveries, and they may even have attacked the same problem, or similar problems, in the same area before. Writers generally agree that the apparent "luck" favors the person who is prepared for it (Cannon, 1940; McLean, 1941). Letting anything go as an "accident" is antiscience; it stops questions and investigations.

Psychoanalytic Theories. Psychoanalysts have had a great deal to say about the subject of creativity. Much of it is restricted to creative performances of visual artists and writers, particularly poets. It is almost entirely restricted to motivational and emotional aspects, with practically nothing on intellectual aspects, in line with the psychoanalysts' general tendency in dealing with psychological theory.

Although psychoanalysts agree that creative thinking is not conscious, and they are not alone in this respect, they are by no means in agreement on many other points. They do not associate creative thinking with the unconscious, for the latter is regarded as stereotyped and restricting. Creative thinking is a function of what they call the preconscious. There is relief from repression so that communication is improved between the id and the ego (Kris, 1953), permitting preconscious thought development.

To say that creative thinking is not conscious says nothing new and explains nothing. The problems of describing and comprehending the nature of that thinking still remain, and our observations must be very indirect and the conclusions highly inferential. Nor is unconscious thinking restricted to what is more commonly recognized as creative thinking. A very large proportion of all thinking is unconscious, in the sense that it cannot be directly observed and reported by the thinker himself. As Hadamard (1945) points out, in ordinary conversation, the sentence that comes out so glibly at one moment was organized a moment before unconsciously.

Creative production is often referred to as a regression to childish or primitive modes of thinking (Rosen, 1953; Maslow, 1958; Hilgard, 1959). This idea checks with the imaginative thinking of the typical child, free as he is from the hardening effects of increasing knowledge of "reality" and effects of cultural restraints, with their pressures for the child to grow up. Invoking the mechanism of regression may actually be gratuitous in many instances, for it should be possible for a child not to grow up with respect to his habits of thinking without remaining childlike in other respects. Under the latter condition, regression would be unnecessary.

Sublimation is often mentioned by psychoanalysts as playing roles in creative production, but there are disagreements as to the importance of this aspect and as to the role that it plays. Works of art are said to arise out of the personal conflicts of the artist. He can express himself in symbolic ways that relieve his guilt feelings (which many of those who enjoy his product share) without direct recognition of them (Grotjahn, 1957; Kris, 1953).

Psychoanalysts naturally have much to say regarding relations between creative thinking and pathology. For example, Kubie (1958) states that neuroses corrupt, mar, distort, and block creativeness. The two are therefore believed to be not positively related, generally. If the neurotic succeeds in producing art objects, his unconscious dominates, and the objects are expressions of his neurosis.

Creativity and High IQ. Whereas psychoanalysts have overemphasized the roles of motivation and conflict in creative performances, a number of psychologists have attributed creative performances to high IQ, at least implicitly. Studies of genius have generally regarded any outstanding intellectual performance, including creative productions, as being attributable to high IQ, where IQ is determined operationally by means of traditional intelligence tests. Current information decidedly fails to vindicate that assumption. With the development of tests of creative abilities it has been possible to test that idea.

Even before the advent of DP tests, as so recognized, some earlier investigators had found very low correlations between DP-type tests and IQ tests (Dearborn, 1898; Chassell, 1916; Andrews, 1930; Welch, 1946). Recent studies show the same kind of result quite dramatically. Getzels and Jackson (1961) found that selecting high-school students in the highest fifth in terms of IQ would miss about 70 per cent of the highest fifth on DP tests.

The writer has some data on 204 ninth-grade students, whose range in IQ was from 62 to 145, with a median of about 114. If one were to select gifted children on the basis of IQs above 130 (about 9 per cent were in that category) one would miss more than 80 per cent of students scoring in the highest 20 per cent on semantic DP tests. Among the selected students, above 130 in IQ, would be many with average and below-average DP scores. It would seem that a program for gifted students should give as much weight, at least, to DP status as to IQ status.

Table 11-1 summarizes correlational information, which shows strikingly low correlations. Some studies represented in the Table may have dealt with highly restricted ranges, but the school populations did not. The writer has obtained a large number of correlations, at the ninth-grade level, between single DP tests and mental age on several different IQ tests (Guilford, et al., 1961). The coefficients ranged from −.20 to +.52, with a mean of +.18.

It should be noted, however, that all the correlations cited pertain to linear models. The writer has plotted a number of scatter diagrams for some of the ninth-grade data, and he has found many triangular relationships. The regressions of IQ-test scores on DP-test scores have negative acceleration. Thus, although students with the highest IQ scores may scatter quite a bit over the range of DP scores, students with the highest DP scores are likely to be above average on IQ scores.

From this result one might draw the conclusion that high IQ is not a sufficient condition for high DP performance, but an above-average IQ is an almost necessary condition. It is commonly noted that outstandingly creative professional groups are high on IQ tests. This is to be expected, for a college education is almost essential for individuals to enter the professions. The members of those professions have therefore passed over some high academic hurdles.

With respect to children, who have yet to

TABLE 11-1

Some representative correlations between traditional intelligence-test scores and assessments of creative potential and performance

Torrance (1960*b*)	Elementary grades	Stanford-Binet	DP-test composite	.16, .17
		Otis		.32
		Kuhlman-Anderson		.26
		California TMM		.24
Torrance (1961*a*)	High school	Lorge-Thorndike	DP-test composite	.30
Torrance (1960*b*)	Graduate students	Miller Analogies	DP-test composite	−.02, .11
Torrance (1961*a*)	Graduate students	Ohio Psychol. Exam.	DP-test composite	.10
D. W. Taylor (1960)	Engineers	Terman Concept Mastery	Ratings	.20, .07
MacKinnon (1961)	Architects	Terman Concept Mastery	Ratings	−.08
	Scientists		Ratings	−.07

reach high educational hurdles, there is a possibility that the paucity of high-DP, low-IQ children is due to the fact that the DP tests are not adequate measures of DP performances of low-IQ individuals. This hypothesis is still to be tested. It should also be noted that the correlations cited thus far pertain to semantic abilities and verbal tests. It is possible that high *nonverbal* DP abilities may yet be found coupled with low verbal IQ. It may also be found that within a nonverbal area, for example, with figural tests only, a curved relation also holds between DP and figural cognitive abilities, by analogy to that found in the semantic category.

Spearman's Theory. Spearman (1931) one time proposed a special intellectual theory of creative performance. In his conception, every creative act is a matter of "educing correlates." A "correlate" is a "fundament," or unit of information, needed to complete a relationship, when the relation and another unit are given. The opposite of love is ________? The given relation is "opposite" and the given unit is "love." The required unit is "hate."

The reader will perhaps recognize that such an activity is what was earlier referred to as the convergent production of a semantic correlate. Thus, Spearman attempted to account for all of creative thinking on the basis of a single factor of intelligence; one, or, at most, four (for four kinds of content) out of a possible 120. He could well overemphasize the importance of this one kind of operation and product by virtue of the fact that he thought the cognition and use of relationships is the essence of the master ability he called g. We can actually place the Spearman concept in a very limited location within the structure-of-intellect model. Doing so merely emphasizes the serious limitation of his conception of creative thinking.

Thurstone's Theory. Thurstone tied his theory of creative thinking to his theory of intelligence, neither of which, curiously enough, he attempted to relate systematically to factor theory or to intellectual factors. Regarding intelligence, he stated "The psychological act originates in the essentially affective and non-verbalized, non-focal motivations and needs of the individual." (Thurstone, 1952, p. 33.) He went on to say that successive particularizations occur, involving successive decisions at choice points. When any choice point becomes focal in consciousness, there is rational control. The earlier the person becomes conscious of the process, the more intelligent he is.

Creativity fits into this picture in that some individuals, more than others, have contact with the prefocal, unconscious phases of the act. They become intuitively aware of information that is available to them from prefocal sources. This conception is apparently not far from that of the psychoanalyst's idea of a preconscious activity, through which the thinker is in closer touch with unconscious material. Thurstone was undoubtedly thinking of the process of intuition, concerning which much more will be said further on.

Associative Theory. It is natural that within the behavioristic climate in the United States, psychologists should attempt to find associative interpretations of thinking, including creative thinking; even in terms of stimulus-response associations. Two new proponents of a theory of the associative type have recently been Maltzman (1960) and Mednick (1962). Both evidently took their empirical starting point from the finding that a word-association test, with weighted scoring for statistical infrequencies of responses, is a measure of individual differences in the factor of originality (Wilson et al., 1954). Maltzman and his associates have applied this criterion of originality in a series of experiments on training subjects to be more original. These experiments will be mentioned in connection with development in a later section.

Mednick (1962) has elaborated a theory of originality, based upon the infrequency principle. He supposes that in connection with each stimulus word, an individual has a hierarchy of potential responses, each response with its own degree of readiness when the stimulus word comes. The degree of readiness can be expressed in terms of probability, grading off from the most ready response to the least. Some probability gradients are steeper, some flatter. The more original individual tends to have flatter gradients, hence he is relatively more likely to give associative responses of lower probability in his population.

On the basis of this theory, Mednick has developed what he calls the Remote Associates Test. To do an item successfully, the examinee must give associative responses that are likely to be at a low probability in almost anybody's hierarchy. Some sample items, but not from his test, will be given.

What word is associated with all three words?

fog	toad	shoe	Answer: horn
hook	story	cat	Answer: fish

The test is reported to have good predictive power (Mednick, 1962), but it is not known what factor or factors it represents. It probably represents more than one factor, but of course it does not begin to cover the many divergent-production and transformation abilities mentioned earlier.

Association theories are time honored, and there is no disputing of the fact that they have had considerable value. Any concept that could persist so perennially in various forms since the time of Aristotle must have been sufficiently reinforced to lead to the conviction of its scientific value. There is no question that it has served psychology well. But, as this writer has said elsewhere (Guilford, 1960*a*, 1961), to achieve an adequate theory it is necessary to go well beyond the association principle. He has proposed to substitute for it the six psychological products—units, classes, relations, systems, transformations, and implications.

The association principle has been made to serve generally as a first approximation to most of these products, excepting only perhaps units and transformations. Wundt and Titchener attempted to give even units associative interpretations but failed. The product coming nearest to simple, unadulterated association by contiguity is an implication. But in the context of products, implications are better interpreted as expectations, a concept favored by such psychologists as Tolman and Lashley. Implications are more than mere concatenations of items of information. At least implicitly, they carry the meaning of "If this, then that," very much as in logic. It is obvious that we can do considerably more in the way of theory with six varied concepts than we can with the single concept of association. If we attempt to get along with that single concept, we either apply it very loosely to a whole range of phenomena, or we have to ignore many of them.

The Wallas Stages of Creation. With no pretense that he was offering a scientific theory, Wallas (1926) proposed four stages or phases in the total sequence of events in a creative production. Wallas was merely attempting to tell others how to think more effectively and more creatively. He proposed breaking up the obviously complex and sometimes protracted affair that culminates in a finished invention into manageable segments, easier to handle.

Regardless of the modest way in which the stages of *preparation, incubation, illumination,* and *verification* were put forth, they have been taken more or less seriously, and some psychologists have taken steps to find empirical evidence regarding them, for example, Patrick (1935, 1937, 1938, 1941). There are certain aspects of the stage concepts that deserve serious study.

Empirical evidence concerning the 1-2-3-4 order of the stages show that they rarely run off in a clearly sequential manner (Patrick, 1935, 1937; Vinacke, 1952). There is much overlapping of events and much backtracking. Even Wallas did not insist that the sequence is clear-cut. It would be better, then, to speak of "phases," or "aspects" of creative production than "stages."

Rossman's Stages of Invention. While we are on the subject of typical sequences of events in creative production, it is of interest to note the list to which Rossman (1931) came, after studying more than 700 prolific inventors by means of questionnaires. The steps are:

1. Observation of a need or difficulty
2. Analysis of the need
3. Survey of all available information
4. Formulation of objective solutions
5. Critical analysis of solutions
6. The birth of the new idea—the invention proper
7. Experimentation to test out the idea

In terms of structure-of-intellect concepts, a striking thing about this list is the frequent implication of evaluative activity. This could be said of the alternate steps 1, 3, 5, and 7. One is reminded here of the suggestion of Miller, et al. (1960), that the unit of behavior is the TOTE sequence, where TOTE stands for a sequence of test (evaluate), operate, test, and then E, for exit. Rossman's complete unit of behavior for the inventor would be symbolized by TOTOTOTE, E being an eighth step. Of course, within each step there are smaller TOTE patterns of various lengths. The steps given are merely major things that must be accomplished, if an invention is to be consummated and tested.

A TRANSFER THEORY OF CREATIVE THINKING

SIMILARITY TO PROBLEM SOLVING IN GENERAL

If we are to understand any phenomenon, analysis of some kind is absolutely essential. There are many kinds of analysis, some better adapted to the understanding of certain phenomena and some less adequate. The kind of analysis just mentioned is very rough and rather superficial. It is very reminiscent of the listing of steps in problem solving. John Dewey (1910) proposed steps that stood alone for many years: felt difficulty, its location and definition, suggestions of possible solutions, consideration of consequences, and testing of the solution. More recently, D. M. Johnson (1955) has proposed that we get along with only three stages: preparation, production, and judgment. Merrifield, et al. (1962) return to a five-stage model with: preparation, analysis, production, verification, and reapplication. The "reapplication" phase was introduced to take care of the many instances of backtracking or return to earlier steps. There is little point in our attempting to evaluate these formulations or to reconcile them. The important implication, when comparing them with the Wallas and Rossman stages of creative production, is the marked similarity between steps in creative production and problem solving in general.

This similarity has by no means passed unnoticed. A number of writers have made comments to the effect that creative production *is* problem solving. If it appears that creators in the arts have no problems, psychoanalysts remind us that they have. Where other creative people, like scientists and engineers, find their problems in the environment, artists have personal problems, found in themselves, whether they recognize that they have problems or not. By recognizing that creative production and problem solving are made of similar cloth, we have the advantage of applying what we know about problem solving to the understanding of creative thinking, and vice versa.

Identifying creative production with problem solving means that there is something at least minimally creative about solutions to all problems. If this idea is difficult to accept, let us consider the nature of a problem. If a stimulus situation arises, to which the organism reacts without hesitation because it is fully prepared, there is no problem. But to the extent a stimulus situation catches the organism unprepared or the organism's prepared response proves inadequate, there is a problem. Some problems, to be sure, are solved almost instantly. But there is at least an element of novelty in every solution to a problem, either in the form of a new strategy or tactic that is formed by the organism or in the form of a new application of one previously acquired.

In the quickly solved problems, it is especially difficult to note the various steps that have been proposed, as listed above. The lists were obviously not made to describe such problem solving. They were made for complex problems that keep the problem solver from reaching an adopted solution for at least a matter of minutes, but more likely a matter of days and even weeks. We need pictures of problem solving that describe quick solutions to simple problems as well as protracted solutions to complex problems. The pictures of the two should agree fundamentally when all the events are known.

But even two quick solutions or two protracted ones are probably not alike in every respect. There is evidence from factor analysis (Guilford, 1960; Merrifield et al., 1962) that different kinds of problems called upon different weighted combinations of intellectual factor abilities, depending upon the nature of the problem and the strategy applied by the problem solver. This reemphasizes the importance of the basic intellectual abilities and the potential usefulness of their concepts in describing problem-solving behavior at different levels of complexity.

Let us proceed now in an attempt to elaborate upon the very sketchy stage pictures that have been offered to describe problem solving, particularly of the more obviously creative type. In everyday life, we recognize some instances of problem solving as being creative because there is something a bit dramatic about them. That "something dramatic" is in the form of highly novel solutions that probably come rather suddenly, in other words, in the form of insights.

MOTIVATION FOR CREATIVE THINKING People recognized as creative are not only more sensitive to problems, they seem to go out of their way to find problems, a fact that should catch the notice of theorists who overwork homeostatic principles of motivation. At this point we are crucially concerned with the problem of motivation. What is it that motivates creative people, and why do they like to solve problems, where solutions have no immediate utilitarian value? Can the primary drives of hunger, thirst, sex, and fear account for the problem seeker? Would secondary drives based upon them give a satisfactory account? Does problem seeking fit the general principle of drive reduction? Can the psychoanalyst give us any help when he attributes creative drive to the oral stage of development and says, in effect, that the creative thinker is attempting to get even with his mother for weaning him (Grotjahn, 1957)?

An Intellectual Drive. There has been a thread of thinking, not often vocal, which is partial to the idea that there is something like an intellectual drive. Some animal psychologists who see this sort of thing in exploratory behavior, toy with the idea of a curiosity drive. But others go further. Thorndike (1931) suggested that the organism's normal use of his equipment is satisfying and man, particularly, with higher-level intellectual equipment, has an urge to use that equipment; to function intellectually. White (1961) has recently treated this subject at some length. In simple terms, he supports the thesis that an organism desires to grow and develop, to master its environment by developing knowledge and skills, in other words, what he calls a drive for competence. Rogers (1962) finds motivation for creativity in man's tendency to try to "actualize" himself; to realize his potentialities; to achieve high accomplishment for its own sake.

The idea that problem solving can carry its own intrinsic rewards is supported by observations of productive inventors (Rossman, 1931). Many inventors spoke of the feeling of superiority and of exhilaration experienced with success. Such intense reinforcement leads inventors to seek new problems so that the happy experience may be repeated. Except for its lower intensity, this drive condition is reminiscent of drug addiction.

Suchman (1961) provides similar evidence from a different source. In his inquiry training, he teaches children how to ask good questions regarding physical phenomena that are demonstrated to them. By a line of questioning they arrive at a satisfying hypothesis to account for the phenomenon. The children are reported to find such intellectual exploration and mastery highly rewarding. In this connection, one is reminded of Wolfgang Köhler's apes, who after gaining insightful use of tools, showed a period of apparent enjoyment from just manipulating those tools rather than using them immediately to satisfy a hunger drive.

Secondary Sources of Satisfaction. It is sometimes said that leading creative people have a desire to benefit humanity; an altruistic motive. That some of them do have such a motive cannot be denied, but studies of scientists (Roe, 1952), inventors (Rossman, 1931), and other creative people fail to show that it is very common, at least as the mainspring of power. Rossman's inventors spoke of liking to improve things and of seeing needs, but rarely of desire to help humanity. Many creative producers no doubt take secondary satisfaction in knowing that what they do is beneficial. At the opposite pole are those who create devilish devices of torture and of destruction.

Although a drive for competence, development, and intellectual mastery may be posited as the main source of motivation of creative people, there are many secondary sources of satisfaction involved. Hadamard (1945) remarked that artists have the urge "to do something different," which does not also apply to mathematicians. The last part of this assertion, however, should probably be discounted. Houston and Mednick (1961) tested the hypothesis that highly original individuals have a strong preference for novel responses, as such, or an urge to avoid the trite and the banal. Their experimental results lent support to this idea.

Barron (1953*a*, 1958) has found that not only artists but also highly creative people in other professions express preferences for complex visual designs over simple, regular, and symmetrical designs. One interpretation of this is that the complex designs offer challenges to comprehend and to try to establish order. This interpretation brings us very close to the idea of a general drive for intellectual competence. With children, the relationship between preference for complexity and originality is not found (Kincaid, 1961; Burkhart, 1962). The preference for complexity apparently develops with age and experience. These findings suggest that the individual's preference is for a level of complexity that will offer opportunity to grow beyond his present abilities to comprehend, but not for complexity that goes too far beyond his ability to handle.

It is often noticed that creative people tend to be independent thinkers, to have their own values, and to be nonconformists in various ways. With the use of the Asch-Crutchfield conformity test, Crutchfield (1957) has shown that there is a negative correlation between originality and a tendency to conform to group judgment under social pressure. In the conformity test, the examinee hears four of his associates render what he thinks is an obviously wrong judgment, e.g., of lengths of lines, before he gives his own. The score is the number of times E succumbs to group "opinion."

Factors of Interest in Thinking. Some studies of thinking, interests and other motivational traits, and the relations of such traits to creative tendencies are pertinent to the general problem of motivation for creativity. Factor analysis shows that there are several dimensions in this area, including interest for: reflective thinking, logical thinking, autistic (versus realistic) thinking, convergent thinking, divergent thinking, and a factor of tolerance for ambiguity (Guilford, et al., 1961).

When scores for these factors and for other motivational factors are correlated with various DP tests, with young men as subjects, some small, but significant, correlations are found (Merrifield, et al., 1961). Associational-fluency tests correlate with need for adventure and tolerance of ambiguity. Expressional-fluency tests correlate with liking for reflective thinking. Originality tests correlate with liking for divergent thinking, tolerance for ambiguity, and liking for reflective thinking. Originality tests correlate negatively with need for meticulousness and need for discipline. Spontaneous-flexibility tests correlate positively with need for variety. Thus, motivation for intellectual activity, including creative thinking, appears to be quite complex. The preceding discussion has by no means exhausted the various potential contributors.

PREPARATION FOR CREATIVE PRODUCTION

Having paid attention to the motivating conditions for creative production, a subject that is slighted seriously by those who have proposed stages in that complex phenomenon, let us organize the next discussions in line with the Wallas steps: first, preparation.

The Importance of Information. Preparation is primarily an information-gathering phase. Although we may agree with Albert Einstein that imagination is more important than knowledge, we must admit that knowledge or information is a requisite for creative thinking. The information we use is either offered by the immediate environment, is sought after by us in the environment, or is in our memory storage.

E. L. Thorndike (1949) demonstrated dependence on stored information when he performed a rather simple but novel experiment. He presented to subjects a mixture of uncommon words and fictitious words, each to be defined, such as "apogee" and "illiberate." Thorndike's conclusion was that the invented definition came from remembered information. In this connection it should be emphasized that it is *information used in a new connection*. Information recalled for use in a new connection is a phenomenon of transfer, hence the reference to a transfer theory of creative thinking, mentioned above.

One serious difficulty with associative interpretations of creative thinking is the assumption, usually implicit, that nothing can be recalled in connection with another thing, unless the two were experienced contiguously before. In creative thinking it is easy to find examples showing that two things are brought into connection that in all probability have never been experienced together before. Incidentally, one can also find examples to show that things experienced in contiguity need not be remembered together. Thus, contiguity, the cornerstone condition of modern associationism, is neither a necessary nor a sufficient condition for either learning a connection or for the recall of one. The association theory of thinking could be bolstered a great deal by making greater use of Aristotle's principle of association by similarity. It is somewhat surprising that this principle has not been employed more, since it has been given a good behavioristic dress in the form of stimulus and response generalizations.

The phenomenon of transfer, as we know, has suffered many swings of psychological fortune and misfortune. From an early position known as "formal discipline," which envisaged very broad transfer events, psychologists generally swung almost to the opposite extreme of belief in only specific learnings early in this century. A moderate position is most consistent with the facts and with reasonable theory. Transfers are known to exist in learnings of many levels on the specificity-generality continuum. If we were to take our cue from factor theory, we should expect to find that almost every learning event has both specific and general aspects; aspects that are unique to the task and not transferrable, but also aspects that are transferrable, just as we find there are specific and common-factor components in performance on a task.

The structure of intellect, representing common factors of an intellectual nature, provides a systematic scheme on which to base expectations of transfers of some degree of generality. Similarity is the key to transfer. Intellectual tasks resemble one another in having a common kind of operation, of content, or of product. The more of these aspects in common, the greater the transfer. Tasks representing the same common factor or the same products should show the greatest amounts of transfer.

According to the writer's informational view of psychology (Guilford, 1961), information is the key concept. Learning is the acquisition of new information. This sounds commonsensical until we define information in a noncommonsensical manner. Information is defined as that which the organism discriminates. Discriminations are along the lines of the various products and kinds of products. The creative thinker generates new products or old products in new connections, hence, creative thinking, like all problem solving, is an instance of learning.

The Role of Systems. Among the kinds of products that a creative thinker generates, that of systems most often catches the attention of writers on creativity; perhaps because it is the most impressive and the most complex, and because the terminal product is in the form of a system. Systems are variously referred to as schema, themes, or motifs. Those who have studied creative producers assert that the creator develops a system early among the production stages. This is reported to be true of mathematicians (Hadamard, 1945), painters (Eindhoven and Vinacke, 1952), poets (Patrick, 1941; Arnheim, 1948), and composers (Cowell, 1926). Novelists outline their story plots and scientists organize a set of hypotheses to guide further steps, the details to be added later. Although the role of systems is most obvious, one should not overlook the roles of other products, such as transformations, which, from factorial results, appear to be an important secret of being original.

Recall of Information. The importance of information, and, in particular, the importance of stored information seems well indicated. Stored information is of no value in creative thinking, if we cannot retrieve it when and where it is needed. This involves us in the subject of recall of information; a seriously neglected subject, in view of its importance in connection with thinking in general. It is curious that so much psychological attention has been given to learning and so little, by contrast, to recall, a situation that should be expeditiously corrected.

Recall during problem solving is not like recall in a test of how much learned material a subject remembers. In the latter instance, recall is intended to be in just the way the information was learned. In problem solving, chances are that recall is to be in connection with things with which it was *not* learned. There must be transfer recall. How is it that the information we want can be retrieved out of its learned context? This is the significant question for understanding the role of recall in problem solving.

The concept of "search model" has been invoked for use at this point (Duncker, 1945). It is a promising idea. A search model arises from the nature of the perceived problem or perceived goal to be reached when the problem is solved. The solution-to-be carries specifications of the information wanted. It can be any kind of psychological product, from units to implications. The specifications of the search model somehow act as an instigator of arousal of the needed information. Items of information congruent with the search model are most likely to be aroused. Principles of congruency are very much needed in order to complete our psychological picture of transfer recall. The information aroused is then evaluated by comparing it with the search model, then accepted or rejected. From all this, it appears that if we are to improve creative thinking, one of the places at which we could make progress would be to improve efficiency of recall of information for new and unusual uses.

THE ROLE OF INCUBATION The phase known as incubation and its apparent benefits has been one of the major mysteries regarding creative production. Periods of incubation can be of varied length, from a minute or so up to days, months, or years. It is presumed that the thinker, although stopping work on his problem, keeps up his desire to solve it, with an intention to come back to it. Even without this intention, the problem may come back to him, as if by a kind of perseveration. The striking fact about a period of incubation, during which no progress toward solution is observable, is that when attention returns to the problem, there are often signs of progress without having worked on the problem. Patrick (1935, 1938), who had subjects work on poems and scientific problems in the laboratory, could observe signs of incubation when a subject would drop one idea or one method of attack for a while and then take it up again later.

Hypotheses Concerning Incubation. A number of hypotheses have been suggested to account for the phenomena of incubation. Poincaré (1913) was one of the first to suggest that unconscious problem solving is going on during the interim. The psychoanalysts share this view, of course, except they call it preconscious activity. It would be natural to expect a fatigue hypothesis to the effect that the thinker stops work because he is tired, obtains rest during the pause, and comes back to the task rested. The fatigue postulated must be rather specific to the strategy employed, however, for the thinker often turns to something else just as tiring during the "rest" interval.

Some writers call attention to the fact that taking up the problem at a later time more readily favors a new start, a new set, or a new strategy (Patrick, 1935). Woodworth (1938) favored the hypothesis of fading interferences. Wrong or useless steps are often very persistent in continued work on a problem. Because of their recency value they remain strong, interfering with thinking of possible alternatives. During the incubation period, such steps are forgotten, losing their interference value.

Incubation and Spaced Learning. It will be recognized that most of these hypotheses have also been suggested to account for the phenomena associated with spaced practice and learning. It may be that what accounts for phenomena in the one case also explains, to a large extent, the phenomena in the other. Experiments on incubation are of course more difficult than common experiments on spaced practice, owing to the greater lapsed time with greater loss of control of the experimental subject during the interval. Patrick's (1938) attempt is a rare instance of research on incubation. Half of her subjects worked on planning experiments in the laboratory and half at home, with lapsed time for incubation. On the average, the latter group produced better ideas.

THE ROLE OF INSIGHT OR INTUITION *Emotional Aspects.* The striking feature of the stage of illumination is the moment of the "flash of genius," which is recognized by psychologists as an insight; some say "intuition." Some of the casual descriptions of the event mention emotional accompaniments, sometimes with implications that emotion has something to do with the successful emergence of the idea. Patrick (1937) reported that professional and nonprofessional painters during the course of painting in the laboratory sometimes experienced emotional stirring along with insight, sometimes not. Rogers (1962) speaks of an "Eureka feeling," also of a feeling of anxiety, and a desire to communicate to others.

The degree of emotion or feeling occurring with illumination probably depends upon several circumstances; the strength of motivation of the creator, the amount of frustration he has endured, the size of the "intuitive leap," and the apparent importance of the outcome. The relation of emotional aspects to the *success* of the thinker is unknown.

The Nature of Insight. There is little doubt regarding the suddenness of the arrival of ideas, however, and some relatively large leaps, even when the creation is not complete. Maier (1931) asked sixty-one student subjects to give four different solutions to his famous string problem.[4] He reported that the solutions came as wholes in 75 per cent of the cases. Rarely can conscious steps be caught by the self-observer immediately preceding the full-blown insight. The solution emerges like the hidden figure that jumps out at the observer of a picture. There is a suggestion here of a kind of discontinuity or quantum principle.

Because of the unconscious nature of whatever activity immediately precedes the insight, some writers are likely to conclude that the process of insight is indescribable and unknowable (Rogers, 1962). But to the scientist, this state of affairs should be a real challenge. It could well have been said at one time that the inside of the atom is also indescribable and unknowable. No such thought has stopped the atomic physicist; it should not stop the investigating psychologist.

As a matter of fact, quite a bit of experimental research has been done on this problem, and it is continuing. Some of this research pertains to determining what kinds of mental activity can be done unconsciously, and some of it pertains to the conditions under which insights occur. For example, one kind of experiment demonstrates how far individuals can go toward the formation of a class without awareness of the class concept. The results show that the subjects have an unconscious appreciation of the class, for they make correct classifications beyond chance levels of success before they are aware of the class concept (Pickford, 1938; Snapper, 1956).

---

[4] For a brief description of the Maier test, see p. 443.

Other experiments (Westcott, 1961, 1962) give attention to the willingness of individuals to resort to intuitive solutions on the basis of limited information. Westcott has followed the principle of feeding information to the subject (*S*) clue by clue, until *S* demands no more and reports a solution. The following item is a sample of the problems used, with successive clues:

4:2 9:3 25:5 100:10 64:8 16:__

The number of clues S demands in this and similar problems is a "demand score." The number of correct intuitive solutions is a "success score." The ratio of success score to demand score is an index of efficiency of intuition (Westcott and Ranzoni, 1963). The correlation between the demand and success scores is essentially zero, while there are substantial to high reliabilities for the two basic scores. Ss are therefore found who combine high and low scores for demand with either high or low scores for success, making four combinations, each with notably different personality traits.

Conditions Affecting Insight. Some experiments on the conditions affecting insight have given attention to the role of previous experience. This is an important problem theoretically, because some psychologists with associationist bent are too ready to dismiss insight as completely explainable on the basis of previous learning, by which they probably mean a direct dependence upon learned associations that are revived in the insight situation (McGeoch and Irion, 1952). In holding such a view, there seems to be a self-blinding to the fact that there is at least something new that happens in an insight.

Birch (1945*b*) did a transfer experiment, in which six young chimpanzees were first tested to see whether they could use a hoe to bring in food from a distance but within reach. Two succeeded. All were given training in hauling in things using plain sticks and were retested on the hoe problem, whereupon all succeeded very quickly. This experiment shows two things; that practice with plain sticks transfers to the use of a hoe, but it also shows that two apes could develop insight into the use of the hoe without the training with sticks. Associationists may say, "Yes, but the two chimpanzees had had other prior incidental experiences, which accounted for their relatively early insights." Possibly so; and possibly they had experiences that the other four did not have, although this is not likely. There still remains the fact that the residues of previous experience did not in themselves contain all that was needed to effect insights, with or without the special training.

With graduate student Ss, Maier (1930) first taught separately three principles that would be needed later in solving a complex problem involving all three. In five different groups of Ss he presented the complex problem, giving different numbers of hints in different groups. Only nine of the eighty-four Ss got the correct solution, eight being in the group receiving the most hints. Thus, past experience of the kind that Maier gave was by no means sufficient to ensure a solution. Something else was necessary. One of these additional aids was in the form of hints that invited the attention of some Ss in favorable directions.

Birch (1945*a*) studied the effect of level of motivation upon the incidence of insight. Six young chimpanzees solved a number of problems requiring some degree of insight, with six different levels of motivation, controlled by number of hours of food deprivation: 2, 6, 12, 24, 36, and 48 hours, respectively. Results with the string problems were typical. After 2, 36, and 48 hours without food, the results were no better than chance. The best scores were obtained at 6 and 24 hours. The absence of 12 hours from the second list was explained by the fact that a period of sleep occurred during the 12-hour delay, no meal actually having been skipped. At the shorter intervals of delay there is lack of interest in food, and S is easily distracted. At 36 and 48-hour delays drive is so strong that S pays too much attention to the goal and not enough to the means. Frustration, screaming, and tantrums also hindered performance.

Some observations lead to the hypothesis that the human subject's mental disposition at the time of illumination may also have an important bearing. Poincaré (1913) reported that new mathematical ideas came to him while in a state of mental relaxation or distraction, as when boarding a bus or taking a walk. Kris (1953) remarks that the person ready for insight shifts into a more primitive, dream-like type of thinking in which there is more freedom. It has long been known that relaxation is a favorable condition for recall. The dreamlike, dispersed type of attention may mean that there is a relaxation from a restrictive use of a search model, resulting in a broadening of the scanning among stored items of information. The wider search may favor more remote transfers. These are also hypotheses that can be investigated.

THE ROLE OF FLUENCY Fluency tests call upon the examinee to bring forth from his memory storage as many selected items of information as he can in a short time. The scoring emphasizes quantity of production and ignores quality, without tolerating irrelevancy. The criterion of relevancy is largely the distinguishing mark between the outpourings of a manic and the listings of the nonpathological examinee. There are other symptoms of pathology, of course, in addition to irrelevancy.

Such leading creative figures as Sir Winston Churchill and Thomas A. Edison were said to be unusually high on rate of production of ideas. Observers have remarked about the speed with which they could come out with one idea after another when faced with a problem (Thurstone, 1952). Quantity of production obviously pays off when it is also high-quality production. What are the relations between quantity and quality of idea production?

Quantity and Quality of Production. The answer is not a simple one, for there are several ways in which the relationship can be conceived. The Buffalo group (Parnes and Meadow, 1959; Osborn, 1962; Parnes, 1961*a*) hold to the principle that "quantity breeds quality." Osborn, for example, asserts that individuals who think up twice as many ideas produce more than twice as many good ideas, where "good" means unusual and useful. Since high-quality scores often measure the factor of originality, Osborn's principle should lead us to expect positive correlations between ideational-fluency tests and originality tests. Inspections of such correlations from a few of the studies involving these two factors at the University of Southern California reveals very small correlations, mostly below .20, but nearly all positive. There is this much support for the principle.

Hyman (1960), however, found contrary results. Two instructions were given at different times: to produce as many ideas as possible and to list only creative ideas. The subjects produced 68 per cent more ideas under the quantity instructions, but the number of "good" ideas was the same under the two instructions. For Hyman, "good" meant uncommon and of high quality. Hyman offers the hypothesis that producing low-quality ideas stands in the way of producing good ideas, as if one has to get them out of the way first when they are allowed. He also points to a difference between his kinds of problems and those used by Parnes and Meadow (1959). The latter experimenters employed an unusual-uses problem (list unusual uses for a wire coat hanger, for example), whereas Hyman's problems called for hypotheses and solutions. As Hyman suggests, the correlation between numbers of low-quality and high-quality responses may depend upon the nature of the problem, as well as upon the instruction.

One way of ensuring that quantity will breed quality is to allow a longer working time, for there is a rather consistent tendency for later-produced responses in a working period to be of higher quality. This hypothesis was tested by using tasks scored for the numbers of uncommon responses and the numbers of remote responses (Christensen, et al., 1957) and found to be supported in every case. When the criterion of originality was "cleverness," however, it was found that the average degree of cleverness as rated remained essentially constant over a working period of twelve minutes. Parnes (1961*a*) has found a tendency for responses in second halves of lists given in five minutes and also in fifteen minutes, with trained and untrained Ss, to be of higher quality. The task was to give unusual uses. High quality was in terms of criteria of uncommonness and usefulness.

Some Principles of Recall. Tests of verbal fluency are essentially tasks involving rapid recall of members of a class of items of information. It should therefore pay us to give some attention to experiments on recall, particularly those of Bousfield and his associates. When subjects are told to list names of four-footed animals or makes of automobiles, the task is similar to ideational-fluency tests, with one significant difference. Naming familiar objects of a class involves more direct recall, utilizing learned connections. Writing titles for a story plot, for example, involves much more in the way of transfer or indirect recall of information.

The Bousfield experiments (Bousfield et al., 1954) find that the cumulative output curve is negatively accelerated, approaching an asymptote. It must do so, because the number of stored and available items of the typical class is finite and sometimes not very large. A study of the production curves for items used in testing ideational fluency, on the other hand, yielded curves that are approximately linear, with little or no sign of approaching asymptotes, at least within periods of twelve to eighteen minutes (Christensen et al., 1957). This result may reflect the fact that the potential supply of such responses is very great.

But an alternative hypothesis can be suggested, which might apply to direct recall also. In both instances, it may be assumed that each item of information to be produced has its own level of "difficulty." For a direct-recall task, the frequency distribution of such items on a difficulty continuum is likely to be positively skewed, with a relatively large number of items being easy to revive almost immediately and a smaller number spread out toward the most difficult levels. Items of information given in an ideational-fluency test like Plot Titles, however, may have a distribution that approaches rectangular form, hence the rate of production is uniform.

A certain principle of recall found by Bousfield and his associates (1954) helps to understand a phenomenon frequently found in intercorrelations of tests (e.g., Guilford et al., 1961). This phenomenon is a slight, but somewhat consistent, negative correlation between tests of ideational fluency and vocabulary tests. This general finding indicates that the more verbal ideas we have in our possession the less efficient we are in getting them out. The Bousfield result is that the larger the supply within a class, the lower the rate with which that supply is exhausted.

The size of supply and the rate of exhaustion of supply are estimated from the equation

$$n = c^2mt/(1 + cmt)$$

where n = number of responses given by time t (in min.)
c = number of items in the supply
m = a rate index

The correlation between constants c and m is usually negative, in line with the principle stated above. It is undoubtedly a stronger negative correlation than we find between IF tests and vocabulary tests, for in the latter case we do not have a measure limited to the supply in a particular category. But since the supply available for indirect use is relatively large, it may well be positively correlated with size of total vocabulary.

Bousfield et al. interpret the negative correlation between m and c as meaning that, as the supply c increases, the number of interferences to recall also increases, thereby reducing rate. The rate index should logically be a measure of ideational fluency, and it could possibly provide an improved measurement of that ability, but it is time consuming to compute. The score that is ordinarily used is n, which is a function of both c and m. It is possible, however, that in the recall of information for new uses, rather than for direct use (as learned), some other kind of functional relationship would apply relating n to m.

THE ROLE OF FLEXIBILITY It was indicated earlier that factor-analytical studies had revealed three distinct types of rigidity in thinking. We shall now examine those concepts in more detail.

Flexibility of Classes. The factor first called spontaneous flexibility is now conceived as a facility in shifting from one class to another in thinking. Tests of perseveration within classes have been shown to be strongly negatively related to the factor (Frick et al., 1959), a verification of the present placement of this factor in the structure of intellect. The factor is not the same as Spearman's factor of perseveration, however, for he regarded his factor P as completely general.

Other writers have evidently sensed the importance of this kind of flexibility for creative thinking, using different terminology. For example, in teaching others how to think creatively, Gordon (1961) emphasizes the importance of approaching a problem at a high level of abstraction. This could be interpreted to mean that the thinker starts to work within a much larger, more general class of ideas. Such a mental set should broaden considerably the scanning of stored information in search for understandings and solutions. Starting within a narrower class would possibly exclude information that is needed.

A concrete example of this approach to problems is provided by Arnold (1962*b*). Suppose you decide that you want a new and better printing press. The largest possible category within which to put the goal idea would be: devices for producing multiple copies of permanent visual records in readable form; a highly abstract definition of a concrete device. Four dimensions are then recognized: (1) carrier of information, (2) method of information transfer, (3) visual rendering, and (4) production of multiple copies. This breakdown provides four generalized search models. The next step would be to list all the variations possible in each category. These can be combined in all possible ways, yielding a very large number of possible devices, a printing press being only one such combination. Anne Roe (1952) may have been pointing out the same breadth-of-scanning tendency when she remarked that the top scientists she studied seem to be generalizers.

Too much familiarity with an object can work against a broader view. Reference to spontaneous flexibility earlier mentioned the "disease" of hardening of the categories. Arnold (1962*a*) cites an example of this. Employees of the AC Sparkplug Company had difficulty in listing the attributes of a sparkplug, whereas they had little difficulty in listing attributes of a bicycle. Employees making heavy machinery, on the other hand, had little difficulty in listing attributes of a sparkplug. It is thus difficult to become abstract regarding very familiar things. It is said that Einstein made a practice of denying his understanding of the obvious, in order to obtain new looks at things. It is also noted that an amateur or tyro can sometimes be first to see a new idea for an invention among a group of experts in the field. It pays to stand back and take new looks at things; to reclassify them. This is spontaneous flexibility.

Flexibility in the Form of Transformations. The factor of adaptive flexibility, first found in Match Problems and similar tests, is now recognized as the ability to produce figural transformations. It was originally thought to represent readiness to change strategy, a kind of habit-breaking disposition. But the changes in strategy in the Match Problems test may be a function of readiness to look at lines differently; to redefine them, to reinterpret their functions in the act of divergent thinking. Tests of an inclination to persist in an attack in one direction correlate negatively with the factor. But, alternatively, this persistence may merely represent resistance to transformation.

The last-mentioned characteristic suggests that the factor is at the opposite pole of the kind of rigidity that Luchins (1942) has investigated so extensively (Luchins and Luchins, 1959). The task involved is the famous Luchins Water Jars test, in which the first five problems, all solvable by the same formula, are expected to set up a habitual strategy. In later problems, the subject has to break the habit if he is to succeed. A group form of the Luchins test was used in a study in the Aptitudes Project at USC (Frick et al., 1959). Analysis showed a loading of only .18 on the factor of adaptive flexibility. But it must be remembered that this factor is in the figural category, whereas the Luchins test is more reasonably classed as a symbolic task. In the study just mentioned, the test's loading was −.09 on originality, the parallel semantic factor, so the Luchins test is apparently not a measure of semantic adaptive flexibility. There is some question whether the group form used has the same factor composition as Luchins's individual form. Until the possibility of a relationship to a still unknown factor DST (divergent production of symbolic transformations) is eliminated, the hypothesis that Luchins's rigidity is opposite to adaptive flexibility (symbolic) is still alive.

Factors of Redefinition and Functional Fixedness. The third type of flexibility in thinking that has been isolated by factor analysis was called "redefinition," and it is identified in the structure of intellect as convergent production of transformations. It would perhaps be better to say transformations in convergent production. From all appearances, these redefinition factors (three are known—figural, symbolic, and semantic) also represent what is commonly called "functional fixedness" as an opposite to redefinition.

The traditional task used in research on functional fixedness is Maier's ingenious string problem. Briefly, in this problem, two long strings are hung from the ceiling, each by itself within reach of the experimental subject but so far apart that he cannot bring them together to tie them, which he is told to do. The solution is to take a small object, such as pliers that are lying idly by, make a pendulum with this tied to one of the strings and catch the latter on the fly while holding the other string. The transformation required is a redefinition of pliers as a tool to pliers as a weight in a pendulum. That the rigidity of functional fixedness is different from Luchins's *Einstellung* effect was shown by Adamson and Taylor (1954), who found only a small and insignificant correlation between scores from the Maier and Luchins tests.

A favored type of experiment recently has been on the effect of immediately preceding experience upon functional fixedness (FF). These experiments give some subjects prior tasks in which they employ the to-be-redefined object in the usual manner, e.g., to construct electric circuits using a relay in one group of subjects and using a switch in another. A third group has neither experience (Birch and Rabinowitz, 1961). There is no doubt that using an object in the usual way just prior to the need for redefinition inhibits redefinition. All ten of the Ss having experience with the relay chose the switch to make the pendulum; seven of the nine with switch experience chose the relay; the control group was evenly divided in their choices.

In another study (Flavell et al., 1959) one group of twenty-four Ss had a prior experience of the usual use with a switch, and four other groups of the same size had experience with from one to four *unusual* uses with a switch. In the test later, the numbers who chose the switch were 3, 8, 9, 13, and 12, for the five groups, respectively. It appears not to take much experience with unusual uses to overcome the FF effect. Maltzman, Brooks, et al. (1958) found that experience in unusual uses of a string, a screwdriver, and balsa wood, was followed by less FF behavior in solving the subsequent string problem. Bond (1955) found that it is only necessary for S to observe unusual uses of an object to help prevent the FF type of response.

Adamson and Taylor (1954) also tested the hypothesis that effects of training favoring the FF effect would reduce with lapse of time. The time period between training (usual use with switch or relay) and the FF test was varied from one minute to one week. The percentages *not* employing the previously used object were: at 1 minute, 70; 30 minutes, 70; 1 hour, 65; 1 day, 53; and 1 week, 50 per cent. The FF effect due to training in habitual uses, therefore, can wear off within a day's time.

Johnson (1962) has demonstrated that the greater the degree of embeddedness of the object the greater the functional fixedness. He worked with concealed words, hence the subjects were dealing with symbolic information. Three degrees of embeddedness were represented by the following conditions: (1) single words presented for S to judge for pleasantness versus unpleasantness; (2) words in phrases, also to be so judged; and (3) words in sentences. The following task was to build anagrams, in which the previously exposed words could be used. From the first to the third condition, the Ss did increasingly less well in using the pre-exposed words. In a minor variation, words that could be used in the anagrams test were incorporated in the instructions that were printed and exposed continuously during the anagrams task. Ss did not use this exposed information.

THE ROLE OF EVALUATION Wallas's stage of verification is reputedly one of both evaluation and elaboration. It was intimated earlier that elaboration is more characteristic of later time periods in the completion of a product of some degree of complexity, the thinker having worked from a skeleton-like system developed earlier. There is nothing to prohibit some elaboration earlier, as revisions are made in the general scheme. It was also more clearly pointed out that evaluation can come at any time and probably does. But there is much evidence that it is better to suspend judgment during the idea-generating operations, and perhaps experienced creators follow this practice, wittingly or unwittingly.

Suspended Judgment. One of the outstanding features of Osborn's (1957) brain-storming method is its suspension of all evaluation during the idea-generating sessions of group thinking. In an experiment by Meadow, et al. (1959), one group generated ideas with suspended judgment and another without. Not only that, the latter individuals were given penalties for any except high-quality ideas. The problems were to think of unusual uses for a wire coat hanger and for a broom. With solutions weighted for quality (unique and useful), the first group (suspended judgment) produced an average of 7.9 ideas, the other group only 3.9. With the two instructions alternated in the two problems, the group having the suspended-judgment condition first did better under the other instructions second.

Effects of Evaluative Attitudes. Idea generation is evidently better after one reads previous-research reports with a constructive attitude than with a critical, faultfinding attitude (Torrance, 1959*a*). Two groups of forty-nine graduate students presented new ideas for research after reading with the two attitudes. The number of high-quality ideas produced were seventy-five and eighty, for critical and constructive attitudes, respectively. Graduate students who read with sets: (1) to retain, (2) to evaluate, and (3) to improve upon, were given examinations on the material with four kinds of test items: (*a*) cognitive (multiple choice), (*b*) memory (completion), (*c*) evaluative, and (*d*) creative (new applications). The creative set led to the highest mean on creative items; the evaluative set to highest mean on evaluative items; and the memory set to the highest mean on cognitive and memory items.

The advantages in terms of generating ideas of high quality are not always on the side of suspension of judgment. Christensen, et al. (1957) gave the Plot Titles test to some groups without any mention of cleverness and in others with the explicit instruction to list clever responses. It may be presumed that under the second type of instructions the examinees impose some degree of censorship, i.e., evaluation. The result is clear. Under instruction to be clever, the total quantity of responses decreased, the total number of clever responses increased, and also the average degree of rated cleverness. There can be little doubt concerning the last two conclusions. But as to the first conclusion, regarding quantity, there is some uncertainty. We do not know how many ideas *E* generated but discarded. As a matter of fact, the same state of experimenter ignorance applies under the instruction without reference to cleverness.

Similar effects of criticism were found by Weisskopf-Joelson and Eliseo (1961). Groups "brainstormed" new names for a cigar, a deodorant, and an automobile, half with instructions to be critical, half not. The noncritical groups gave higher quantity, but the critical groups gave a higher proportion of high-quality responses. This kind of result may develop with age of the individual, for Torrance (1962*b*) found that instructions to give "clever, unusual, and original" responses tended to increase quantity of production at the fourth-grade level, but to decrease it slightly above that grade.

SOCIAL CONDITIONS AFFECTING CREATIVE PERFORMANCE

Two of the most prominent special methods for training individuals to think creatively and to solve problems involving novel solutions are conduced in the form of group thinking; (1) the brainstorming method of Alex F. Osborn (1957) and (2) the synectics method of Wm. J. J. Gordon (1961). All individuals, including the creative ones and those who could be or who try to be creative, live in a social milieu, which must necessarily have effects upon them as well as their affecting others within their reach. For these two reasons, we are faced with problems of effects of social conditions upon creative thinking.

GROUP THINKING *Group Creativity versus Individual Creativity.* Gordon (1961, p. 10) maintains that group thinking is always superior to individual thinking. He cites a number of logically based advantages. He claims that a group can condense into a few hours the production that might take one individual several months. Groups encourage "irrational thinking," which is desirable during the idea-gathering stages of problem solving. Groups encourage daring, invite competition, and broaden the scope of the search for ideas. All these assertions have the status of hypotheses that should invite the attention of social psychologists.

D. W. Taylor et al. (1958) provide some negative evidence. They found that arbitrarily constituted sets of four individuals, who solved problems individually under the condition of suspended judgment, produced more unrepeated ideas than actual groups of four working under the usual brainstorming conditions. Osborn's claim has been that one value of the group is interpersonal stimulation, one person's idea suggesting another. When one person's idea suggests an idea to another person in the group, the latter is called a "hitch-hiking idea." It is said that about a third of the ideas coming out of a group may be of this kind. A danger pointed out by Taylor et al., however, is that one person helps to channel the thinking of others, which may prevent matching the scope attained by individuals working alone.

In connection with the Taylor experiment, it should be remembered that the lone thinkers worked under the instruction for suspended judgment. The experiment was not therefore a test of the brainstorming method, as such, compared with the usual types of individual thinking, where judgment is not suspended. By applying suspended judgment in both group and individual thinking, however, the Taylor experiment comes nearer to answering the question of superiority in group versus individual thinking, under the suspended-judgment condition.

In connection with experiments on group versus individual thinking, many conditions must be considered and tested. The advantages are probably not all one-sided, and it may well be that each has its better places. It is characteristic of highly creative persons that they are independent thinkers, with their own values, and they are less sociable than the average. These traits are reported by many writers. Such traits do not suggest that highly creative persons would seek group creative activity, certainly in the arts and in science, except after the major insight has been achieved and there is a felt need to communicate. At that time, of course, the need for idea generation is largely past. It may be that persons of lesser creative talents would profit much more by thinking in a group, and that this would be more true where the problem is of a kind that lends itself to a kind of committee approach.

Homogeneous versus Heterogeneous Groups. Experiences have shown something about how groups of problem solvers should be selected, organized, and operated to achieve optimal results. The psychological principles involved have not received much attention. In the context of elementary education, Torrance (1961*b*) has investigated the question of whether homogeneous or heterogeneous groups are better in producing ideas. The two kinds of groups were formed on the basis of IQ and also divergent-production tests.

Homogeneous groups proved to be advantageous from several points of view. There were more signs of stress in heterogeneous groups, as shown by behavior signs. The less creative child became more productive in a homogeneous group, where he could compete on more equal terms. Members of such groups expressed more enjoyment with the task (developing explanations for a scientific toy). The less able children expressed more self-confidence and self-esteem in homogeneous groups, and the more able children were more modest and self-effacing. The child with the highest IQ in a heterogeneous group was expected to produce ideas, and when he failed, he felt that he had let the group down. From these experiments there is much food for thought for those considering how to group children in educational practice.

SOME SPECIAL SOCIAL CONDITIONS In addition to the conditions incident to group thinking, there are many others impinging on the individual from the formal and casual groups in which he finds himself. There are conditions that both help and hinder the creative thinker. For example, there is evidence that a competitive situation can have a facilitating effect on both quality and quantity of production (Torrance, et al., 1960*c*; Torrance, 1962*a*). The range and limits of other conditions under which competitive atmosphere will be helpful are not known.

Imposed Evaluation. Other important socially applied conditions in criticism or, more broadly expressed, evaluation, are evaluation by leaders and evaluation by peers. In one experiment (Torrance, et al., 1960*c*) the peers in one group of elementary-school children pointed out defects in individual production (destructive criticism) and in another group they made suggestions for improvement (constructive criticism). The latter type of evaluation tended to produce better results, especially in grades four to six. Children in kindergarten and grades one and two, however, were sometimes helped more by destructive criticism.

In another experiment by the same investigators, criticism of either kind was followed by poorer productive thinking. The task was to demonstrate how a scientific toy works. In a third experiment, in which a variation in the form of no evaluation versus contribution of remarks designed to increase originality and elaboration was applied, it was found that the unevaluated group did better, except in grade 6. It would appear that the condition of suspended judgment, even when judgment is favorable and applied by peers, is better for children as well as for adults. It is possible that the comments direct the child's attention to self-evaluation, which is not a condition of suspended judgment.

Group Restrictions. In the experiments just discussed, evaluation was injected by instruction from an experimenter. Spontaneous peer evaluations and other treatments of the outstandingly creative group member are a different matter and provide some illuminating pictures of group dynamics. Torrance and his associates (1960*c*) did a series of experiments with groups, each composed of five elementary-school children, one of whom was assigned to the group because of his higher score on DP tests. The purpose was to see what this most-creative child and the other children would do. Would he show his superiority in idea production, and would the others accept his ideas? The task was to develop a way to demonstrate and to explain the principles involved in a scientific toy.

There was clear evidence of superiority of the high-DP members. Of the twenty-five, 70 per cent initiated more ideas than other members of the groups. There was also clear evidence that the other children resented the most-creative member, and only about 25 per cent of such members were recognized as making the most valuable contributions. The typical group developed sanctions in an effort to control the creative child. The sanctions and methods of control took the form of openly expressed hostility, criticism, rejection, and ignoring. In the higher grades there was use of organizational machinery, for example, electing the creative child to an administrative position or making him a recorder, with paper work.

The creative child's counteractions are also interesting. A few children were compliant, going along with the wishes of the group. Others showed counteraggression, indomitable persistence in spite of everything, ignoring of criticism, or clowning, the latter as if to distract attention from ideas and to gain appreciation for a more accepted exhibition. Others went off and worked alone, a reaction that decreased as a function of age. Some became apathetic, were silent and preoccupied. Some fluctuated in performance, and still others forswore their intellectual leadership and offered minor aids to others.

The resemblance to adult behavior is most striking. Parents, teachers, and adult peers feel threatened by the exhibition of superior creative production, or they do not understand it and do not know how to evaluate it, for it often fails to fit customary patterns. The creative person who knows or senses that he is creative likes to express himself and to display his productions for others to see. He is on the self-assertive side, consequently his behavior gives an appearance of aggressiveness. He is often rebuffed and frustrated and shows counteraggression. Many writers remark that these qualities tend to go with the more creative personalities. Torrance (1962*a*) reports that children sometimes have some awareness of this social situation and show it in stories they write.

Self-imposed Restrictions. Torrance (1962*a*) lists many social conditions that tend to inhibit full creative development of children, most of which cannot be given space here. Some of the inhibitions are self-imposed in order to achieve other goals. The inculcation of sex roles, for one thing, works against development of creativity. The boy must become a he-man, which excludes all things "sissy" or feminine. If the boy takes these pressures seriously, or if he cannot swim against the current, he closes avenues toward creative occupations. The girl is pressured to become compliant and dependent. She must not show independent thinking or consider going into the more masculine professions.

It is curious, however, that with the less masculine vocations in the arts open to women, the greatest artists have been men. Sex differences in DP tests are very small and often not statistically significant. The writer and his associates (Guilford et al., 1961) found small but consistent differences in DP tests at the ninth-grade level. There was a tendency for girls to have higher means on tests of fluency but for boys to have higher means on tests of flexibility. From this result one might expect the so-called weaker sex to be a bit long, relatively, on quantity but a bit short on quality of idea production.

Outstandingly creative adults seem to have escaped to some extent the pressure toward adopting sex roles, for creative males score more feminine and creative females score more masculine on masculinity-femininity scales, such as those in the MMPI and the Strong Vocational Interest Blank. Whether such dispositions make the individuals more creative is a question. It can be said that had they taken their sex roles more seriously, they probably would have gone into other occupations.

Pressures other than the sex-role ideal also put dampers on creative development. The child is faced with norms; height norms, weight norms, age-grade norms, and aptitude-achievement norms. Too much attention to norms arouses fears of being different, which is too readily interpreted as abnormal. There has been a general pressing of school children toward mediocrity. Every child, too, is expected to become well-rounded and well-adjusted, where adjustment means conformity to someone's ideal personality pattern, a pattern that would minimize individuality and independence of thought and values.

Even adult scientists are not immune to social inhibitions, many of which they impose upon themselves. Barber (1951) reminds us that scientists often resist new ideas for various reasons. Older scientists resist ideas of younger scientists.

baffle, disagreeable

High-ranking scientists resist ideas of low-ranking scientists. Rival schools of thought resist one another. Members of one area of specialization look with suspicion upon ideas of outsiders. Some scientists resist ideas in conflict with their religious convictions. Nor are scientists alone in being subjected to such resistances. Probably the best antidote is to make each creative person aware of his defenses, a suggestion that is in line with Kubie's suggestion (1953) that a scientist should be analyzed to make him more aware of his motivations.

DEVELOPMENT OF CREATIVITY

The usual search for personal-background determiners looks into hereditary features, childhood milieu, and educational influences. We shall have very little evidence to consider in connection with heredity, for so little is actually known about heredity of intellectual traits in general and of creative potential in particular. Certain family-background features seem to be associated with creativity in children and adults, with some consistency and are enough to help predict creative achievement in scientists, for example (C. W. Taylor et al., 1961) and perhaps enough to call for home circumstances that favor development. Much more research has been devoted to operations in attempts to train individuals in creative thinking, and here we find much hope for fostering development.

BIOGRAPHICAL BACKGROUND *Heredity and Creativity.* Galton's classic study of men of genius (1869) stands alone because of its size and scope. He sought to determine how many relatives, at different levels of nearness, there were among best-recognized men in various fields—judges, statesmen, military commanders, writers, scientists, poets, musicians, painters, and religious leaders. The case for creativity is not so very clear, except that among those of outstanding achievement there must have been a large proportion of creative people. Eminence could arise from numerous intellectual qualities apart from the abilities more closely allied with creative potential, however. Galton did find that the nearer the relationship, the greater the number of other prominent individuals. But the effects of heredity and environment were seriously confounded, hence there is no clear-cut conclusion.

In his study of inventors, Rossman (1931) found that top inventors tend to run in families. Again, the result is inconclusive because of the confounding of determiners. The family backgrounds of his inventors tended to be at professional and commercial levels.

confuse

Family and Community Characteristics. In agreement with Rossman's findings, several investigators report that the recognized creative scientist or engineer is likely to come from socio-economic levels of middle class and above (Knapp and Goodrich, 1952; Roe, 1952; Repucci, 1962*a*). This would ensure large proportions with high IQ, which seems to be an important condition for high creative output, as pointed out above. A few writers mention birth order and agree that creative scientists, at least, are first born, or among the first born in the family (Roe, 1952; Weisberg and Springer, 1961; Repucci, 1962*a*). The eldest child, if given responsibility for looking after the younger ones in the family, and perhaps other responsibilities, could consequently develop more readily the habits of self-initiated thinking and independence of thinking.

The rural versus urban milieu of the child seems to have an important bearing on development. In a study involving several thousand children, Torrance (1960*c*) found that there are definitely more signs of tolerance of nonconformity in thinking among rural and town children than among urban children. The signs of creative tolerance were observed in written stories. Children were asked to write stories about a flying monkey or a lion who could not roar. Some children accept the unusual condition and build their stories upon it, others see to it that the animal becomes more normal. The percentages who tolerated the deviating condition in the various groups were: urban, 38; medium-sized towns, 68; rural, 74. Torrance (1962*a*) attributes the difference to a more common use of sanctions to deviant thinking in cities. Where people live in closer proximity, the pressures for conformity are likely to be greater. The independence of farm life has long been one of its greatest attractions. The erstwhile relative isolation of farm-living people may have been a favorable condition for independent thinking, but this condition has been changing. With the rapid urbanization that has been going on, one might expect a downward trend in creativity.

Roe (1952) makes a great deal of the relatively high incidence of loss of a parent in the cases of her outstanding scientists. She suggests that this event leads to lack of personal closeness and to

indecision

independence of acting and thinking. Parental loss was more true of natural scientists than of social scientists. Parents of social scientists were relatively more conscious of social status, which might have turned the attention of the children to social phenomena. The social scientists, as children, also had difficult relationships with parents, which may have called attention to interpersonal behavior.

There is general agreement that the home life of creative scientists, and of some other professional groups, has been less happy than average. MacKinnon (1960) reports this fact. Relationships with parents are reported to be difficult, and the male child often found it impossible to identify himself with his father. This circumstance may have lowered the boy's masculinity. Weisberg and Springer (1961) agree, in general. Through tests and interviews, they elicited the information that the family of the more creative child is not a close one. The marriage is likely to be poorly adjusted, with much dissension in the home. Little emphasis is placed on the child's adopting parental values and little concern is paid to the child's regressions. The last statements would seem to describe good training ground for some aspects of creative adult behavior.

The religious background of creative people has received some attention. Lehman and Witty (1931) found that of 303 starred scientists in American Men of Science for 1927, who gave information on church affiliation, these distinguished scientists came in undue proportions from Protestant faiths and more particularly from the more liberal church groups. Only three reported membership in the Catholic Church. Knapp and Goodrich (1952) have confirmed this kind of finding.

What kind of academic achievement is shown by the creative child and youth? Since creative individuals appear to require an above-average IQ, one should expect them to do better than average in school. This is generally true, but there are some variations. Getzels and Jackson (1961) found that high-school students scoring high on DP tests were often overachievers, in the usual sense that their academic achievement was higher than should be expected on the basis of their IQs. The criterion of achievement was in the form of standard achievement examinations. The creative student might not fare so well in terms of grades based on subjective judgment, for it is reported (Getzels and Jackson, 1961) that teachers commonly do not like the merely creative students as well as they like the merely high-IQ students.

The overachieving due to high DP status should be of interest to those teachers and counselors who are said to attribute overachieving to neuroticism or at least to motivational or emotional factors. The result on high-DP overachievers finds intellectual sources other than aptitudes measured by IQ tests to be responsible. One should also look for still other intellectual qualities, apart from either IQ or DP status, to account for other cases of overachievement.

RELATION TO CHRONOLOGICAL AGE The development of any mental function is commonly determined by a cross-sectional approach, measuring status of different children in the various age groups. We have that kind of information regarding growth in DP abilities. We also have information regarding the probable growth and decline of creativity from young adulthood.

In Childhood and Youth. Using a number of tests of fluency and flexibility, Torrance (1961*a*, 1962*a*, 1964) has found some characteristic growth curves for semantic DP tests. The trends for boys and girls separately are shown in Fig. 11-1. The data on which Fig. 11-1 was based came from the Ask-and-Guess test, the Ask score. In this test, a picture is shown to the child, who is told to ask questions that cannot be answered by looking at the picture. It is not certain which of the DP factors are most represented in this score, perhaps the three factors pertaining to semantic units (ideational fluency), transformations (originality), and implications (semantic elaboration). At any rate, the curves are typical.

The general shapes of the curves justify the description to the effect that growth is typically steady in grades one to three. From there on, we find some setbacks at grades four, seven, and eleven, the one at four being the most drastic, causing the greatest concern.[5] Recovery comes during grades five and six, but the third-grade status is not again exceeded until grades nine (for girls) and ten (for boys). The slump is

---

[5] It should be pointed out that below the fourth grade the DP tests are administrated individually, with oral responses, whereas above the third grade written group tests are used. It is not known whether and to what extent the apparent slump may be due to this change in testing procedure.

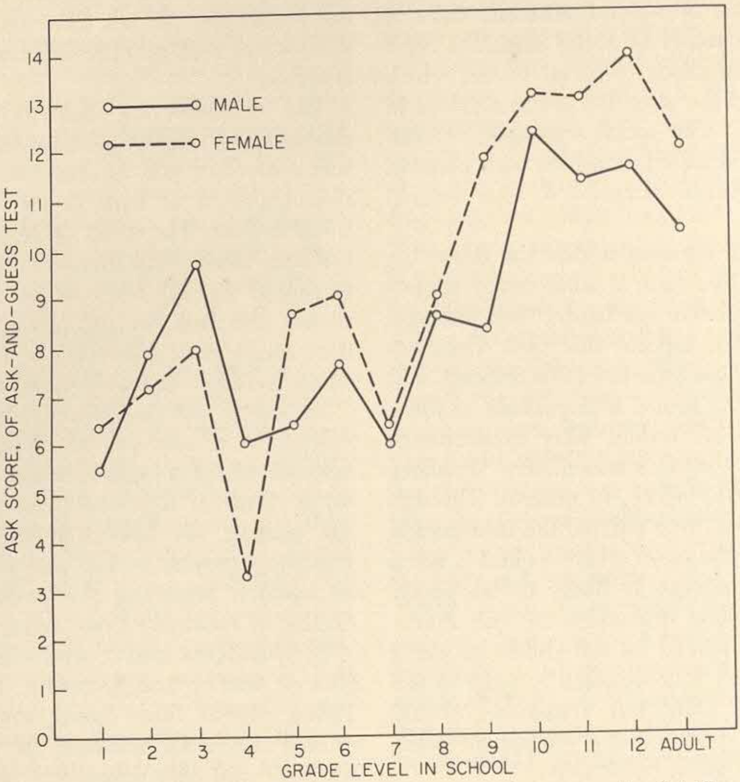


Fig. 11-1. Typical growth curves for male and female subjects in the "Ask" score of the Ask-and-Guess test. (Reproduced by permission of Prentice-Hall, Inc. and Dr. E. Paul Torrance.)

shown in ways other than that of test data. There is a drop in self-initiated writing and a lowering of interest in making up songs.

Torrance has given considerable attention to the fourth-grade slump. He attributes the slump to social or cultural causes, and he is in the process of investigating this hypothesis. Tests given to children in other cultures show different pictures of development in grades three and four. For example, the fourth-grade slump does not appear with Samoan children (Torrance, 1962*b*). It is found at somewhat different ages for children in Australia, Germany, and for American Negro children. The children of India are like our own in showing the drop at the age of nine.

Torrance believes that the fourth-grade slump is not necessary. Although children encounter new pressures toward acceptance of cultural norms and values at that age and they also become increasingly aware of sex roles, some children make the transition without a break in development. On the other hand, some children who do regress, may show little or no recovery (Torrance, 1962*a*).

The pressures toward conformity on the child in the fourth grade may also be accompanied by personality disturbances, some of which may stem from the repression of creative tendencies. Torrance goes so far as to say "I believe there is little question that prolonged, enforced repression of the creative desire may lead to actual breakdown of the personality." (Torrance, 1962*b*, p. 32). There is need for much more analytical research on problems connected with the fourth-grade situation.

The slump at grade seven coincides with the time of entrance into junior high school. It is possible that the kinds of tests used seem suddenly childish to children who are growing rapidly physically and who are putting childhood behind them. As they feel more secure about the transition from childhood to adolescence, perhaps the threat of not growing up diminishes, permitting them to work more nearly up to capacity on the tests. This hypothesis could be tested in schools that do not have the junior high school, where grade seven does not represent a sudden change in school advancement.

There are less ready hypotheses regarding the slight drop after grade ten. The locus of the slump there, if there is one, is not so clear. The curves in Fig. 11-1 show the adult means to be lower than those in senior high school. This result should be checked with other tests. The writer's findings show that adult groups rather consistently have higher means on tests of fluency and flexibility than do ninth-grade groups.

In Creative Adults. In the study of changing creativity with adults, the major interest has been in the "most creative" years; the years at which creative producers accomplish their masterpieces. At what age did each person reach his peak performances?

Lehman (1953), who studied this particular problem most, came to conclusions regarding the ages of most creative outputs in several fields, with the following results: chemists, 26-30; mathematicians, 30-40; musicians, 30-40; authors, before 45; and philosophers, 35-39. Many who are interested in the problem have been surprised that the "most creative years" have been found when the producers are so relatively young. But the ages attached to greatest productions are probably even a bit younger than those given, for the latter are publication dates. Furthermore, poets achieve their peak performances at ages younger than authors in general, and 61 per cent of Rossman's 710 inventors produced the first invention before the age of 25, the average age being 21.3.

Many possible reasons have been suggested for the most creative years being commonly thirty to forty (Lehman, 1953); the incidence of physical decline, let-down in motivation following high achievement, cumulated criticism from others, increasing family responsibilities, to name but a few. To these we might add that many creative people who have achieved, either in the academic or industrial situation, are made administrators. Then, too, after an outstanding production the person keeps busy following out the implications, as he is expected to do, and these are pale by comparison.

The trend for creative potential after maturity belongs with the general problem of intellectual growth and decline. Abilities that commonly enter into IQ tests appear to reach their maximal levels in the early twenty's, with noticeable declines coming in after age forty. From this information, one might ask why creative potential reaches its crest somewhat later. Emphasis upon peak quality suggests that fluency is not emphasized in studies like Lehman's, but flexibility *is* important. Thus, there may be different trend curves for fluency and flexibility. Actually, trend curves should be determined for each factor separately, if possible.

One reason the peak creative potential comes later than for other intellectual abilities may be that accumulated information is needed for creative production, and this may take a matter of years. But then, if information keeps cumulating as time goes on, why does the highest-level production decrease after forty? For a possible answer to this question, we may invoke what is essentially a proactive-inhibition hypothesis. We saw evidence earlier that the greater the supply of information a person has in storage within a certain category, the more difficult is retrieval of that information for productive use. The same kind of principle applies in the recall of information for direct use, as learned, and the phenomenon is called proactive inhibition. In general, the remedy should be to reduce interferences that function during recall. One can probably work at this at both ends; that is, during learning and also during recall. There are probably favorable ways of committing information to storage and also favorable ways of getting it out for indirect use.

Young scientists, and others, who may be discouraged to learn that the most creative years are typically past by the age of forty, should take heart from a very comforting fact regarding the quantitative aspect of creative production. The *rate* of output of productions remains high through the forty's and fifty's, and declines slowly after sixty (Dennis, 1956). From a study of 100 eminent physical scientists of the nineteenth century, who lived into their seventy's and eighty's, it was found that the average man was more productive, in terms of quantity, in his eighty's than in his twenty's. The average rate of production between the age of thirty and sixty remained fairly constant at about two contributions per year.

RELATION TO TRAINING In the remaining paragraphs, we shall be concerned with problems of training aimed at improving creativity, whether of

decrease

a general or specific character, and with experimental results and consequences.

Possibilities of Training. Among promoters of creative education, there is general optimism, and experimental results appear to give them much support. Some psychologists share Bartlett's view (1958) that all thinking can be regarded as intellectual skills that are trainable, by analogy to psychomotor skills. The writer believes that this is the conception to follow, with the added suggestion that each of the intellectual factors is a unique kind of generalized skill, which may have been developed largely as a skill and is to a large extent trainable. No doubt that heredity sets limits. Not everyone could become a four-minute miler in running, and not everyone could become an equivalent star in thinking. But anyone can improve.

From those who have studied top creative people this idea gains some support, Agnew (1922*a*) reported that musicians generally testify to the fact that they learn to gain control over their auditory imagery through practice. Rossman (1931) believes that an inventor with high aptitudes for invention will invent, no matter what or how much formal education he may have. But he goes on to say that even high aptitude can be more effective with training of the right kind. We shall not enter into a debate on the issue of whether increased performance following training means increased creative potential or whether it means a better use of that potential. The writer's view is that probably both changes occur.

General Training in Creativity. General training in creativity is of two kinds. In certain regular school courses, teachers may take special pains to teach creatively. The subject matter is that of the courses in question, but the methods of teaching are designed to encourage creative thinking. The other kind is in the nature of a course on creative thinking, in which the content of the course is about the nature of thinking, and it provides student exercises in thinking. Such courses are more likely to be found outside educational institutions. There have been experiments designed to test the effectiveness of both kinds of courses. The usual design is a simple one. An experimental group that has the training and a matched control group that does not, with pre- and posttests at appropriate time intervals in the two groups.

In an experiment by Torrance et al. (1960*c*), ten teachers were taught principles of teaching creatively, after which they applied those principles in courses in general over a four-month period. Another ten teachers taught in their usual ways. There were pre- and posttests of DP abilities. The tests showed superior gains in originality and elaboration test scores in four of the six grades, superiority in fluency in three, and in flexibility in two.

It is sometimes suggested that courses in art should be favored as training ground for creativity in general, because of the more obvious opportunities to give practice in being creative. One experiment reported on this kind of training (Owens, 1962) used matched groups of twenty-five each in grades six and seven. The art training emphasized creative activity. The experimental group scored higher on a variety of posttests, including some DP tests and problems, e.g., problems in science. Interesting, from the standpoint of differential DP abilities depending upon test content, the superiority of the experimental group tended to be greater in figural than in semantic tests. The experimental group was not higher in the test of problems in the social sciences, which appears to this writer to be a test of cognition (cognition of semantic implications) and not of divergent production. Although there is some indication of transfer of training in creativity from an art course, it is doubtful that the extent of transfer is as broad as the proponents of art training expect.

Quite a number of studies with pre- and posttests in connection with courses on creative thinking agree rather well that the trained Ss gain significantly more in most DP tests, but definitely more in tests of originality than of fluency, in which sometimes there is no gain (Gerry et al., 1957; Meadow and Parnes, 1959; Parnes and Meadow, 1959; Nicholson, 1959; Parnes, 1962). These results might suggest that fluency is a personal quality that is little influenced by training, or that the kind of training is not suited to promoting gains in fluency, or that the courses call the learner's attention more to the value of high-quality ideas. The courses have varied from a few weeks to a whole semester. The special methods used have been several, with Osborn's brainstorming method probably dominating. Parnes (1962) reports that the benefits of training apply to those initially high or low in the DP tests, to those with high or low IQ, and to old (23-51 years) as well as to young (17-22). Parnes and

Meadow (1960) have ascertained that the training effects are somewhat persistent, as shown by testing students who had completed the training eight months to four years previously.

There are also some evidences of transfer effects, more of which we shall see from experiments with specific kinds of training. Inventory scores show changes in the direction of greater dominance (confidence, self-reliance, and the like) as well as in self-control (Meadow and Parnes, 1959; Parnes, 1962). These changes might be interpreted as very general or broad transfer effects. At the AC Sparkplug Company plants, Simberg and Shannon (1959) gave a ten-week training course to employees who had had very high records in feeding the suggestion box and also some who had very low records of this sort. For the year following the training, both experimental groups showed gains in total number of suggestions offered, number accepted, and amount of monetary reward. There were increases in both quantity and quality. The initially high suggestion group kept its lead through training. The latter fact suggests that individuals performing at higher levels of creativity are not necessarily working near their peak possibilities.

Special Training Methods. It is not possible to describe at any length the various methods or devices that have been invented as aids to creative production. It will have to suffice to characterize each major method and to examine it with respect to its relations with what we know about the psychology of creative thinking. Such relationships can be observed. Except for brainstorming, no special method has been subjected to experimental examination. Some of the techniques were designed for application in group thinking, but most can be used or adapted in individual problem solving. The condition of suspended judgment is common to several of them. Most of them concentrate on ways of generating ideas and they provide steps to facilitate the scanning process in recall of information.

For example, Osborn recommends a "checklist" procedure. It is a question-asking approach, which ensures a wide coverage of information. Another method, which encourages divergent production as well as transformations, is called "attribute listing" (Crawford, 1954). In describing an object or a situation, one thinks of specifications, limitations, needs, and so on. An example taken from Arnold (1962*a*) pertains to thoughts about a screwdriver. A typical screwdriver has the following major attributes:

A round, steel shank
A wooden handle riveted to the shank
A wedge-shaped end to enter screw head
It is manually operated
Torque is provided to achieve a twisting movement

If one wished to improve upon the typical screwdriver, having listed the attributes, one could change one attribute at a time. Osborn's check-list method would apply at this point. One could change the handle from wood to plastic, which is a better insulator and is more durable. The last comments are by way of evaluation. The shank could be changed from round to hexagonal, in order to use with a wrench or pliers. Such screwdrivers are already common, but they are of recent vintage.

A comprehensive and systematic way of combining checklist and attribute listing is found in the method known as "morphological analysis," a logical approach named and used extensively by the astronomer Zwicky (1957). Suppose we wished to think up some new mode of human transportation, to use another example given by Arnold (1962*a*) with a few modifications. The problem is first analyzed in terms of the fundamental dimensions or variables or parameters of transportation. First, there is the type of carrier or supporting component: cart, chair, sling, or bed. Second, there is the medium of support for the carrier: air, water, oil, hard surface, rollers, rails. Third, there is the source of power: compressed air, internal-combustion engine, electricity, steam, magnetic fields, atomic power, animal, man. All possible combinations of attributes by threes, one from each parameter, provide us with some unique transporting devices, some of which are not yet in existence. If the reader will think back to the early parts of this chapter, he will recognize that the structure of intellect is an example of morphological thinking. The model has served very well in generating ideas of new abilities unsuspected before. Furthermore, the concepts connected with it have suggested principles and concepts of value in psychological theory in general (Guilford, 1960*a*, 1961).

Training for Originality. Maltzman and his associates have performed a series of experiments aimed specifically at training individuals (college students) so as to increase their originality. The major hypothesis has been that giving uncommon responses, which is one of the criteria of originality, is a habit that can be strengthened by the usual procedures of operant learning, in other words by practice with reinforcement (Maltzman et al., 1960).

The typical experiment uses a simple word-association task. The experimental and control groups have an initial test consisting of twenty-five stimulus words, scored with weights for unusualness. The experimental group then has further applications of sets of twenty-five stimulus words, to which they are forced to give uncommon responses by being told not to repeat earlier, more common, responses or they are rewarded for not doing so. A final test with twenty-five stimulus words is given, all tests being scored weighting uncommonness of responses. Sometimes the Unusual Uses test is also given as an indication of possible transfer effects.

When Ss are given the initial list repeated several times, with instructions to avoid giving the same response to the same stimulus word, there is a significant average gain in the final test, and also often in the Unusual Uses test, the latter result indicating some transfer. Other conditions of training are also effective, including rewarding Ss for giving uncommon responses during the training series (Maltzman, Bogartz, and Berger, 1958), and presenting pairs of remotely associated words, S to select one of them. Giving items of the Unusual Uses test, with S instructed to give a different response each time, also showed transfer effects in performance on the word-association test.

The larger the number of training trials, the greater is the improvement. Ss having five and ten trials improved more than those with one trial, but the latter group improved some. Retention of training effects was found to persist at least forty-eight hours after training, with more originality shown after one hour than after forty-eight hours. There were also some persisting transfer effects.

Others have also found transfer effects. After some special exercises in creative thinking, children showed more creative writing and a better attitude toward creativity (Torrance, 1964). Hyman (1960) found that the Maltzman technique of training for originality transferred to creative problem solving other than in Unusual Uses.

In attempting to account for the improvement in originality on the basis of association theory, Maltzman (1960) makes two assumptions, both of which can be questioned. Recognizing that the reinforcement is not specific and neither is the transfer, Maltzman assumes, first, that every associative response is associated with every other associative response. This would account for the fact that S can give remotely associated responses. The second assumption is that associations are stronger between pairs of uncommon associates than between an uncommon and a common response.

In Maltzman's originality training, uncommon associations are reinforced. The reinforcement, when no reward is given, is in S's achievement of a difficult associative response, which he is highly motivated to give. There is transfer of reinforcement to other uncommon responses because of assumption two. Common responses are not reinforced, which presumably builds up a resistance against giving common responses, which transfers to other common responses.

An alternative explanation would be that the Maltzman originality training is a case of learning how to learn, a theory that he rejects (1960). If one is willing to admit that the Ss recognize uncommon responses as a class and common responses as another, the differential reinforcement of classes would provide a very simple explanation calling for no questionable assumptions concerning associations. Hyman (1960) offers another alternative. The training forces S to give more difficult responses and thus raises his standards as to quality of response. The implicit assumption is that S has a goal of giving high-quality responses. This is likely to be true except when S receives a suspended-judgment instruction.

Some Implications of Creativity Training for Education. There are numerous implications in all this for education, but going into this extensive subject would take us too far from the basic theory with which this chapter is primarily occupied. The best that can be done is to refer the reader especially to the contributions of Torrance, most of which are listed in the following bibliography.

SUMMARY

An old subject, generally avoided by psychologists, creativity came to life in a big way during the previous decade. Largely responsible for general interest in the subject are the needs for new ideas for solving problems in years after the Second World War. Largely responsible for greatly increased research activity by psychologists has been the realization that creative qualities are not restricted to the favored few but are broadly distributed in the population. The multivariate approach of factor analysis has demonstrated the many-faceted nature of creative potential and has provided many new concepts that are necessary for the systematic exploration of creative phenomena.

Previously existing theories regarding the nature of creative thinking and creative production were found wanting. A new theory adopts the idea that a major motivating force behind creative behavior is an intellectual drive, whose satisfaction is found in intellectual achievement. Many secondary motivating sources also make contributions.

In their intellectual aspects, creative thinking and problem solving are regarded as one and the same, and both are forms of learning in which transfer effects are most significant features. The most dramatic feature of problem solving is insight or intuition, which is also the key to the understanding of creative thinking. Insight is highly dependent upon previous experience, but definitely involves something more that is crucially important. That "something more" is in the nature of the formation of products of information of the kind recognized in the writer's structure-of-intellect theory. We must learn how such products come about, even though the processes are almost completely unconscious, and we have the resources with which to do this. An important aspect of this problem is recall, or the retrieval of personal, stored information; a very neglected subject in psychological research.

Social influences have their effects upon creative problem solving, whether in group thinking or in individual thinking. A society that would make the most of its creative resources could do much to promote better conditions for creative production. Creative potential and creative production develop, and certain training procedures are known to be effective, perhaps in terms of both creative potential and creative production. Improvement in training procedures will depend very much upon our increased understanding of the nature of creative thinking itself and upon the environmental conditions that affect it.

REFERENCES

ADAMSON, R. E., AND TAYLOR, D. W. Functional fixedness as related to elapsed time and to set. *J. exp. Psychol.*, 1954, 47, 122-126.

AGNEW, M. A comparison of the auditory images of musicians, psychologists, and children. *Psychol. Monogr.*, 1922*a*, 31, 268-278.

———. The auditory imagery of great composers. *Psychol. Monogr.*, 1922*b*, 31, 279-287.

ANDREWS, E. G. The development of imagination in the preschool child. *Univer. Iowa Stud. Character*, 1930, 3(4).

ARNHEIM, R., AUDEN, W. H., SHAPIRO, K., AND STAUFFER, D. A. Poets at Work. New York: Harcourt, Brace, 1948.

ARNOLD, J. E. Useful creative techniques. In S. J. Parnes and H. F. Harding (eds.), *A Source Book for Creative Thinking*. New York: Charles Scribner's Sons, 1962*a*, 252-268.

———. Education for innovation. In S. J. Parnes and H. F. Harding (eds.), *A Source Book for Creative Thinking*. New York: Charles Scribner's Sons, 1962*b*, 127-138.

BARBER, B. Resistance by scientists to scientific discovery. *Science*, 1961, **134**, 596-602.

BARRON, F. Complexity-simplicity as a personality dimension. *J. abnorm. soc. Psychol.*, 1953, 48, 163-172.

———. The disposition toward originality. *J. abnorm. soc. Psychol.*, 1955, **51**, 478-485.

BARTLETT, F. *Thinking: An Experimental and Social Study*. New York: Basic Books, 1958.

BEITTEL, K. R. Creativity in the visual arts in higher education: criteria, predictors, experimentation, and their interactions. In C. W. Taylor (ed.), *Widening Horizons in Creativity*. New York: John Wiley & Sons, Inc., 1964.

BERGER, R. M., GUILFORD, J. P., AND CHRISTENSEN, P. R. A factor-analytic study of planning. *Psychol. Monogr.*, 1957, **71**, Whole No. 435.

BIRCH, H. G. The role of motivational factors in insightful problem solving. *J. comp. physiol. Psychol.*, 1945*a*, **38**, 295-317.

———. The relation of previous experience to insightful problem solving. *J. comp. physiol. Psychol.*, 1945*b*, **38**, 367-383.

———, AND RABINOWITZ, H. S. The negative effect of previous experience on productive thinking. *J. exp. Psychol.*, 1951, **41**, 121-125.

BOND, N. A. An experimental study of transfer effects in human problem solving. Unpublished doctoral dissertation, University of Southern California, 1955.

BOUSFIELD, W. A., SEDGEWICK, C. H. W., AND COHEN, B. H. Certain temporal characteristics of the recall of verbal associations. *Amer. J. Psychol.*, 1954, 57, 111-118.

Brittain, W. L. and Beittel, K. R. A study of some tests of creativity in relationship to performances in the visual arts. *Studies in Art Education*, 1961, 2, 54-65.

Burkhart, R. C. *Spontaneous and Deliberate Ways of Learning.* Scranton, Pa.: International Textbook Company, 1962.

Cannon, W. B. The role of chance in discovery. *Sci. Monthly*, 1940, **50**, 204-209.

Carroll, J. B. A factor analysis of verbal abilities. *Psychometrika*, 1941, **6**, 279-307.

Chassell, L. M. Tests for originality. *J. educ. Psychol.*, 1916, 7, 317-328.

Chorness, M. H., and Nottelman, D. A. *The Predictability of Creative Expression in Teaching.* Lackland AF Base: Personnel and Training Research Center, December, 1956.

———, and ———. *The Prediction of Creativity among Air Force Civilian Employees.* Lackland AF Base: Personnel and Training Research Center, March, 1957.

Christensen, P. R., Guilford, J. P., and Wilson, R. C. Relations of creative responses to working time and instructions. *J. exp. Psychol.*, 1957, 53, 82-88.

Cowell, H. The process of musical creation. *Amer. J. Psychol.*, 1926, 37, 233-236.

Cowen, E. L. Stress reduction and problem-solving rigidity. *J. consult. Psychol.*, 1952, **16**, 425-428.

Crawford, R. P. *Techniques of Creative Thinking.* New York: Hawthorne Books, 1954.

Crutchfield, R. S. Personal and situational factors in conformity to group pressure. Proceedings of the 15th International Congress of Psychology. *Acta Psychol.*, 1959, **15**, 386-388.

Dearborn, G. V. A study of imagination. *Amer. J. Psychol.*, 1898, 9, 183-190.

Dennis, W. Age and productivity among scientists. *Science*, 1956, **123**, 724-725.

———. The age decrement in outstanding scientific contributions. *Amer. Psychologist*, 1958, 13, 457-460.

Dewey, J. *How We Think.* Boston: D. C. Heath and Company, 1910.

Drevdahl, J. E. Factors of importance for creativity. *J. clin. Psychol.*, 1956, 12, 21-26.

Duncker, K. On problem solving. *Psychol. Monogr.*, 1945, 58, Whole No. 270.

Eindhoven, J. E., and Vinacke, W. E. Creative process in painting. *J. gen. Psychol.*, 1952, **47**, 139-164.

Elliott, J. M. *Advancing Our Methods for Measuring Creative Abilities.* Information service for A.N.A. members, New York Association of National Advertisers, May, 1962.

Flavell, J. H., Cooper, A., and Loiselle, R. H. Effect of the number of pre-utilization functions on functional fixedness in problem solving. *Psychol. Reports*, 1959, **4**, 343-350.

Frick, J. W., Guilford, J. P., Christensen, P. R., and Merrifield, P. R. A factor-analytic study of flexibility in thinking. *Educ. psychol. Measmt.*, 1959, **19**, 469-496.

Galton, F. *Hereditary Genius.* New York: D. Appleton & Company, Inc., 1869.

Gerry, R., DeVeau, L., and Chorness, M. *A Review of Some Recent Research in the Field of Creativity and the Examination of an Experimental Creativity Workshop.* Lackland AF Base: Training Analysis and Development Division, 1957.

Getzels, J. W., and Jackson, P. W. *Creativity and Intelligence.* New York: John Wiley & Sons, Inc., 1961.

Gordon, Wm. J. J. *Synectics.* New York: Harper & Brothers, 1961.

Grotjahn, M. Creativity and freedom in art and analysis. In M. Grotjahn (ed.), *Beyond Laughter.* New York: McGraw-Hill Book Company, Inc., 1957.

Guilford, J. P. Creativity. *Amer. Psychologist*, 1950, **5**, 444-454.

———. Three faces of intellect. *Amer. Psychologist*, 1959, **14**, 469-479.

———. An emerging view of learning theory. In *Intelligence, Creativity, and Learning.* In J. A. Ross and R. Thompson (eds.), *Proc. Summer Conference*, Bellingham, Wash.: Western Washington College, 1960*a*.

———. Basic conceptual problems in the psychology of thinking. *Ann. N. Y. Acad. Sciences*, 1960*b*, **91**, Art. 1, 6-21*b*.

———. Factorial angles to psychology. *Psychol. Rev.*, 1961*a*, **68**, 1-20.

———, Christensen, P. R., Frick, J. W., and Merrifield, P. R. Factors of interest in thinking. *J. gen. Psychol.*, 1961*b*, **65**, 39-56.

———, and Merrifield, P. R. The structure-of-intellect model: its uses and implications. *Rep. psychol. Lab.*, No. 24. Los Angeles: University of Southern California, 1960.

———, ———, and Cox, A. B. Creative thinking in children at the junior high school levels. *Rep. psychol. Lab.*, No. 26. Los Angeles: University of Southern California, 1961.

Hadamard, J. S. *An Essay on the Psychology of Invention in the Mathematical Field.* Princeton, N.J.: Princeton University Press, 1945.

Hargreaves, H. L. The 'faculty' of imagination. An enquiry concerning the existence of a general 'faculty' or group factor, of imagination. *Brit. J. Psychol. Monogr.*, 1927, Supp. No. 10.

Hilgard, E. R. Creativity and problem solving.

In H. H. Anderson (ed.), *Creativity and Its Cultivation*. New York: Harper & Brothers, 1959.

Hoffa, H. E. The relationship of art experience to some attributes of conformity. Unpublished doctoral dissertation, Pennsylvania State University, 1959.

Houston, J. P., and Mednick, S. A. Creativity and the need for novelty. *J. abn. soc. Psychol.*, 1963, **66**, 137-144.

Hyman, R. *Some Experiments in Creativity*. New York: General Electric, 1960.

———. On prior information and creativity. *Psychol. Reports,* 1961, **9**, 151-161.

Johnson, D. M. *The Psychology of Thought and Judgment*. New York: Harper & Brothers, 1955.

———. Problem-solving processes. *Amer. Psychologist,* 1962, **17**, 327. (Abstract)

Jones, C. A. Some relationships between creative writing and creative drawing of sixth grade children. Doctoral dissertation, Pennsylvania State University, 1960.

Kettner, N. W., Guilford, J. P., and Christensen, P. R. A factor-analytic study across the domains of reasoning, creativity, and evaluation. *Psychol. Monogr.*, 1959, **73**, Whole No. 479.

Kincaid, C. E. The determination and description of various creative attributes of children. *Studies in Art Education,* 1961, **2**, 45-53.

Knapp, R. H., and Goodrich, H. B. *Origins of American Scientists*. Chicago: University of Chicago Press, 1952.

Kris, E. Psychoanalysis and the study of creative imagination. *Bull. New York Acad. Med.*, 1953, 334-351.

Kubie, L. S. Some unsolved problems of the scientific career. *Amer. Scientist,* 1953, **41**, 596-613.

Lehman, H. C. *Age and Achievement*. Princeton, N.J.: Princeton University Press, 1953.

———. Reply to Dennis' critique of *Age and Achievement*. *J. Geront.*, 1956, **11**, 333-337.

———, and Witty, P. A. Scientific eminence and church membership. *Sci. Monthly,* 1931, **34**, 544-549.

Lowenfeld, V. *Creative and Mental Growth*. 3d ed. New York: The Macmillan Co., 1957.

Luchins, A. S. Mechanization in problem-solving behavior. *Psychol. Monogr.*, 1942, **54**, Whole No. 248.

———, and Luchins, E. H. *Rigidity of Behavior; A Variational Approach to the Effect of Einstellung*. Eugene, Ore.: University of Oregon Books, 1959.

MacKinnon, D. W. The highly effective individual. *Teachers Coll. Rec.*, 1960, **61**, 367-378.

Maier, N. R. F. Reasoning in humans. Part I. On direction. *J. comp. physiol. Psychol.*, 1930, **10**, 115-143.

———. Reasoning in humans. The solution of a problem and its appearance in consciousness. *J. comp. physiol. Psychol.*, 1931, **11**, 181-194.

Maltzman, I. On the training of originality. *Psychol. Rev.*, 1960, **67**, 229-242.

———, Bogartz, W., and Berger, L. A procedure for increasing word association originality and its transfer-effects. *J. exp. Psychol.*, 1958, **56**, 392-398.

———, Brooks, L. O., Bogartz, W., and Summers, S. S. The facilitation of problem-solving by prior exposure to uncommon responses. *J. exp. Psychol.*, 1958, **56**, 399-406.

———, Simon, S., Raskin, D., and Licht, L. Experimental studies in the training of originality. *Psychol. Monogr.*, 1960, **74**, Whole No. 493.

Maslow, A. H. Emotional blocks to creativity. *Humanist,* 1958, **18**, 325-332.

McLean, F. C. The happy accident. *Sci. Monthly,* 1941, **53**, 61-70.

McGeoch, J. A., and Irion, A. L. *The Psychology of Human Learning*. New York: David McKay Company, Inc., 1952.

Meadow, A., and Parnes, S. J. Evaluation of training in creative problem-solving. *J. appl. Psychol.*, 1959, **43**, 189-194.

———, ———, and Reese, H. Influence of brainstorming instructions and problem sequence on a creative-problem-solving test. *J. appl. Psychol.*, 1959, **43**, 413-416.

Mednick, S. A. The associative basis of the creative process. *Psychol. Rev.*, 1962, **69**, 220-232.

Merrifield, P. R., Guilford, J. P., Christensen, P. R., and Frick, J. W. Interrelationships between certain abilities and certain traits of motivation and temperament. *J. gen. Psychol.*, 1961, **65**, 57-74.

———, ———, ———, and ———. The role of intellectual factors in problem solving. *Psychol. Monogr.*, 1962, **76**, Whole No. 529.

Miller, G. A., Galanter, E., and Pribram, K. H. *Plans and the Structure of Behavior*. New York: Holt, Rinehart, and Winston, Inc., 1960.

Nicholson, P. J. An experimental investigation of the effects of training upon creativity. Doctoral dissertation, University of Houston, 1959.

Osborn, A. F. *Applied Imagination*. Rev. ed. New York: Charles Scribner's Sons, 1957.

———. Developments in creative education. In S. J. Parnes and H. F. Harding (eds.), *Source Book for Creative Thinking*. New York: Charles Scribner's Sons, 1962, 20-29.

OWENS, C. An investigation of creative potential at the junior high level. *Studies in Art Education*, 1962, 3, 16-33.

PARNES, S. J. Effects of extended effort in creative problem solving. *J. educ. Psychol.*, 1961, **52**, 117-122.

———, AND MEADOW, A. Effects of brainstorming instructions on creative problem-solving by trained and untrained subjects. *J. educ. Psychol.*, 1959, **50**, 171-176.

———, AND ———. Evaluation of persistence of effects produced by a creative problem-solving course. *Psychol. Reports*, 1960, 7, 357-361.

PATRICK, C. Creative thought in poets. *Archives Psychol.*, 1935, **26**, 1-74.

———. Creative thought in artists. *J. Psychol.*, 1937, 4, 35-73.

———. Scientific thought. *J. Psychol.*, 1938, **5**, 55-83.

———. Whole and part relationship in creative thought. *Amer. J. Psychol.*, 1941, **54**, 128-131.

PICKFORD, R. An experiment on insight. *Brit. J. Psychol.*, 1938, **28**, 412-422.

POINCARÉ, H. *The Foundations of Science*. New York: Science Press, 1913.

REPUCCI, L. C. Biographical differences between hi and lo creative subjects. Unpublished paper. Midland, Mich.: Dow Chemical Co., 1962*a*.

———. Predictive value of unreduced tension as related to creativity. Unpublished doctoral dissertation, Wayne State University, 1962*b*.

ROE, A. A study of imagery in research scientists. *J. Personality*, 1951, **19**, 459-470.

———. *The Making of a Scientist*. New York: Dodd, Mead & Co., 1952.

ROGERS, C. R. Toward a theory of creativity. In S. J. Parnes and H. F. Harding (eds.), *A Source Book for Creative Thinking*. New York: Charles Scribner's Sons, 1962, 64-72.

ROSEN, V. H. On mathematical illumination and the mathematical thought process, in *The Psychoanalytic Study of the Child*. New York: International Universities Press, 1953, Vol. 8, 127-154.

ROSSMAN, J. *The Psychology of the Inventor*. Washington, D.C.: Inventors Publishing Co., 1931.

———. A study of the childhood, education, and age of 710 inventors. *J. Patent Office Soc.*, 1935, **17**, 411-421.

SIMBERG, A. L., AND SHANNON, T. E. The effect of AC creativity training on the AC suggestion program. *AC Personnel Research Report*, No. 27. Flint, Mich.: AC Sparkplug Division, General Motors Corp., 1959.

SNAPPER, A. G. Mediating verbal responses in transfer of training. Cambridge, Mass.: Undergraduate Honors Thesis. Department of Psychology, Harvard University, 1956.

SPEARMAN, C. *Creative Mind*. London: Nisbet, 1931.

SUCHMAN, J. R. Inquiry training: building skills for autonomous discovery. *Merrill-Palmer Quart. Beh. Developmt.*, 1961, 7, 147-169.

TAYLOR, C. W. A factorial study of fluency in writing. *Psychometrika*, 1947, **12**, 239-262.

———, SMITH, W. R., GHISELIN, B., AND ELLISON, R. Explorations in the measurement of prediction of contributions of one sample of scientists. *Technical Report* ASD-TR-61-96. Lackland AF Base: Personnel Laboratory, 1961.

TAYLOR, D. W. Thinking and creativity. *Ann. New York Acad. Sci.*, 1960, **91**, Art. 1, 108-123.

———, BERRY, P. C., AND BLOCK, C. H. Does group participation when using brainstorming facilitate or inhibit creative thinking? *Admin. Sci. Quart.*, 1958, 3, 23-47.

THORNDIKE, E. L. *Human Learning*. New York: D. Appleton-Century, Inc., 1931.

———. The psychology of invention in a very simple case. *Psychol. Rev.*, 1949, **57**, 192-199.

THURSTONE, L. L. Primary mental abilities. *Psychometric Monogr.*, No. 1, 1938.

———. Creative talent. In L. L. Thurstone (ed.), *Applications of Psychology*. New York: Harper & Brothers, 1952, 18-37.

TORRANCE, E. P. Explorations in creative thinking in the early school years: a progress report. In C. W. Taylor (ed.), *The Third (1959) University of Utah Research Conference on the Identification of Creative Scientific Talent*. Salt Lake City, Utah: University of Utah Press, 1959*a*.

———. Sex role identification and creativity. An exploratory study. *Research Memorandum* BER-59-10. Minneapolis: University of Minnesota, 1959*b*.

———. Explorations in creative thinking in mental hygiene: III. Effects of induced evaluative sets on the development of new ideas. *Research Memo.*, BER-59-15. Minneapolis: University of Minnesota, 1959*c*.

———. Explorations in creative thinking in mental hygiene: IV. Need characteristics of more creative, mental hygiene students. *Research Memo.*, BER-60-8. Minneapolis: University of Minnesota, 1960*a*.

———. Educational achievement of the highly intelligent and the highly creative; eight partial replications of the Getzels-Jackson study. *Research* Memo., BER-60-18. Minneapolis: University of Minnesota, Bureau of Educational Research, 1960*b*.

———, et al. *Rewarding Creative Thinking.* Minneapolis: University of Minnesota, Bureau of Educational Research, 1960*c*.

———. Factors affecting creative thinking in children, an interim research report. *Merrill-Palmer Quart. Beh. Developmt.*, 1961*a*, **7**, 171-180.

——— Can grouping control social stress in creative activities? *Element. School J.*, 1961*b*, **62**, 139-145.

———. *Guiding Creative Talent.* Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1962*a*.

———. Developing creative thinking through school experiences. In S. J. Parnes and H. F. Harding (eds.), *A Source Book for Creative Thinking.* New York: Charles Scribner's Sons, 1962*b*, 31-47.

———. The Minnesota studies of creative thinking. In C. W. Taylor (ed.), *Widening Horizons in Creativity.* New York: John Wiley & Sons, Inc., 1964.

———, AND HARMON, J. A. Effects of memory, evaluative, and creative reading sets on test performance. *J. educ. Psychol.*, 1961, **52**, 207-214.

VINACKE, W. E. *The Psychology of Thinking.* New York: McGraw-Hill Book Company, Inc., 1952.

WALLAS, G. *The Art of Thought.* London: G. A. Watts, 1945.

WEISBERGER, P. S., AND SPRINGER, K. J. Environmental factors influencing creative function in gifted children. (Mimeographed) Cincinnati: University of Cincinnati, 1961.

WEISSKOPF-JOELSON, E., AND ELISEO, T. S. An experimental study of the effectiveness of brainstorming. *J. appl. Psychol.*, 1961, **45**, 45-49.

WELCH, L. Recombination of ideas in creative thinking. *J. appl. Psychol.*, 1946, **30**, 638-643.

WESTCOTT, M. R. On the measurement of intuitive leaps. *Psychol. Reports*, 1961, **9**, 267-274.

———, AND RANZONI, J. H. Personality correlates of intuitive thinking. *Psychol. Reports*, 1963, **12**, 595-613.

WHITE, R. W. Motivation reconsidered: the concept of competence. *Psychol. Rev.*, 1961, **66**, 297-333.

WILSON, R. C., GUILFORD, J. P., AND CHRISTENSEN, P. R. The measurement of individual differences in originality. *Psychol Bull.*, 1953, **50**, 362-370.

———, ———, ———, AND LEWIS, D. J. A factor-analytic study of creative-thinking abilities. *Psychometrika*, 1954, **19**, 297-311.

WOODWORTH, R. S. *Psychology*, 3d ed. New York: Henry Holt and Company, Inc., 1934.

ZACCARIA, M. A., CHORNESS, M. H., GERRY, R., AND BORG, W. R. *Student Evaluation and Grading: Prediction of Creative Ability.* Lackland AF Base, Analysis and Development Division, 1956.

ZWICKY, F. *Morphological Analysis.* Berlin: Springer-Verlag, 1957.

CHAPTER 12

Personality Theory

JULIAN B. ROTTER
University of Connecticut

INTRODUCTION

Any attempt to delineate an area of study or to construct a theory has an implicit or explicit purpose. For the most part, theories of personality have two general purposes. One of these is to understand or predict how individuals acquire and change their relatively stable, complex behavior. The second purpose is to understand or predict individual differences. Whereas theories of learning, perception, sensation, group behavior, etc., are concerned with common aspects of human behavior, personality theory is concerned with individual differences under similar conditions of stimulation and concerned particularly with consistent individual differences that are predictable from one situation to another.

For the most part, in the development of modern American psychology, an interest in personality followed an interest in individual differences restricted mainly to intellectual functions. Consequently, many personality theories were developed to account for individual differences other than those manifested in intellectual activities. Such abilities were broadly accepted to be dependent upon faculties of the mind such as intelligence, judgment, mechanical aptitude, etc. In its most characteristic usage the term personality is commonly used to refer to individual, stable modes of behavior and attitudes other than those relating to intelligence, achievement, and aptitude.

In this chapter personality will be regarded as a construct and not an entity. It is a term used to designate some *aspects* of behavior of complex individuals. Sometimes the purposes behind the development of a theory are forgotten, and scientists spend much wasted time arguing about what personality really is. But personality is not a thing, it is only a term which is constructed to account for some characteristics of human behavior. While it makes sense to argue whether one personality theory is more useful, more predictive, or more heuristic than another, it does not make sense to argue about what personality really is. Although many recent textbooks on personality spend many pages discussing the question of "What is personality?" these same texts rarely come to any conclusion and the best solution they can arrive at is a summary statement that different writers regard personality in somewhat different ways. Before these various theories of personality can be described and evaluated it is necessary briefly to analyse the nature of psychological constructs.

PSYCHOLOGICAL CONSTRUCTS

If a variety of common household objects were arranged in a room and you were asked to describe them or tell the kinds of terms you would use to describe them, a series of terms could be arranged from very general to very specific based on the number of objects to which they could be applied. For example, the term "heavy" could be used to describe some of the objects but not all of them, whereas weight could be used to describe all of the objects. The concept of weight is more general than that of heavy or light. The terms "heavy" or "light," in addition, are *relative* terms; they are not absolute properties of the objects, but the terms have meaning only in relationship to other objects. Is a chair heavy or light? If a chair can be considered to be light does that mean that all objects that weigh less could not be called heavy?

Blue might describe some of the objects present but colored would describe all of them. Color is a more general concept than blue. But where is the color in the object? Where is the blue in the sky? Where is the height in the table? Where is the roundness in a ball? What these questions are trying to illustrate is that the terms used to describe the events of nature deal with *aspects* of events as perceived by man, not solely with the events themselves. They need not have any specific locus in the objects they are describing, nor any absolute meaning. *They are abstractions of aspects of events ranging from specific to very general and serving various purposes for the people who use them.*

If you were to consider a number of machines, some in poor working order, some working but with obvious difficulties, and some in smooth working order, and ask a superstitious person to describe the working conditions of these motors, he might say that they were all inhabited by gremlins. In the motors that are working well the gremlins are asleep. In the ones that are working poorly, the gremlins are up and beginning to make trouble. And in the ones that are not working at all the gremlins have made the machine break down. Now, if you ask this individual, "How do you know when the gremlins are sleeping and when they are not?" or when it is that the gremlins are causing the motor not to work and when it is that other things like broken parts, lack of fuel, etc., are the "cause," he will tell you that when he can find none of the other things, then the trouble is always being caused by gremlins.

This example illustrates two things about concepts, terms, or constructs. One of them is that because someone has a term or uses a term it does not mean that this term mirrors or reflects something that is real or truly exists. It also illustrates the point that the concept of gremlins serves a purpose for this particular individual and explains for him what he cannot otherwise explain. It satisfies his need to account for something for which he cannot otherwise account. However, it is wholly unsatisfying to the scientist or mechanic, since it does not tell him how to fix the machine, or when to expect it to work and when to expect it not to work. The gremlin construct may be useful to one person for a particular purpose but not be useful at all to the scientist who is interested in predicting events or understanding how these events come to be.

In all sciences there have been terms which have been used to describe the events of nature and which have later been discarded as poor or erroneous conceptions. The belief that a term that has been used to describe events for a long time is therefore a good term or is necessarily something real or useful for scientific explanation is an erroneous one. In more advanced sciences the scientist has learned to ask the question, "What is the most predictive or useful way of describing some events?" rather than asking, "What is this thing really?" Psychology being a relatively new science has many terms and concepts which require revision or elimination and eventual replacement by better ones. The modern psychologist does not ask the question, "What is intelligence really?" He rather asks, "What is a useful way of describing people's abilities so that we can best understand why they behave the way they do and what may be anticipated about their future behavior?"

For the scientist there are two characteristics of scientific terms themselves which can be used as criteria for their value. One of these is *reliability* or measurability. By reliability is meant the degree to which the same event, observed by many different scientists, would be described in the same way by all of them. If they were measuring some aspect of this event, the measures would all be the same. Obviously, if the descriptions varied in regard to whether something is present or absent or the measures of the degree of presence were markedly different when different scientists observed the events, communication would be limited and there could be no body of public knowledge. If the terms and concepts used by scientists are not reliable, there is no science in the true sense but only the individual subjective or personal opinions of a number of different people.

The other general criterion for a good concept, term, or construct, is its *utility* for a given purpose. We cannot look into the object somewhere to find out whether its weight is really there or not, but the concept of weight is valuable and useful for many purposes. When we consider the weight of an object, we are able to understand something about the force it exerts when it falls. We can make predictions about how the object will act under many conditions. By taking into account other concepts along with its weight such as its size and porosity we can predict whether or not it will float. In other words, utility is defined as the degree to which we are better able to understand how the event came to be, or what the conditions were which gave rise to the event and what predictions we can make about the future.

DIMENSIONS OF PERSONALITY THEORIES

The personality theorist observing behavior has an infinite number of possibilities open to him in attempting to describe commonality of different behaviors. If he observes a child crying he might characterize this behavior as an instance of emotional upset, frustration behavior, an indication of anxiety, as an expression of inadequacy feelings, as regression, or as an expression of conflict, etc. In fact, one of the things which characterizes a theory is the series of choices the theorist makes as to what constitutes the most important, most basic, or most valuable descriptive terms to be used. In the same way the person involved in assessing different personality theories has an infinite number of different dimensions open to him which he can use to compare theories. He too must make the choice as to what constitutes the most important or basic dimensions on which to compare the theories. In this chapter six dimensions will be used to contrast theories. In addition, the position of each theory in regard to two questions which do not lend themselves easily to simple dimensionality will be asked.

In the second part of this chapter five representative personality theories will be described briefly, and it is hoped that their comparison on the six dimensions and two questions will provide the reader with the basis for understanding their implications and potential utility.

Three of the dimensions to be used are formal dimensions having to do with the nature of the theory or system itself. They are: (*a*) systematic versus nonsystematic, (*b*) operational versus nonoperational, and (*c*) content versus process.

The other three dimensions have to do with explanation of the bases for individual differences or personality differences. They are: (*a*) constitution, heredity, and structure versus experience, (*b*) generality versus specificity, and (*c*) internal versus situational.

The two questions we shall ask about each theory are: (*a*) "What are the assumptions implicit or explicit regarding the nature of motivation or directional behavior?" and (*b*) "How is personality change explained?"

Systematic versus Nonsystematic. Theories in psychology can vary from a loose collection of statements to a tightly organized series of propositions mathematically interrelating a set of constructs and allowing for precise mathematical prediction. While some theories in limited areas do have such mathematical properties, there are no such theories presuming to encompass the area of personality. However, theories do vary greatly in their degree of systematization.

What makes one theory more systematic than another? Perhaps the most important characteristic of a system is that all of the constructs or terms used to describe events at a high level of abstraction are interrelated by a set of propositions, and the direction of the relationships is specified. For example, in psychoanalysis the statement that a strong superego prevents the full development of the ego links two constructs with the direction specified. The statement that a weak ego and strong superego leads to a high degree of anxiety links three constructs and states the direction of the relationship.

A second characteristic of a system is that the assumptions underlying the propositions which make up the system are made explicit, as they are in a mathematical system. Similarly, the phenomena, events, or aspect of events to which the system applies are made explicit. Finally, the generality (the level of abstraction) of specific constructs is made explicit so that propositions which are deduced from higher order propositions can be identified.

A system usually involves several levels of abstraction. Direct observations are used to define lower-level constructs, and the lower-level constructs are used in turn to define higher-level constructs. If the higher-level constructs are missing, then less generality is present or prediction must be more narrowly contained. If the lower level constructs are missing, then it becomes extremely difficult to test the hypotheses or relationships posited among the higher-level or more abstract constructs. Too few intermediate or lower-level constructs result in an inability to test propositions and to make predictions.

While too few constructs present a problem in some theories, others may be inefficient because of too many overlapping constructs. A system is probably never less predictive because it has more constructs, but it may be less efficient because many of the terms are overlapping, or have essentially the same referents, or have the same consequences as do others. For example, in some of the theories of psychopathology the antecedent conditions are presumably the same for many different disorders, and the disorders themselves are characterized by many overlapping symptoms. The result is an unreliability of diagnosis and an inefficiency of prediction.

Some theorists believe that the stating of precise relationships limits the development of a theory, and they justify the absence of general propositions relating constructs on the basis that not enough is known to specify relationships. But the explicit statement of relationships among constructs has many advantages. One of the primary advantages is that in tackling a new problem it is usually possible to deduce a hypothesis or make a prediction. In other words, systems have a great heuristic value in that they lead to research and focus the direction of research and observations thereby presuming to save time and effort over a trial and error procedure. A second advantage to a system is that when the research is done, not only are some empirical observations obtained, but a test is made of a general proposition or even a series of general propositions, so that the generality or utility of the findings is increased considerably. The third advantage of a system is that of the increased prediction which can result from a single or a limited number of observations. If in a given system a concept such as strength of a need for achievement is linked to many other constructs in a series of propositions, then the single measure of strength of need for achievement will permit the making of a great many predictions. The more systematic linkages of the specific construct the more prediction can be obtained from a single measurement.

Many psychologists feel that in a *predictive* science an additional characteristic of a good system is one in which the linkages include clear-cut statements of antecedents and consequences. Stated in other words, constructs need to be anchored at both ends. For any given construct the prior conditions to its appearance and the anticipated consequences of its presence are necessary for full prediction. However, if antecedents are pushed back in time, all psychological systems have to accept at least some constructs as givens or use descriptions of antecedents from another level of abstraction, such as biological, physiological, genetic, etc.

In summary, the following characteristics define a more complete and more adequate system:

1. Assumptions, postulates, or givens are explicitly stated.

2. The constructs are linked together into a network by a series of propositions stating the direction of the relationship between (among) the constructs.

3. As much as possible, time relationships are specified so that constructs are anchored both by antecedent conditions and consequent conditions.

4. Broad principles, generalizations, laws, or hypotheses relating higher-level abstractions are anchored to observations by a series of lower-level constructs providing a maximally efficient predictive organization.

5. Overlapping constructs, that is those with the same antecedents or same consequences, are eliminated to increase efficiency of prediction and reliability of measurement.

Operational versus Nonoperational. A personality theory to be useful should: (*a*) account for generally agreed upon observed data, (*b*) lead to new research or data collection, and (*c*) lend itself to empirical test so that the hypotheses can be accepted as valid, refined, changed, or rejected. In order to accomplish these aims, the terms, constructs, or abstractions used in the theory must be capable of measurement. The degree to which the theory provides operations for measuring its constructs or is inherently susceptible to the development of such measures can be considered as the degree of operationality of the theory.

A theory might provide specific operations for its constructs, rules or principles describing how operations for measurement are arrived at (i.e., by factor analysis), or it may define carefully enough a sufficient number of lower level constructs so that operations for measurement can be arrived at relatively easily by common sense or general consensus. The nature of the constructs in the system also relate to the degree of operationality. For example, if all the constructs in a theory refer to internal "feeling states" which cannot be directly observed and are not related in the theory to behavior, their operations would be difficult to achieve.

Criteria for good operations for measurement are generally agreed upon by scientists. They should be: (1) *intersubjective,* (2) *logically consistent with the ideal definition of the construct,* (3) *specific to the construct,* (4) *as quantitatively precise as possible,* (5) *practically possible,* and (6) *based on explicitly designated observables.*

The fact that some theorist has devised operations for some of his theoretical constructs does not make a system operational unless the operations at least approach the six criteria listed above. Many discussions of operational definitions slight the second criterion listed above, although it is most crucial for the test of theoretical propositions.

In discussing the operationality of various theories in the section to follow, a specific example of research or data collection will be presented as well as a general discussion. An attempt will be made to select a study which is frequently cited by adherents of that theory or would be regarded by them as representative of research stimulated by that theory.

Content versus Process. We have noted earlier that generally speaking personality theories have two purposes. One of these is to understand how individuals obtain or acquire, and change their characteristic behavior patterns. The second is to understand, predict, or describe individual differences. To satisfy the first purpose a process theory is necessary, which specifies the relationship of prior events (experience or inheritance) to later behavior and the conditions necessary for a change in behavior. To satisfy the second purpose one needs a content theory or a theory which specifies what are the useful descriptive terms which characterize the more general and consistent behavior of one individual versus another, allowing for prediction in what culturally would be considered a variety of important life situations. A learning theory which specifies only the abstract relationships between variables but does not group behaviors functionally into general units would be a process theory lacking content. A theory which only described eight or nine basic personality types and the characteristic behaviors of each type would be a content theory lacking in process relationships.

The dimension process versus content is not a true dimension but actually two dimensions since it is possible that a theory could be high or low on both. Obviously a theory which provides both process laws and content categories is potentially the most useful. A theory lacking either process or content would be seriously hampered in major areas of prediction.

Heredity, Constitution and Structure versus Experience. The approach to personality by an exclusive emphasis on experience involves both limitations and advantages. The major limitation is the inability to account in very early learning for differences in response to what are apparently similar environmental conditions. In other words, antecedents for early learning cannot be identified, and the theorist must accept some individual differences simply as givens. On the other hand the emphasis on experience (or learning) provides a logical basis to relate hypotheses regarding acquisition and performance to hypotheses regarding change. Knowledge acquired from one source of investigation is applicable to the other.

If a theorist attempts to explain the presence of particular characteristics of the individual on the basis of genetic inheritance, constitution, or physiological *structure* he is almost invariably assuming: (*a*) a strong consistency to behavior over time and (*b*) a relatively high limitation on the potential for change through experience. Once such an assumption is made and relatively little emphasis is placed on understanding how experience can change the individual, problems involving how he has acquired his characteristics are relegated to other fields, such as genetics and physiology. Such orientations typically lack process laws stating how the individual acquires his characteristics.

Since no one has isolated specific personality genes or provided any strong consistent evidence for the relationship of constitution or structure to personality—although some, notably Sheldon

(1940) have made attempts to relate constitution to behavior—prediction is extremely limited. Before one can determine what kind of personality someone will have, the pattern of behavior must appear and be identified. For such theories constitution, genetics, structure, etc., do not really provide an explanation but simply relegate explanation by assumption to scientific areas which hopefully can come up with meaningful relationships sometime in the future. In the case of constitution theories such as Sheldon's, body-build characteristics are not readily determined reliably at early ages, and in the case of females the system of predicted relationships between body build and personality seems to break down completely. In other words, for the most part attempts to explain or understand personality on the basis of genetics, constitution, or body structure are extremely limited, since they are not explanations at all but merely assumptions regarding antecedent conditions which cannot be identified prior to the characteristics they are supposed to predict. They are also limited in the degree to which the theory is likely to lead to research hypotheses or new knowledge regarding personality change and methods of psychotherapy.

Generality versus Specificity. The dimension of "generality versus specificity" refers to the degree to which a theory explains behavior in terms of one or a few broad general characteristics versus the degree to which a theory explains behavior in terms of a large number of specific and relatively independent traits or potentials to respond. The typological approach to personality is a good example of the highly generalized approach. In the typologies, if one were to either explain behavior or to make a prediction, it would be based on a single categorization of the individual. It is assumed that some single characteristic is the core of the individual's personality and permeates everything he does. What kind of psychopathology he is likely to have, how he dances, who he would like, and a great variety of other specific characteristics would all depend on his type. Of course, if such typologies were valid, they would be extremely efficient since almost everything the individual does reflects his typology; consequently it would be relatively easy to diagnose types. Once typed, a vast number of predictions would be possible. The major advantage of such systems, if they were valid, would be the great efficiency of prediction. In fact, they have severe limitations. Usually the empirical data do not support broad typologies as predictive in any sense and, in those instances in which they do, the level of prediction is very low. The systems themselves usually do not provide any basis for understanding individual differences within types, that is, for making differential predictions about the vast numbers of individuals who fall within the same category or type. If one divides the world up into extroverts and introverts, the vast important differences in behavior among all the introverts cannot be determined from such a simple system. The same thing would be true if the world were divided into those with positive self-concepts and negative self-concepts, those who are self-actualized versus those who are not, etc.

At the other end of the dimension the conception of personality is one of a large number of specific potentials to respond or traits, all relatively independent or not bound together by broader, more generalized characteristics. Such a view has both advantages and disadvantages of its own. Instead of a generalized characteristic of being dependent on other people, one could conceive of the individual as having a specific dependency rating in regard to all of the significant figures in the individual's life space. He is dependent on his mother at the 60th percentile, the 30th on his father, the 10th on his children, the 90th on his physician, the 40th on his friends, etc. If each one of these characteristics were assessed carefully, then specific predictions would undoubtedly be much higher than would prediction based on a generalized characteristic. Undoubtedly, there is specificity in everyone's reactions to all differentiated social stimuli or life situations. The more a system of personality takes this specificity into account, the higher its level of prediction will be. On the other hand, if it is necessary to make predictions to arrive at such specific measures of the individual's characteristics, then efficiency is at a very low level. One could not predict how hostile, dependent, recognition-seeking, anxious, bizarre, etc., an individual would act when faced with his wife from the way he behaved with a different social stimulus. To assess an individual's characteristics, a tremendous number of measurements would be necessary. While it is true that research in psychology shows with overwhelming consistency that each situation contains its own specificity, there is also a vast literature to indicate that varying degrees of generality exist in behavior. *In fact, the problem of developing a useful and valid content theory is largely one of* *finding the constructs which best describe that generality without assuming generality to be present when it is not.*

In summary, either extreme of this dimension involves severe limitations. The too general approach to personality leads to erroneous predictions or is limited to a very low level of accuracy. The extreme position of specificity involves making a great many measurements in order to make predictions and the loss of efficiency from potential predictions based on valid generalized characteristics.

Internal versus Situational. In the older views of personality each man was considered to be a member of some type, or perhaps a mixture of types, and his behavior was to be predicted on the basis of his categorization. In describing a person or a personality it was never necessary to specify the circumstances in which he behaved; he didn't *act* in a given way in a given situation but rather, he *had* a characteristic. Explanation was entirely in terms of an internal state, characteristic, type trait, or disorder. Kantor (1924) was one of the first modern psychologists to challenge this view of personality, which he defined as the reactional biography (or history of past reactions) of an individual reacting to a meaningful environment. Kantor felt that personality was an interaction between the individual and his meaningful environment rather than some entity which the individual carried around within his skin. A similar view was held by Lewin (1935). Lewin was less concerned with the so-called permanent characteristics which the individual carried around within him and more concerned with the dynamics of the field or life space in which the individual was acting. Brunswik (1947) also emphasized the importance of the situation in all psychological prediction and called for *systematic sampling of situations as an alternative to sampling individuals in a fixed or constant situation.* For some time some sociologists, anthropologists, and social psychologists [Mead (1933), Linton (1945), Newcomb (1950)] have tended to explain individual differences in terms of different roles assigned to persons by the social group rather than internal, relatively unchanging characteristics.

Internal versus situational determination of behavior is clearly related to the previous dimension of generality-specificity. However, this aspect of a theory can be independent of generality-specificity. A theory might list several hundred specific traits but never base prediction on the situation in which the person is acting. On the other hand, a theory might involve a few types whose behavior varied according to threatening versus nonthreatening situations, providing an interaction model but a highly general one.

The question can be put this way: "Is it sufficient to describe individual A as more aggressive than individual B, or is it necessary to describe A's and B's aggression as a characteristic of certain environmental or situational conditions?" In the latter case it would be said that A is more aggressive when treated unfairly and B is more aggressive whenever his status or position is threatened. Can it be assumed that situations have the same effect on individuals with stable characteristics and that it is not necessary to understand personality in terms of situational determinants, since simple multiplicative processes can be used to make finer predictions of how the individual would react in any specific situation. The evidence in psychology is clearly to the contrary. That is, the stability of an individual's behavior tends to be consistent for a particular kind or class of situations. An approach to personality which fails to recognize this would lack prediction for specific situations and be limited only to gross predictions with a very low statistical probability of accuracy. On the other hand, attempts to understand individual behavior in terms of the differences of the situations in which individuals find themselves neglects entirely the history of previous experience of these individuals, which seriously affects their perception of any meaningful life situation. What one individual finds stressful another one does not. What is rewarding or reinforcing for one individual is punishing for another. We might ask about any theory which attempts to predict behavior, to what extent does it utilize both internal and situational determinants? To the degree that it neglects either, prediction will be reduced accordingly.

Motivational Assumptions. Almost all theories of personality recognize the importance of directionality of behavior. The organism in reacting is perceived as attempting to change his environment in some way. Where do the directions come from? Some personality theories attempt to answer this in terms of an energy which pushes the individual from within to obtain certain goals. Usually these are characterized as drive theories. Some approaches do not attempt to explain the basis for unlearned positive and negative reinforcement but consider it sufficient to identify such reinforcements from the effects of their presence or absence on behavior. That is, they utilize an empirical law of effect for their motivational assumption. Still others use instincts or other forces not specifically labeled but which imply a natural direction to behavior which is characteristic of the human species.

Differences in personality as conceived from a motivational point of view can be described in terms of differences in the strength of drives, learned psychological needs, instincts or still broader life forces (such as a force for self-actualization, internal consistency, or growth). Individual differences can also be explained in terms of the results of inhibition of such forces. To understand fully and evaluate any personality theory it is necessary to examine the motivational assumptions, and particularly the degree to which these assumptions are subject to test.

Personality Change. One major characteristic of man is his constant attempt to improve himself and his environment. By education, child-rearing practices, and some form of psychotherapy he tries to provide a more satisfying life. In fact, one of the primary purposes behind the development of personality theories themselves is the attempt to understand man so that his behavior may be controlled or changed in a way that is more satisfying to himself and more beneficial to others. Once one has assumed that people do achieve some stable characteristics, how are changes accomplished? When does personality achieve relative stability? How much change is possible after stability has been achieved? These questions in fact are part of the general question of what is the process underlying the development and change of personality characteristics. If a theory has no clear-cut implications for how relatively stable characteristics can be changed, once the organism has acquired them, then it lacks utility for the broader problem of psychotherapy, which is increasingly becoming a central problem in clinical psychology and other fields dealing with the adjustment of the individual.

AN ANALYSIS OF FIVE REPRESENTATIVE THEORIES

The outlines of five approaches to personality theory will be described and evaluated along the dimensions proposed in the previous section. The limitations of space require that the theories be presented in minimal detail and with major emphasis on their theoretical structure.

There are numerous theories of personality, and many theories have in turn many versions. We can attempt here only to sample some of these. Orthodox psychoanalysis as presented by Sigmund Freud and Alfred Adler's Individual Psychology have been chosen not only because they are still widely followed and applied to a variety of practical problems, but also because they have both had a major influence on a large number of current theories which are generally categorized as Neo-Freudian or Neo-Adlerian and frequently are a compromise of the two older theories.

Carl Rogers' theory has played an important role in psychotherapeutic practices, and it may also be regarded as representative of a variety of theories which stress subjective experience as their basic datum. Gordon Allport's personalistic theory has been chosen as a representative of the more characterological and trait theories. While Allport's approach differs from the factor-analytic approaches to personality which are largely descriptive in nature [such as Cattell (1963) and Eysenck (1953)], it does provide a more complete picture of the development and nature of the human organism than other trait theories. The author's social learning theory has been selected as a representative of learning theory approaches because of its emphasis on the direct application of learning principles and its inclusion of a content theory also related to learning principles. Other approaches to personality from a learning theory point of view frequently deal only with process and are either contentless or attempt a loose *rapprochement* with some other content theory such as the Freudian, psychiatric nosology, or trait theories.

It is difficult to evaluate each theory in terms of its supporting experimental data since the present state of the field is such that most studies can be interpreted as supporting or challenging many different theories. In addition, the complexity of developing adequate operations and the difficulty of both knowing and being able to control relevant variables is so great that negative studies individually cannot reflect much information about a theory. Rather than trying to summarize the data obtained in support of, or against, a particular point of view we shall merely try to illustrate the kind of research which typically is used to support the theory and reflects the nature of the operations of which the theory is capable. Consequently, in discussing the operational characteristics of each theory, one investigation stimulated by the theory will be presented in detail to illustrate the nature of the data typically collected.

In describing and analysing these five theories an attempt will be made first to present the theory and then to evaluate it. The evaluation will be made under the three headings of systematic characteristics, operational characteristics, and content and process characteristics. Analyses of the content dimensions, motivational assumptions, and change implications will be included under these three headings.

THE FREUDIAN APPROACH

There is hardly a current approach to personality theorizing that has not been influenced to a large degree by the work of Sigmund Freud (1938). The terms "motivational" and "dynamic" have become value terms within psychology to such a degree that points of view which are not motivational or dynamic are frequently quickly rejected. There can be little doubt that Freud's contribution to understanding human behavior has been enormous. However, these contributions appeared at a time when the scientific aspects of psychology were just beginning to be developed and relatively little was known regarding research methodology or system construction in the field of psychology. Consequently, the current status of the theory needs to be evaluated independently of its unquestioned historical importance.

It is difficult indeed to describe or characterize psychoanalysis because the term means so many different things to different people. Textbooks have long classified Freud, Adler, and Jung as representing the psychoanalytic schools, although many Adlerians feel that they have little in common with Freudian theory. In addition, there are many modern variants of classical psychoanalysis (the theories of Horney, Sullivan, Kardiner, Alexander and French, and others). Many times criticisms of psychoanalysis in turn are criticized as being naive because they do not take into account the new or modern developments of psychoanalytic theory. But some of the more classical psychoanalysts, such as Wittels (1939), supported by a study of Ansbacher (1953), have characterized many of these modern variants as "Neo-Adlerian" and essentially not psychoanalysis. It is true that many current theories consider themselves as psychoanalytic theories, despite the fact that they have borrowed liberally from Adler, learning theory, sociology, and other sources. Sullivan's (1953) point of view is a case in point.

Not only is there obfuscation in the use of the term "psychoanalysis" because it is applied to so many different theoretical positions, but also within the field of classical psychoanalysis the term applies to three different kinds of statements. These three are only loosely related, so that general statements about the adequacy of psychoanalysis frequently lead to confused communication because the speaker and listener are thinking of different aspects of psychoanalysis. The first aspect derives from empirical observations made by Freud and his followers, many of which have contributed greatly to our knowledge of human behavior. These include such observations as the meaningfulness of dreams, of errors, or slips of the tongue; the strong relationships often existing, at least in our society, between parents and children of the opposite sex; the relationship between feelings of guilt and inadequacy and what is generally referred to as projection; the tendency that some people have to "forget" unpleasant past events and so on.

The second aspect of psychoanalysis has to do with a personality theory, a theory of the development of personal characteristics dealing particularly with what has sometimes been called the person's emotional life. The third aspect of psychoanalysis is a method of psychotherapy or treatment which, as Alexander and French (1946) have pointed out, has gone through a series of stages. The orthodox form, however, may be characterized as a long-term technique averaging usually three years or longer in duration, and involving methods such as free association, analysis of the past, dream analysis, and the interpretation of the transference neurosis. We are concerned here only with the second aspect; namely, the classical personality theory.

The personality theory itself may be thought of as having different parts. One part might be referred to as the theory of instincts and drives, including the theory of psychosexual development. A second segment of the theory, only loosely related to the first, is the theory of the mind entities. From the combination of these two, two additional facets of psychoanalytic theory have been developed. One may be referred to as the mental mechanisms, the second as the character types.

Basic to Freud's personality theory are two assumptions, both of which are widely regarded as two of his major contributions. The first is his principle of *psychic determinism*. In contrast to faculty notions of personality and typological theories which assume that the important characteristics of human behavior are simply typical of the species, or part of general nature, Freud felt that human behavior was motivated or directed toward obtaining specific goals. He stated that all of human behavior, including psychopathological behavior, had particular significance or meaning that could be understood in light of the person's motives. The person himself strives only to obtain gratification of instinctual drives and to avoid frustration of these drives.

The second major assumption is that of *unconscious motivation*. While other psychologists and philosophers had recognized that man was moved sometimes by unconscious motives, none gave this idea such prominence as did Freud. He postulated an unconscious mind, not only as a repository for many of the instincts for pleasure and destruction, but also as a repository for ideas, feelings, or wishes which the individual would not accept about himself—associations, ideas, or desires he strives to forget or avoid.

It is not surprising, in light of Freud's medical background, that he should begin his working theory of personality with the concept of physiological drives. Freud, wishing to account for behavior logically, sought an explanation in terms of some principle of energy that compels the individual to action. He postulated, therefore, a conversion of energy derived from physiological, biological, inherited, or instinctual processes into mental energy. Mental energy acted in a fashion similar to physical energy. Thus Freudian theory was dualistic, but dualistic in a fashion characteristic of his time.

Freud postulated two general sets of instincts: first, the life or sex instincts; second, the death instincts. The death instincts were later developed in *Beyond the Pleasure Principle* (1922) in an attempt to account for phenomena such as masochism and what Freud referred to as the "repetition compulsion," which did not seem to fit readily into his previous formulations. Modern variants of psychoanalysis to a large extent have now rejected Freud's postulated death instincts, but they have not rejected the instinctual approach itself. Essentially, this is a method of explaining a phenomenon or behavior by relegating it to a hypothesized energy source that is inherited but that is not itself directly measurable or predictable. Questions such as why children around the age of six show considerable attachment to parents of the opposite sex, why people become aggressive following frustration, why many people show sexual attraction to the same sex, and so on, are answered, "They have inherited an energy source that directs them to do it." Freud did state that the particular form which the expression of the energy took was perhaps a function of learning or experience and that behavior itself, over a long period of time or in adulthood, was determined by what happened to the person when these instinctual energies were expressed in early childhood. To the ultimate "whys" Freud could say little other than "because it is an instinct."

Not only did Freud use instincts to account for the behavior of the infant, but, as behavior changed in the developing child, Freud frequently sought new instinctual sources of energy, sources equally inherited but latent until later development. So, whereas at first a child has an instinctual urge for one kind of sexual gratification, later on he seeks another kind. This theory of psychosexual development proposes that there is an instinctually determined object for gratification of sexual drives, that this object changes with development, but that it may be affected by experience. The stages described by Freud in his later writings included first an oral sucking stage, then an oral biting stage, then the stage of anal gratification, an urethral state, and finally a phallic stage.

The frustration of instinctual needs usually leads to aggression. However, if the aggressive impulses are not acceptable, they are repressed. Continuous frustration may lead to a regression to an earlier and more satisfying stage of development.

Along with these stages go common conflicts or problems resulting from interaction of drives for gratification with the environment. The oedipal conflict (and, accompanying it, castration fear) is considered universal. The phallic phase itself is divided into latent, homosexual, and finally heterosexual phases. It is inevitable that, with such an attempt to explain what might be called normal development, a person who behaved abnormally would be characterized either as having too much or too little of an instinct or as having undergone some crucial experience during the phase in which that instinct was dominant, which interrupted or fixated the normal course of development. Freud initially used the first type of explanation but this later gave way to the fixation theory. It might be noted at this point that, whereas the typologies explained by relegating to a classification, Freud explained by relegating to an instinct.

The second part of psychoanalytic theory deals with the mind entities. Freud's training in hypnosis with Charcot and Bernheim led him into an attempt to explain the phenomenon of hypnosis by dividing the mind into different parts that might function relatively independently of one another. First, he postulated an unconscious mind, a foreconscious mind, and a conscious mind. As he and Joseph Breuer experimented with hypnosis and suggestion, Freud began to attribute to these hypothesized parts of the mind different kinds of content and different functions. He endowed the unconscious with the ideas that a person apparently could not accept about himself—the associations or ideas he strove to forget or avoid—which Freud referred to as repressions. He also placed in the unconscious the tendencies or pressures for gratification of instinctual urges. Later Freud reconceived the nature of the mind in terms of forces acting upon the person and frequently warring within him for control. This division of the mind did not supplant, but merely overlaid, his earlier division.

Freud labeled the energies striving for immediate sexual gratification as the *id;* these forces are unconscious or lie in the unconscious mind. The instinctual urges toward self-preservation and toward the avoidance of punishment result in a series of learnings or perceptions that gradually become differentiated as the *ego* and are in part conscious. Finally, from the ego itself is differentiated a *superego* having to do with the avoidance of punishment from parents by the "introjection" of parental standards and ideals. It is concerned with keeping the child's instinctual urges in check to avoid loss of love from the parents and parent substitutes. It is primarily the superego which, in the child's later development, opposes the id in a kind of constant warfare. The superego, like the ego, may also be partly conscious and partly unconscious.[1]

The third aspect of Freud's theory deals with the dynamisms or mental mechanisms or what have been more broadly called "defenses." The mental mechanisms have to do with the behaviors that develop from the warfare among the ego, id, and superego as a result of instinctual drives or converted instinctual drives operating for expression or gratification. In this way repression is a technique of the ego to control bad instinctual impulses. Rationalization and projection are ways the ego has of defending itself against failure or fear of external threat. Guilt is a mechanism of the superego that occurs as a result of danger to the internalized standards. Neurotic anxiety results from the superego becoming aware of the impending escape of unacceptable id impulses and warning the ego to repress the impulse. Reaction formation is a superego mechanism that prevents the expression of "bad" impulses. Conversion symptoms, dreams, and symbolic behavior are means by which the id obtains gratification or the release of repressions and, at the same time, avoids threat to the ego or superego. In "conversion hysteria" the "mental energy" becomes reconverted into "physical energy" and is expressed as "physical" symptoms.

Although maladjustment in the Freudian schema was originally thought of as being expressed in symptoms resulting from repressions or converted unconscious drives, later emphasis stressed the fixation of personality at one of the early stages of psychosexual development. Apparently such fixation could occur as a result of either trauma, frustration of the normal expression of the instinctual drive, or overgratification of the instinctual drive. For example, a person could become fixated in the oral stage of development if he were weaned too soon, too late, or too suddenly, and similarly for the other psychosexual stages. When a person's behavior was thus fixated at an early stage, he supposedly continued throughout his life to attempt to continue to gratify the instinctual urge dominant at the point at which the fixation occurred. It is apparent that Freud's energy concept breaks down here. It seems that whether an instinctual energy source is allowed too much or too little expression, it continues to persevere indefinitely, and as a result of this fixation a person may seek to gratify this urge for twenty or thirty years but still not diminish it. It is extremely difficult to maintain Freud's analogy of physical energy for such a conception.

---

[1] More recently a group of psychoanalysts such as Heinz Hartman (1958), Ernest Kris (1950), and David Rapaport (1959) have placed greater emphasis on the ego and its role in the psychic life. For these "ego psychologists" the ego is given independent status and emphasis is placed on the organism's attempt to cope with reality, the integrative functions of the ego, and the degree to which the ego defends the organism against punishment and failure. Such ego defenses may also distort reality in a manner characteristic of the individual.

This later development in psychoanalysis also resulted in the development of typologies. In this case the types are based not on instinctual or genetic grounds but on fixation as a result of early experience. These types, however, have the same limitations of being unable to account for change and of trying to explain too much with too little.

ANALYSIS OF FREUDIAN THEORY *Systematic Nature of the Theory.* Freud is generally clear on the basic assumptions of his theory. The most important ones are psychic determinism and the assumption of unconscious psychological processes. The third major assumption of his theory is that of instincts. That is, behavior as perceived from a psychological point of view, must be traceable back to biological causes. This is essentially reductionism and reflects the dualism of Freud's time. Freud felt that in order to explain motivation or behavior he must postulate a physical source of energy, which, in some mysterious way, was converted to "psychic energy," and sometimes converted back again to physical energy, particularly in symptom formation.

Possibly the greatest difficulty with psychoanalysis as a scientific language of description is its dependence upon instinctual drives that are presumed to be present but cannot be predicted in strength or time of occurrence on the basis of antecedent conditions. Neither may their presence be directly measured, since they may manifest themselves without disguise, be present but repressed, or be present but occur in many different symbolic ways that require subjective interpretation on the part of the clinician. On the other hand, the potential for understanding and making predictions about behavior on the basis of a motivational principle is now widely accepted. The construction of psychological needs, empirically observed, objectively definable, and predictable in terms of previous experience and antecedent conditions, may be of primary significance in the development of any adequate personality theory.

In general, Freudian theory is remarkably complete in the network of relationships among constructs. The defense mechanisms are tied to the energy systems of the various mind entities. The development of the psychosexual instincts through stages is related, in turn, to the development of id, ego, and superego. If fixation takes place too early in the child's life, the ego tends to be weaker; the formation of the superego is dependent upon the satisfactory resolution of the oedipus complex. The nature of the dominating id drives is dependent upon the satisfactory completion of earlier psychosexual stages. The relationships are explicit with a direction of relationship specified for the higher-level constructs. However, interestingly enough, as the constructs get closer to specific behavior, the nature of the relationships becomes less clear. Repressed energies lead to sublimation, reaction formation, symbolism, etc. But it is not clear whether this occurs all of the time, some of the time, or why one mechanism dominates in one person and a different one in another. A strong ego is more likely to lead to sublimated id energies and to reaction formation, but which id impulses would be sublimated or lead to reaction formation and which would not is not clear. In other words, the direction of the relationships between the constructs tends to be less explicit as the level of abstraction of the constructs tends to get closer to observable behavior. Related to the problem of the specificity of relationships is that of time sequences and the statement of antecedent and consequent conditions. The picture is not one of uniformity. While antecedents are described for many constructs, such as "oral fixation," "failure to resolve the oedipus complex," "anxiety," many of the constructs do not have clear-cut antecedents. Instincts, the oedipus complex itself, castration fears, etc., are givens and, to a large extent, classical psychoanalytic theory assumes what other theories might try to explain. In many cases where antecedents are given, they are given in such a way as not to be particularly useful. For example, the antecedents for oral fixation are either too little gratification, too much gratification, inconsistent gratification, or traumatic weaning. Since these conditions are not in themselves carefully described, on a *post hoc* basis they can be applied to almost anyone. If the superego is strong, repression will take place. But where does repression come from? Why does frustration lead to aggression? Why does continuous frustration lead to regression? The mechanisms, in other words, of psychoanalysis are also givens without clear-cut descriptions of antecedents. They are instinctual behaviors as additions to the instinctual energies.

The relationship between observations and higher-order constructs, as mediated by intermediate-order constructs, is fairly well specified. A forgotten experience is linked through repression to a strong superego and an instinctual urge. A strong tendency toward passivity is linked to oral fixation and to early childhood experience. In other words, the system is efficient in the sense that observations provide a maximum of information in regard to the strength or value of systematic constructs at different levels of generality. While the method of obtaining data itself, and the validity of the linkages may be in question, the thoroughness of the system allows the investigator to make a large number of very general hypotheses about the subject from relatively little data.

Operational Nature of the Theory. Classical psychoanalysis presents many constructs which, by their very nature, are difficult to operationalize. This follows both from the fact that it is assumed that there are unconscious processes which the individual cannot describe himself, and unconscious drives which are rarely, if ever, expressed in a direct fashion. How can you tell if an individual is repressing something if, in fact, this repression can lead to many different kinds of expressed behavior? How does one know when an instinctive force is weaker or stronger? If someone likes to paint, is this a naturally learned activity, or a sublimation of a sexual urge? Is the smoking of a pipe evidence of oral fixation or simply the result of the efficacy of the socialization process? Is a sloppy desk a result of too much work, or of oral fixation? Is the *expression* of guilt a technique of the ego used to avoid punishment or an indication of an overly strong superego? Does the tendency to overindulge a child reflect, on the part of the mother, a strong love for the child or is it a reaction formation to an unconscious rejection? Because the antecedents for a given construct are frequently numerous and poorly defined and the consequences are also numerous and poorly defined, it is extremely difficult to arrive at operations which are universally agreed upon as adequate for measuring a given construct or for determining or testing the relationship between two constructs. Frequently, operations which would be more widely accepted are ones which cannot be manipulated experimentally, for example, a weaning trauma or a rejecting, punitive father. Consequently, the operations, particularly those used for experimental purposes, are rejected as superficial and not sufficiently related to the theory by many of the practitioners of the theory. The difficulty is well described by Rapaport in his analysis of Freudian theory, written from a point of view distinctly sympathetic to psychoanalysis (1959, p. 142).

"Most of the experimental evidence for the theory is questionable, even if Sears' (1943) survey, which was loaded with negative bias, was inclined to accept some of it as positive evidence. The overwhelming majority of experiments designed to test psychoanalytic propositions display a blatant lack of interest in the meaning, within the theory of psychoanalysis, of the propositions tested. Thus most of them certainly did not measure what they purported to; as for the rest, it is unclear whether or not they did. Even where the findings appear to confirm a relationship posited by psychoanalysis, the experiments usually tested only an analogous relationship on a high level of the hierarchy of psychological organization. It is not that all these experiments are useless as confirming evidence, but rather that at this stage of our knowledge it is not clear what—if anything—they confirm. It is hard to share Hilgard's (1952) enthusiasm for most of the experiments he considers relevant and confirming. It is likely that some of the experimental findings will fall into place when ego psychology has clarified the hierarchic relationship which obtains in psychological organization. Command of the theory should help toward making the results of future experiments unequivocal, but it is not as much of a guarantee of success as ignorance of the theory is of failure. The experimental psychologist who enters the precincts of psychodynamics meets the same complexities which the clinical observer has been struggling with for over six decades. There are no 'easy pickings' and the 'experimental method' has no magic here."

An investigation by Frieda Goldman-Eisler

(1959) on oral character traits provides a good illustration of an attempt to operationalize some aspects of classical Freudian theory. Working primarily with Abraham's (1942) and Glover's (1925) writings on oral character fixations she set about measuring these traits and determining whether or not they related to information regarding the duration of breast feeding. Abraham and Glover have distinguished two orally fixated types: the gratified type who is characterized by optimism, generosity, sociability, flexibility, and ambition and the nongratified type who is characterized by pessimism, depression, attitudes of withdrawal, a passive-receptive attitude, feelings of insecurity, lack of confidence, ambition which is combined with a feeling of unattainability, sensitiveness to competition, and a dislike of sharing. Presumably the latter type would be related to early weaning trauma and the former type to longer periods of breast feeding before weaning took place. Theoretically it is not clear why fixation would take place for the orally gratified type. According to these writers the nongratified type, frustrated in the oral sucking stage, would compensate by fixation at the subsequent oral biting stage.

Using questionnaires developed by Murray and adding some of her own, Goldman-Eisler developed a questionnaire measure of nineteen traits which were related in the psychoanalytic literature to either the oral pessimism or the oral optimism type. Each scale was composed of an average of eight items, and the corrected internal consistency of these ranged between .56 and .90.

Goldman-Eisler found these traits fell into two clusters around the antithetical traits of optimism-pessimism. Table 12-1 shows the correlations of the nineteen traits with the optimism and pessimism subscales of the questionnaire.

The nineteen subscales were then scored according to agreement with a "standard type" following an adaptation of a technique of Burt (1940). A single score of *oral pessimism* was then derived.

Goldman-Eisler obtained information about weaning by questionnaire from the mothers of 100 adult subjects. Ten of these subjects who were bottle fed from birth were not included in the data analysis. The other subjects were divided into groups based upon their having been weaned at four months or earlier versus five months or later. A second analysis was made with only those subjects who were weaned four months or earlier versus nine months or later. A correlation of .27, which was statistically significant, was found between early weaning and oral pessimism. If only subjects for the late weaning group were used who were weaned at nine months or more, this correlation rose to .31. Contrary to the writings of Abraham, those subjects who were weaned after the ninth month did not show the same characteristics as the early weaned or traumatic group.

Some limitations of the study are pointed out by the author. The arrangement of traits was based primarily on empirical considerations rather than theoretical ones, since, as the author says, different writers disagree on the traits associated with the two types of oral fixations, and they are not clear as to the relative importance or centrality of the different characteristics. Goldman-Eisler also points out that the investigation does not

TABLE 12-1

Correlations of Nineteen Trait Subscales with Optimism and Pessimism Subscales*
(N = 115 Adults)

Traits	Measurement of Traits	
	r with Pessimism	r with Optimism
Pessimism		−.63
Passivity	−.03	−.48
Aloofness	.37	−.37
Oral Aggression	.11	−.36
Endocathexis	.20	−.29
Autonomy	.13	−.23
Aggression	.11	−.31
Guilt	.38	−.15
Dependence	−.02	−.10
Conservatism	.16	.21
Unattainability	−.02	−.34
Impulsion	−.10	−.38
Deliberation	.27	−.11
Change	−.14	.16
Ambition	.05	.27
Sociability	−.34	.20
Nurturance	−.22	.46
Exocathexis	−.55	.51
Optimism	−.63	

* From Goldman-Eisler, Frieda. Breastfeeding and Character Formation, in Kluckhohn, C., and Murray, H. A. *Personality in Nature, Society and Culture.* New York: Alfred A. Knopf, Inc., 1959, p. 155, by permission.

necessarily demonstrate a correlation between weaning trauma and personality characteristics but that the low level of statistical relationship might obtain as a result of other factors such as the attitude of the mother, the constitution of the child, and/or the interaction of both. In any case, the amount of variance associated with length of weaning, although significant, was quite small.

Content versus Process. One of the reasons why psychoanalysis has had such great appeal to clinicians is that it provides a relatively complete process as well as a content theory. The process theory is necessary for deriving psychotherapeutic procedures; the content theory to characterize individual differences. Individuals differ in the amount of instinctual drive, in the strength of the competing internal forces of id, ego, and superego, in the kinds of defenses they use, in the cathexis of the libido for different kinds of sexual objects, and in the traits which characterize fixations at different stages of psychosexual development. In other words, Freud is rich in content variables. Although he has not systematically defined referents for most of these variables, he has illustrated them all in abundant case material.

The range of process and content variables runs from the specific to the general. A particular symptom involving a symbolic reaction can be traced to a relatively specific situation, but the nature of the reaction in that situation is generally explained in terms of fairly broad or general characteristics of the individual. On a dimension of generality-specificity Freud is well over on the generality side, but the explanation of particular specific psychopathological symptoms is frequently related to particular experiences via process laws.

How much variance does Freud attribute to the situation in behavior? For Freud the dynamics of personality are internal dynamics. The war between the id, the ego, and the superego, the instinctual and invariable relationship between frustration and aggression, and the drives of internal origin are the major determiners of behavior. However, Freud does implicitly distinguish between some different kinds of situational variables. These are sex and authority relationships. That is, Freud, in describing his cases, takes cognizance of the situation in the form of people expecting different kinds of behavior in response to males versus females and to authority figures versus nonauthority figures. At least to this extent, situational variables play a role, for Freud, in the prediction of behavior.

For Freud it is clear that motivation is based on inherited instincts. These instincts are in turn related to biological drives. Energy from the biological drives becomes converted to "psychic energy." Behavior tends to be fixated in childhood as a result of the interaction of the instinctual energies and the environment. While specific behaviors may change as a result of experience, the major personality variables are considered to be very stable after the first six years of development. Extensive change in personality can take place only as a result of a thorough psychoanalysis which is likely to last several years, in order to change the early acquired character structure. Even with extensive psychoanalytic treatment Freud, and most analysts, see limits to the amount of change possible. The instincts are fixed. Insight into the method of expression of instinctual forces allows for increased ego control and change, but character fixations are considered to be extremely difficult to eradicate. Therapy is frequently oriented as much toward adjusting the individual to his own character deviations as it is to changing them.

THE ADLERIAN APPROACH

Alfred Adler was an early colleague of Freud's who later broke with him on several important theoretical issues. Adler (1924) founded his own school of thought which he referred to as "Individual Psychology." Adler retained Freud's idea that behavior is motivated or directed toward some goal, and he recognized that the individual is frequently unaware of his own motivations and the meaning or significance of his behavior. He did not, however, divide the mind into a conscious and unconscious or into different instinctual energy systems.

What Adler objected to most in Freud's system was the emphasis on the sexual motivation as primary both in infants and adults and the notion that instincts themselves are determiners of behavior. At a more specific level, Adler objected to Freud's almost exclusive emphasis on the parents' behavior as determining the character and adjustment of the child. Adler emphasized instead what he called the dynamics of the whole family and placed much more emphasis on the importance of sibling position, sibling rivalry, and the family constellation in general. He perceived that two children with very similar parents, one of whom was an only child and the other a middle child of three or more, would have markedly different personalities. In place of instincts as the motivators of behavior, Adler asserted that since all children were born helpless into the world, unable to feed themselves or do anything to obtain gratification and had to depend on others to survive, everyone developed a feeling of inadequacy or inferiority. He felt it was a biological characteristic of man and most animals to make up for their weaknesses, that is, to compensate and/or overcompensate for deficiencies. Since everyone feels inadequate or inferior, every person attempts to compensate or overcompensate for the felt weaknesses by striving for superiority, power, or strength. Each child, as a result of his own unique experiences, learns to do this in a different way depending upon what he perceives around him as a means to power. The means he develops to struggle for superiority Adler referred to as "the style of life."

In the development of a neurotic or pathological style of life, Adler felt that the child misinterpreted his environment. Some examples follow: A boy seeing a younger sister favored by their parents might decide that in order to be loved and powerful he must be like his sister by being feminine. The child discovers, because he observes his parents quarreling all the time, that the one who shouts the loudest and longest wins the arguments, so he believes he must argue and fight for everything until he gets it. Another child feels rejected and pushed aside by his parents in favor of a sibling who displaces him, but he discovers that whenever he gets sick his parents once again give him all the attention and love he had before the other child came. As a result, he develops many physical complaints and becomes a "delicate child."

Adler would say about the three children described above that they had all developed a "mistaken style of life." Seeing the world from their own unique position, without adequate experience, led them to crystallize a distorted view of the whole world. Once they had developed a given style of life, they failed to learn from new experience because they interpreted all new experience along lines that they had already developed. In other words, they perceive new events in a distorted manner. Consequently, they never unlearn their "mistakes."

Adler saw most neurotic patients as lacking "courage." In their struggle for superiority they were afraid to fail and their symptoms were actually defenses against failure. Getting a headache before an examination, using the excuse of a sick parent to avoid the problems of marriage, as well as many of the more bizarre and uncommon symptoms of psychosis are seen by Adlerians as defenses or techniques the patient used to set a *distance* between himself and his goal to avoid a test of his adequacy.

The concept of distance is Adler's concept of the defense behaviors. Although Adler used notions such as projection, repression, rationalization, compulsive behavior, and obsessive thinking, he made no special attempt to differentiate these by their antecedent conditions or to explain them as involving different kinds of dynamics. The concept of distance included all techniques that the patient used in avoiding a test of his adequacy—all attempts to avoid failure or the recognition of inadequacy.

Adler recognized many sexual difficulties in his patients, but he saw these not as the result of the clash of instinctual energies and society's repressive influence but rather as secondary outcomes of the struggle for power. He felt that, in a culture where males dominated, boys were given more privileges and more freedom, men achieved status and recognition, and women had an inferior position, that many girls would see the goal of superiority as that of becoming more masculine. They would seek, in other words, to dominate by whatever techniques they had at their disposal. They would perceive men as rivals rather than as potential partners in a cooperative marital relationship. Such an attitude of *masculine protest* on the part of women Adler saw as basic to many kinds of sexual problems. Later Adler recognized that not only do women strive for masculinity but that the men in a masculine-dominated culture also strive for masculinity. They too feel that in order to be accepted and recognized they must be strong, powerful, sexually adequate, successful, etc. Feeling inadequate as boys in the world of men many would develop styles of life oriented around this striving for masculinity and power. However, the feelings of inadequacy, if they were too great, would lead again to the individual setting up a distance between himself and his goal, and in these defenses against failure many of the sexual problems of men would arise. In some cases, through the special circumstances of a family dominated by the mother or where a male child found himself dominated by several older sisters, Adler also spoke of a feminine protest. In these instances, either boys or girls might see the pathway to power as one involving excessive femininity. Male homosexuality might, therefore, result either from a defense against failure or from a feminine protest, and female homosexuality from a strong masculine protest.

In specifying the particular childhood conditions leading to maladjustment, Adler placed great emphasis on pampering and rejection. Pampering was that kind of parental behavior (through indulgence, overprotection, or overdomination) which affected the child so that he failed to develop independence and responsibility. The pampered child was likely to be egocentric, selfish, dependent, and to try to maintain a one-way relationship with others in which he gave little but took much. Pampering not only depended upon the attitude of the parents but also on the child's sibling position, so that only children were most subject to pampering and so were oldest and youngest children separated from their siblings by several years.

The rejected child became independent early and felt that he must stand on his own feet and could not trust others to help him. But along with this independence was the feeling that society provided him with nothing and that he had no responsibility to society. While the overly pampered child might be expected to become neurotic, the rejected child's experience provided a psychological basis for delinquency and crime.

Adler finally added to his theory another major concept which he referred to as "social interest" (Adler, 1939). Social interest was a feeling for others, a motive to contribute to society, and he saw social interest as opposed to self-interest and the struggle for power. Adler was never clear as to whether such a major goal of the individual was inherent in the species or was inherent in the nature of society. However, he implied that all humans were capable of such social feeling and that the effect of experience was to develop the potential that was there rather than to create it. Both pampered and rejected children tended to fail to develop social interest, and for Adler the outcome of successful therapy involved the person giving up his struggle for power and developing his feeling for others. The concept of social interest, however valuable, was not well integrated into the rest of Adler's constructions and appears to be the introduction of a value concept lacking in clear-cut logical antecedents.

ANALYSIS OF ADLERIAN PSYCHOLOGY *Systematic Nature of the Theory.* Adler is explicit in the origins of two of his three most important assumptions. The feeling of inadequacy or inferiority is a function of the dependent nature of the human infant, his helplessness, and his protracted period of dependence. The striving for superiority or power is a natural biological consequence of experienced weakness or inadequacy. The nature of the assumption of social interest is not as clear.

For the most part Adler has rid his theory of instincts and dependence upon constructs that in themselves cannot be tested. At an earlier time he was considered an environmentalist, and for him experience played the major role in personality development.

As a systematist, Adler is much less orderly than Freud. He appeared to have little concern with setting up specific terms, carefully defined, with their relationships to other concepts clearly indicated. As a result, the concepts are quite general in nature and there are many overlapping terms. For example, "the struggle for personal superiority" and "masculine protest" are somewhat overlapping terms; so are "lack of common sense," "neurotic style of life," "mistaken style of life," and "pampered style of life."

Another weakness of "Individual Psychology" as a system lies in the absence of more specific lower-level constructs. For example, while the style of life is an important construct in Adler's theory, there are no specifications of different kinds, classifications or characteristics of different styles of life. The prediction and description of individual differences, consequently, is limited. Pampering also is a rather general concept and many different kinds of behavior, such as overindulgence, oversolicitude, overcontrol, etc., all of which lead to somewhat different characteristics in the child, are not clearly specified as separate lower-level constructs. Only when Adler talks about the nomothetic characteristics of different sibling positions does he provide the kind of specific description which leads to the prediction of individual differences and a test of hypotheses. While Adler's insights into human nature are great, the theory is so general in nature that its utility is limited.

Operational Nature of the Theory. The general nature of Adlerian concepts, as has been described above, makes operational definitions difficult. If one cannot specify the characteristics of a kind of style of life and accepts rather the uniqueness of each individual's style of life, measurement and prediction are extremely difficult.

The important ideas of pampering and overprotection are susceptible to operational definition and measurement (Rotter, 1962), although Adler himself did not specify exactly what these operations should be. Concepts such as social interest are particularly difficult to operationalize. It is not clear whether an individual who does have a high degree of social interest is characterized by an attitude, or intent, or by his observable behaviors. If it is the latter, are the behaviors those which others in his immediate society characterize as useful, or can they be behaviors aimed at overthrowing existing beliefs? If they are unconventional, their social interest value can be so evaluated by others only after a considerable passage of time. Similarly, concepts such as selfish, egocentric, struggle for power, are sufficiently broad that their interpretation could be a matter of considerable controversy.

One attempt to test a hypothesis of Adler's relating to overprotection and sibling position was made by Clara Rotter (1942). Adler (1930) had noted that pampered children frequently had difficulty in arithmetic, and he felt that since the pampered child was lacking in courage he would give up easily in a subject which he found difficult or in which he did not have an advantage. This was less likely to happen in reading or subjects related to reading since the overprotected child was likely to have more verbal stimulation than other children. His observations had been supported by similar ones by David Levy (1933). To test this hypothesis on a large scale Rotter operationalized the concept of pampering by measuring sibling isolation. She placed in one group those children who were separated by five or more years from their nearest sibling, including only children. A second intermediate group included those children who were separated at least by three or four years from their nearest sibling, and a third nonisolated group included those children who were separated by two years or less (including twins) from their nearest siblings. She obtained two samples of children from school systems in different states where previous records were available on reading, comprehension, and arithmetic computation from standard achievement tests. If Adler's statement were correct, the longer the pampered children were in school the greater should be the discrepancy between their reading ability and their arithmetic ability, if more and more of them would give up competition (set up a distance) in the arithmetic area when they encountered difficulty.

In her sample of children from a small city in Indiana she found sixth grade children who had taken achievement tests at the third grade and again at the sixth grade. A somewhat larger sample of children was obtained in Massachusetts from a comparable size city. In this sample achievement test scores were available for the fourth, sixth and eighth grade for the same chil-

TABLE 12-2

Differences in Standard Score Units between Reading and Arithmetic Achievement for Isolated versus Nonisolated School Children, in Two Communities*

Sample	Sibling Isolation	N	GRADES											
			III			IV			VI			VIII		
			R	A	Diff.	R	A	Diff.	R	A	Diff.	R	A	Diff.
Indiana	5+	55	+.16	+.13	+.03				+.23	+.09	+.14			
	4-3	43	−.09	+.04	−.13				+.01	−.05	+.06			
	2-0	59	−.09	−.15	+.06				−.24	−.05	−.19			
Massachusetts	5+	53				+.19	−.03	+.20	+.16	−.10	+.26	+.40	+.15	+.25
	4-3	44				+.13	+.20	−.07	+.18	+.11	+.07	+.15	+.15	.00
	2-0	81				−.18	−.08	−.10	−.19	+.01	−.20	−.34	−.17	−.17

* From Rotter, Clara B. The Relation of Sibling Isolation and Socio-economic Status to Reading and Arithmetic Achievement. Unpublished M.A. thesis, Indiana University, 1942, by permission.

dren. Excluded from her sample were children with IQs below 70, seriously handicapped children, and children who were not living with their true parents. All scores were converted into standard scores. Her findings are shown in Table 12-2.

The important data in the table are found in the columns marked difference, which show the relative superiority of reading over arithmetic for that particular group.

The results from the two samples are remarkably consistent. Initially, differences between reading and arithmetic are small for the three groups differentiated by amount of sibling isolation. However, these differences increase as the children go through school and by the sixth and eighth grades the isolated children are significantly better in reading when compared to arithmetic than are the nonisolated children. Apparently, the longer the isolated child goes through school the better he is in reading in contrast to arithmetic, while the opposite holds true for the least isolated group. Socioeconomic status in this study was found to be highly related to the overall level of achievement but did not account for the relative difference between arithmetic and reading for the three groups.

While this study was successful in supporting one of Adler's observations and in operationalizing the concept of pampering, it should be noted that other concepts of Adler's are much less amenable to such relatively simple operations.

Content versus Process. Adler provides some content for the description of individual differences. However he tends to use general terms and has a more literary orientation. In describing cases, Adler is able to fill in the gap between specific behaviors and his broad concepts but not with a language which is systematically applied to different individuals. The absence in general of intermediate, more specific constructs limits the power of his theory to describe individual differences systematically. What content is present is largely one which can be used, as in psychoanalysis, to explain different kinds of psychopathology but not the great variety of behavior which is sometimes referred to as "normal." As a process theory, "Individual Psychology" implicitly accepts some notions of learning from experience. Again the major processes are broadly outlined. Experience determines, through learning, the feeling of inadequacy, the struggle for power, the development of a style of life, the setting up of a distance to avoid failure, but the more specific characteristics of the learning process itself are completely absent. Adler was one of the first to emphasize the importance of personality as affecting perception. He felt that both memory and the perception of present circumstances were filtered through the style of life. Once the personality was fixed or in Adler's terms, the style of life was fixed, new learning was also limited in its compatibility to the already formed style of life. In other words, while Adler suggested some molar principles in regard to learning and perception, the details of these processes were not included in his theory. It can be seen that the major emphasis in Individual Psychology was on the internal determination of behavior after the formation of the style of life by age six. After six the individual screened all his experiences through his style of life.

Like Freud, Adler saw behavior as fairly well fixed in early childhood and unlikely to change on the basis of experience once the basic character structure was set. However, change could occur if the individual would recognize the mistaken nature of his childhood adjustments and could be encouraged to develop his social interest. Change takes place by understanding, or more technically, insight. However, in his writings about the treatment of children Adler tended to emphasize environmental treatment—that is, the treatment of parents, teachers, and others who deal with the child daily so that he explicitly accepted the notion that even children whose style of life has been set are more susceptible to change through new experience than are adults whose change is more dependent upon understanding of both their own natures and the nature of society.

ROGERS' SELF-THEORY

Carl Rogers has produced an approach to personality theorizing which is somewhat different from those that have been described, primarily in its concern with psychotherapy. He stresses internal processes of the individual and subjective experience. Because of this emphasis it is generally regarded as a "phenomonological" theory or a self-concept theory, since the concept of self is his major explanatory construct. Rogers (1951, 1959, 1961) feels that it is in the psychotherapeutic situation that one can obtain the most crucial insights into human nature, and he has developed a theory concerned primarily with explaining the changes that take place in therapy. The method of therapy he has evolved is essentially in the Rankian tradition. A brief description of some of Rank's views will help in understanding Rogers' orientation to psychotherapy, which in turn is basic to understanding his personality theory.

Like most of the other apostasies from Freudian psychoanalysis, Rank (1936) objected to the primacy of sexual drive as a basic explanation of motivated behavior in humans. He also, like Adler, tended to reject the importance of the unconscious as a storehouse of energy and the cleaning out of the unconscious as a first step in psychotherapy. For Adler, this meant more direct dealing with the patient at what the psychoanalyst would call the ego level; consequently, more direct interpretation and shorter psychotherapy. Rank elaborated these ideas.

Rank asserted that delving into the past served no useful purpose but fixated the patient in the painful situations of the past, leaving him powerless to deal constructively with his current problems. He rejected, therefore, not only catharsis but insight into the origin of current conflicts as either necessary or particularly useful in accomplishing change in the client. It was also clear to Rank that highly dependent individuals involved in the typical analytic "transference" would be hard to break off in therapy and to get to stand on their own feet. He felt that the relationship between the patient and the therapist from the very beginning should be one which stimulated the patient toward independence.

Without catharsis and insight, however, to explain why people should get better, Rank utilized the concept of willpower, a prevalent concept in Europe at the time. His concept of will was conceived as another human faculty explaining man's efforts toward the obtaining of his goals. He felt that everyone had such a faculty which if directed into constructive channels would allow him to make a better solution of his problems. He spoke, therefore, of the purpose of therapy as that of awakening of the patient's "constructive will" by the therapist acting as a "counter will."

Rogers accepted the general principle that therapy could proceed without an analysis of the past but through the client's ability to solve his problems himself as he saw more deeply into them as a result of the therapist reflecting his feelings. Inherently Rogers' conception still emphasized the dependent nature of the client and the importance of the client's differentiating himself from others.

In his current attempt to systematize his views Rogers (1959) makes three basic assumptions regarding human nature. The first of these is that the basic datum of personality theory is the individual's subjective experience, which is reality for the individual. Since no one can completely understand the internal frame of reference of someone else, the person himself has the greatest potential awareness of what reality is for him.

The second major assumption is that everyone has an inborn tendency toward "self-actualization." Rogers defines actualizing tendency as the inherent tendency of the organism to develop all its capacities in ways which serve to maintain or enhance the organism. It includes not only the satisfaction of biological needs and the development of skills but also development toward autonomy and away from control by external forces. This is Rogers' motivational concept, and he states that the organism's behavior is goal directed toward actualization in the reality as perceived.

The third major assumption is that each person engages in an organismic valuing process. Experience is valued as positive or negative in reference to the actualizing tendency. Experiences which are valued positively are behaved toward with adience and those which are valued negatively are reacted toward with avoidance. For Rogers, this last principle accounts for the adjustive or regulatory ability of the individual. However, he stresses that the environment to which the individual is reacting is a subjective environment which has a reality only in the person's own experience. Rogers appears to be saying here that no predictions about the individual's behavior can be based upon objective descriptions of events or stimulus situations, but rather that one could not predict how the organism would behave until one knew what his subjective experiences in a given situation were.

To these basic assumptions regarding human nature Rogers has added four additional higher-level constructs. The first of these is the concept of the *self*. As part of the tendency toward differentiation which is itself a part of the actualizing tendency, a portion of the individual's experience becomes differentiated and symbolized in an awareness of *being* which in turn is an awareness of self-experience. Through continued interaction with the environment, this awareness eventuates in a concept of self. The self is regarded as an object in the experiential field of the organism. This notion of a self-concept is crucial for Rogers in understanding psychopathology and adjustment. If there is an incongruence or discrepancy between the individual's concept of self and other experiences, then the organism is vulnerable. That is, the person is in a state of anxiety or threat and liable to defense, internal tension, and disorganization.

The second of Rogers' major constructs is *the need for positive regard.* This need is considered to be universal for Rogers and he states that it is irrelevant to the theory whether it is inherent or learned. It is the need for significant others to regard him positively. The satisfaction of someone else's need to regard himself positively will also lead to positive regard for the person. It is also possible (and Rogers implies, unfortunate) that the need for positive regard by significant others can become more compelling than the need for experiences which are of positive value in actualizing the organism.

Rogers' third important variable is a development of a need for *self-regard.* The need for self-regard follows developmentally from the need for positive regard. Rogers simply asserts that positive regard satisfactions or frustrations associated with any particular self-experience come to be experienced by the individual independently of positive regard transactions with social others. Positive regard experienced in this way is self-regard. In other words, the individual, although originally dependent upon positive regard from others, becomes his own determiner of good and bad experiences (presumably after having introjected the positive and negative regard experiences arising in relationship to significant others). The need for positive regard and self-regard together make up a total "self-regard complex."

The last of Rogers' major constructs is the development of conditions of worth. When self-experiences are discriminated either by significant others or by the person himself as being more or less worthy of positive regard or self-regard, the individual is said to have acquired a condition of worth. That is, as soon as significant others react in such a way that the individual discriminates the absence of positive regard or self-regard for some of his experiences, he sets up conditions of worth.

The last three of these major constructs describe a developmental sequence, where the organism first is dependent upon the positive regard of others and experiences both the presence and absence of this positive regard, and he then values some of his behaviors greater than others in that they lead to the positive regard or do not. He "introjects" these values and then regards himself positively or negatively, depending on whether or not his behavior is consistent with or congruent with these values and at the same time develops a set of conditions for self-acceptance or worth. He cannot regard himself positively, as having worth, unless he lives in terms of these conditions. Those behaviors, or reactions, feelings, or experiences which are inconsistent have to be avoided by defense. Defense typically takes the form of a rigidity of perception or distortion of perception in the direction of consistency between the self-concept, including its conditions of worth, and the immediate experience. If the experience were symbolized accurately and as incongruent to the self-structure, this would result in anxiety. If the process of defense is unsuccessful, then anxiety results. If the experiences that are admitted to awareness are sufficiently incongruent and pose a sufficient threat to the "self-structure," then a state of disorganization results. In disorganization the organism vacillates between expressing the experiences which have been denied to awareness and expressing those which are consistent with the concepts of self.

To reverse this process of threat, anxiety, and disorganization by psychotherapy, Rogers has defined what he considers to be the necessary conditions of therapy. The essence of these conditions is that the psychotherapist, himself, in the therapeutic situation be *free from incongruence* or vulnerability, that the therapist be experiencing *unconditional positive regard* for the client, that the therapist be experiencing an *empathic understanding* of the client's *internal frame of reference,* and finally that the client perceive, at least to some degree, the unconditional positive regard and the empathic understanding of the therapist. Presumably, under these conditions, the client can admit all experiences and feelings into awareness (since conditions of worth are eliminated) and reintegrate his self-structure so that incongruent experiences and feelings are reduced and the process of self-actualization is no longer being retarded.

EVALUATION OF ROGERS' THEORY In analyzing Rogers' approach to personality theory, it should be remembered that Rogers' focus is on psychotherapy and that he is not, at least for the present, interested in the development of a set of constructs to be used in predicting or even understanding individual behavior in a variety of other situations except as they may be considered to be indicative of adjustment or maladjustment. His orientation, therefore, is very general but highly focused.

Systematic Nature of the Theory. Rogers' theory is essentially a process theory with content categories only of a very general nature. Rogers has made his assumptions regarding the nature of his major processes quite explicit. However, in defining such terms as experience, awareness, internal frame of reference, actualizing tendency, he tends to use other constructs which are not anchored in observables. For the most part, his basic assumptions simply have to be accepted, not tested. For example, in defining a feeling or experiencing a feeling, Rogers says it denotes (1959, p. 198) "an emotionally tinged experience, together with its personal meaning." But emotion is not otherwise defined in his theory and experience as a verb is defined as (1959, p. 197) "simply to receive in the organism the impact of the sensory or physiological events which are happening at the moment." But how does one know when an impact occurs in the organism and what kind of impact it is? In other words, while Rogers carefully defines his major constructs, he tends to do so by using other abstractions which are either undefined or not clearly related to some kind of observables. The result is what Wendell Johnson (1946) has sometimes referred to as "semantic blockage" or "short-circuited abstracting." While this is not true of some of Rogers' constructs, it tends to be true of his more general ones, such as self-actualizing, experience, feeling, and awareness.

In general Rogers has linked all of his constructs together in a network of propositions specifying the direction of the relationship between the constructs. For example, incongruence between experience and self-concept leads to threat and defense; the absence of positive regard leads to the setting up of conditions of worth; the presence of conditions of worth interferes with self-actualizing tendency. While these relationships are fairly clear, the problem of prediction itself is not so clear. Rogers negates the use of objectively described events for predictions. Whether an event is experienced as positive regard or not must be determined by the subject, and no one else can fully understand the subject's internal frame of reference. Under these rules, it is difficult to see how any predictive sequence can be arrived at. Perhaps it would be fair to say that Rogers is not really interested in prediction, in the usual sense, but in specifying "the necessary conditions for therapy." He is involved, at least here, in a very general way in problems of prediction. Should he go further in an attempt to specify what kinds of vulnerable or incongruent people would benefit more or less from a particular approach to psychotherapy, he would be further involved in prediction.

Perhaps Rogers would answer the previous criticism by saying that although one person cannot wholly know the internal frame of reference or subjective experience of another, he may approximate this through "empathy." Empathy is defined as perceiving the internal frame of reference of another with accuracy. How one accomplishes this or when one knows that they have done so is not clear; however, Rogers has made fairly clear that the diagnostic study of the individual presumably is of no help and in fact may interfere with empathic understanding.

Are some of Rogers' concepts gratuitous? The self-actualizing tendency as defined by Rogers is very broad and would include a great many different events as referents. The tendency is considered as universal and is not used to explain differences among people. Rogers recognizes the difficulty in operationalizing this concept, but since it is not used to predict differences, the question arises as to whether it is a necessary part of his system. However, careful examination of his approach to therapy clearly implies that the product of proper therapy is increased self-actualizing *behavior* in the client. The concept is, then, not gratuitous as long as self-actualizing behavior is relied upon as a criterion of a particular kind of change, even though no particular use is made of differences in the strength of self-actualizing tendency. It is necessary, therefore, for a test of many of Rogers' propositions that self-actualizing behavior be operationalized, even though self-actualizing tendency is not. Perhaps Rogers would say that it is self-actualizing experience that is important rather than behavior. In any case, then there would be self-actualizing experience which would need to be operationalized.

Operational Nature of the Theory. A basic difficulty with Rogers' orientation is that most of his constructs are defined in terms of some internal process (experience, experiencing of feeling, experiencing positive regard, etc.) and are not available to direct measurement. This would not be too great a problem if the methods of indirect measurement were clear and logical. Since Rogers is basically interested in research in psychotherapy, it has been necessary for him to set up operations for at least some of his concepts. For others, operations are lacking. In general, he or his students have attempted operationalizing those constructs which have the most to do with the conduct of psychotherapy and the criteria for psychotherapeutic success.

Examples of some of Rogers' operations follow. Unconditional positive regard is not measured by the individual's subjective experience, but rather by ratings of other therapists as to whether or not a particular therapist is tending directively to control a patient in therapy to a greater or lesser degree. Discrepancy between self-concept and immediate experience is not measured by the individual's experience of this discrepancy or verbal report of it, but rather by the difference between a Q-sort of his "ideal" self-image and his "real" self-image. Whether or not these are logical operations of Rogers' constructs is a matter of opinion. It is clear that such measurements can be made reliably. A study by Chodorkoff (1954) illustrates the operationalizing of some of Rogers' constructs.

Chodorkoff tested some of Rogers' hypotheses regarding the relationship of adjustment, accuracy of self-perception, and defensiveness. He hypothesized that the greater the agreement between an individual's self-description and an objective description of him, the better would be the personal adjustment of that individual and the less perceptual defensiveness he would show. Defensiveness is defined as a perceptual phenomenon which leads to the prevention of threatening events reaching consciousness.

Chodorkoff used the Q-sort technique to obtain his measure of accuracy of self-description. The subject sorted 125 statements according to the degree to which they described him. The same statements were sorted by judges who presumably made objective descriptions based upon their examination of projective tests and a biographical inventory. The projective tests included the Rorschach, the Thematic Apperception test, and a Word Association test. From the relationship of the two sets of descriptions an accuracy of self-description score was obtained for each subject. To measure perceptual defensiveness for each subject ten words from his word association test which were considered to be neutral and ten which were considered to be threatening words for him were later presented by tachistoscope, and the difference between the recognition thresholds for the neutral and threatening words gave a mean perceptual defensiveness score. A second perceptual defensiveness score was obtained from the difference between the sigmas of the neutral and threatening words. Adjustment was measured by ratings of the same judges from essentially the same materials. In addition, one of the judges (the experimenter), who had all the other data available to him, applied the Munroe Check List to the Rorschach protocols and obtained an adjustment check list score. The intercorrelation of the variables is shown in Table 12-3.

For the sake of simplicity all the correlations are presented as positive correlations since they are all in the hypothesized direction. It can be seen that all of Chodorkoff's hypotheses were supported. Accuracy of self-description significantly related to perceptual defensiveness scores and adjustment, and perceptual defensiveness scores also related to adjustment.

As Chodorkoff notes, the major problem with this study lies in the contamination of the judges who made the adjustment ratings from the same test data that they used for their Q-sorts of the subjects. The perceptual defensiveness scores were less contaminated in that the recognition thresholds were not part of the judges' data and were obtained independently. However, even here there is some contamination in that the word association tests were available to the judges, and each subject's response and reaction time to the same words had been included in the basis for Q-sorts and adjustment ratings. Of course, it is difficult to determine to what extent such contamination of the judges affected the results. Taken at face value, however, these results show support for Rogers' hypotheses.

TABLE 12-3

Summary of Correlations*

Hypothesis	Basic Scores Correlated	Type of Correlation†	Correlation
1	ASD with PDm‡	r	.53[b]
	ASD with PDs	r	.44[a]
2	Ac with ASD	r	.34
	Ar with ASD	r_b	.73[b]
	Ar and Ac with ASD	R	.73[b]
3	Ac with PDm	r	.29
	Ac with PDs	r	.37[a]
	Ar with PDm	r_b	.62[b]
	Ar with PDs	r_b	.68[b]
	Ar and Ac with PDm	R	.62[b]
	Ar and Ac with PDs	R	.68[b]

* From Chodorkoff, B. Self Perception, Perceptual Defense and Adjustment. *J. abnorm. soc. Psychol.*, 1954, **49**, 508-512, by permission.

† Product-moment correlation has been indicated by r. Biserial correlation has been indicated by r_b and multiple correlation by R.

‡ ASD —Accuracy of Self Description
PDm—Mean difference of recognition threshold for threatening and nonthreatening words
PDs —Difference in variance of recognition thresholds for threatening and nonthreatening words
Ar —Judges' rating of adjustment
Ac —Munroe check list measure of adjustment from Rorschach protocols

[a] Significant at the .05 level of confidence.

[b] Significant at the .01 level of confidence.

Process versus Content. Rogers' theory is primarily a process theory. His interest in content variables is limited only to a few very broad variables. The main differences among individuals, for Rogers, are the amount of discrepancy between self and ideal concepts, the amount of anxiety and subsequent defense, and the amount of self-actualizing experience. Apparently, it is left for the individual investigator to supply the more specific constructs to describe these three very broad variables. It should be apparent that except for very broad predictions Rogers' theory would not be adequate for most purposes of selection and prediction of individual differences for the wide range of behaviors which might be called "normal." Rogers' theory is more or less explicitly constructed to explicate "the process of psychotherapy." But even here one might still question whether or not there is sufficient description of individual differences or content variables to understand adequately what happens when two individuals interact with the hope that at least one of them would attain a better adjustment as a result. That is, there are very few bases in which to account for differences in outcome of therapy as a result of the differences in the personality of the client. For example, the fact that one patient might be openly and strongly dependent and another rejecting his dependency might have considerable relevance to therapy. However, Rogers apparently would consider such differences unimportant, at least in regard to what would constitute an efficient approach to therapy. Most of his research, then, tends to be oriented toward therapist variables rather than patient variables. It would seem to this writer that even for the most limited problem of understanding and devising therapeutic procedures, much important variance is neglected with this approach.

The theory, itself, is very general in nature. Both the process variables and the content variables are at a high level of abstraction, and the acquisition or development of specific behaviors, traits, or needs is not discussed. Rogers' concern is with the overall growth processes.

While Rogers feels that the actualizing tendency, his major motivational variable, is inherent and that it is irrelevant whether the need for positive regard is inherent or learned, he does not use either variable for predictive purposes. That is, he feels that they are present in all humans and to a sufficient degree so that no important predictions can be obtained from differences in the *strength* of these variables. In this sense, the emphasis is therefore on experience rather than inheritance or constitution. In actuality, though he posits an actualizing tendency as his major motivational variable, it is the need for positive regard and the need for self-regard which are used to explain learning and change.

Perhaps to a greater degree than any other theorist described in this chapter, Rogers' explanatory concepts appear to be internal. His position here seems to be inconsistent. In explaining the success of therapy he puts his emphasis on therapist behavior but in explaining behavior, experience, or feelings, Rogers relies almost exclusively on internal processes. Although he appears to imply that some situations are more likely to create threat or promote growth than others, he generates no predictions from the situation, since it is always the individual's subjective reaction which determines the nature of the situation for him.

THE PERSONOLOGY OF GORDON ALLPORT

Whether or not a personality theorist places emphasis primarily on inherited or constitutional characteristics or on the effects of experience, all would have to admit to the uniqueness of each individual. Most writers attempt to reduce this uniqueness to common characteristics or common laws in order to make comparative assessments of individuals and predictions of individual differences over life situations. Perhaps the best way to characterize Allport's (1939, 1961) approach to personality is to say that he attempts to nurture this individuality and to rescue this uniqueness from its reduction to common characteristics and common laws.

While Allport can be regarded as eclectic in approach, since he attempts to incorporate something of almost all orientations toward personality in his own approach, it is this interest in the uniqueness of each person and his attempt to describe it which is the main theme of his approach and which tends to bind together the many different orientations which he accepts in part.

Allport recognizes that science as usually conceived is nomothetic. That is, it deals with universal laws or generalities not with individual instances. However, as applied to psychology, or more specifically, to personality theory, Allport feels that this is too restricted a definition of science and that science must also include a study of individuality or individual instances. Such study he refers to as ideographic. When the individual as a whole is taken as a phenomenon to be studied, then deviations in his behavior can be compared to norms of the individual himself. The ideographic laws that can be derived are laws regarding the variability in the individual's own behavior without reference to its comparison to others. For Allport the study of personality needs to include both nomothetic and ideographic approaches. The individual may have many characteristics which he holds in common with others, but the organization of these characteristics, according to Allport, is unique. This uniqueness is a primary fact of human nature and cannot be ignored.

The emergence of personality for Allport results from the interaction of the infant's "raw materials" and experience. The raw materials which he abstracts are physique, temperament, and intelligence. These inherited characteristics are of major importance in determining how the individual initially reacts to stimulation. Temperament refers to the characteristic phenomenon of an individual's emotional nature, including his susceptibility to emotional stimulation, his prevailing mood, speed of response, and peculiarities in the fluctuations of these. He regards intelligence and physique in the usual way but points out that all the three variables involve unique patterning in each individual. The interaction of these characteristics with experience produces a growing individual who progresses in both differentiation and integration. Though Allport provides no special theory of learning regarding the growth process, he notes that the principles of learning must be broader than the typical *S-R* laws and must include cognitive laws as well. Learning continues throughout life and personality similarly can be seen as something which changes continuously.

During this process of growth there is an evolving sense of self. Allport distinguishes seven aspects of this sense of self that he feels combine to form an organized whole which he terms the "proprium." There is a sense of bodily self, of self-identity, self-esteem, self-extension, self-image, the self as rational agent, and the self as involved in long-range goals or propriate striving. These aspects of the self emerge at different stages of development, but once established the proprium becomes the principal source of subsequent learning. The proprium is both part of and influences the development of other enduring personality characteristics.

While the proprium exercises a guiding influence on the individual's life, what the person will do under any particular set of circumstances is dependent upon four sets of conditions: (1) enduring personality characteristics; (2) defenses and concealments used by the person; (3) how he perceives the present situation and its relevance to himself; and (4) what the situational assignment actually calls for and expects of him. *Culture* and *role* play a heavy part in determining the last of these four conditions.

Allport recognizes the central place of motivation in personality theory, but he takes no stand himself on how motives arise or are learned. He is, however, considerably interested in how they are transformed during the process of growth. He has formulated the principle of functional autonomy to describe one aspect of this process. "Functional autonomy refers to any acquired system of motivation in which the tensions involved are not of the same kind as the antecedent tensions from which the acquired system developed." (Allport, 1961, p. 229) There are two important considerations here. The first is that a secondary or learned motive which has previously been attached to a particular satisfaction becomes independent of that particular satisfaction (for example, the presence of the mother takes on positive value because of her association with feeding, but later fails to extinguish when she is no longer connected with food). The second important consideration is that the motive is maintained on the basis of having a place in or making up part of the total proprium or self. Motives become autonomous of their original sources but not independent of "propriate structure." Motives become transformed as part of the general force toward unification of life.

This fact of functional autonomy of motives Allport feels may have considerable significance for the practice of psychotherapy. It implies for him that the neurosis may well become autonomous, too, and would better be handled by re-education than by reliving.

For the description of individual differences, Allport relies on the concept of traits. He distinguishes first of all *common traits* which are empirically established when personality tests or other measures can show that people respond consistently over a period of time and with characteristic intensity. Such traits generally show a normal distribution in the population at large. He finds such common traits of use for crude predictions, but he is more interested in a different kind of trait which he refers to as *personal dispositions*. While common traits which have been described in the psychological literature may number in the hundreds, to describe personal dispositions Allport would employ the whole of descriptive language, that is, the thousands of adjectives referring to personal behavior. Personal dispositions refer to the unique patterning of characteristics, and every nuance of the language is necessary in order to capture this uniqueness. The terms which one would apply to one individual are different from those which might be applied to another. They are not common traits but for Allport unique patterns. One may have different dispositions which are characteristically used in different situations but are still consistent in that they are aspects of a higher order disposition. Allport discriminates three levels of personal dispositions. *Cardinal* is the most general, and otherwise may be referred to as a ruling passion, a master sentiment, a unity thema, etc. An individual might have only one cardinal trait, but he would be more likely to have a few. At an intermediate level of generality are *central* dispositions, and at the most specific level are *secondary* dispositions which may appear the least consistent, or most situational or peripheral. These are not sharply demarcated levels. It is Allport's way of indicating that traits have a hierarchical organization. Personal dispositions have motivational qualities, apparently the higher in the hierarchy the stronger the motivational characteristic. It has been noted earlier that personal dispositions or enduring personality characteristics are only one of the determinants of behavior. Consequently, personal dispositions cannot be observed directly but must be inferred from the repeated occurrence of acts having the same significance following a definable range of stimuli having the same personal significance.

It is difficult to summarize Allport's approach to personality, but it is safe to say that he regards it as a *unified system,* which is a product of biological endowment, cultural shaping, cognitive style, learning, and spiritual groping. It is a system which is continuously growing and changing or for Allport *"becoming."*

EVALUATION OF ALLPORT'S THEORY *Systematic Nature of the Theory.* Allport is both far more sophisticated and more thorough than most writers in expressing his underlying philosophy of science assumptions. For his psychological theory per se Allport is again explicit in the assumptions regarding the organization of enduring personal characteristics. He makes clear his assumptions regarding unity, uniqueness, patterning, and an active self. However, his basic principles regarding the acquisition of personality characteristics, that is, those referring to learning and change, are not made explicit partly because Allport only comments on selective aspects of these problems.

While assumptions in regard to the organization of personality are explicit, the nature of the relationships among his major constructs is cloudy. Just how does the proprium relate to the development of cardinal dispositions? What is the effect of one kind of bodily image or bodily self on another aspect of the proprium? If the self or proprium determines the integration of some motives independently of their origins into the unified personality but not others, how does this process work? What characteristics of a proprium will lead to the incorporation of one particular motive and not another? Since so many of these crucial relationships are not specified, it is difficult to test Allport's theory. It would be difficult to know whether a particular empirical finding lent support to the theory or challenged it.

Since Allport advocates the use of almost all of the nuances in the English language to describe personal dispositions, there can be little doubt that Allport has more terms than most theories to account for individual differences. On the other hand, the efficiency of such an orientation is in grave doubt. There must be many instances in which different terms are used to account for things which are functionally equivalent. Even among Allport's higher-level constructs such as the proprium, common traits and cardinal traits there appears to be considerable overlapping. It should be remembered, however, that Allport is really not interested in efficiency but in his concern with capturing the uniqueness of each individual.

Operational Nature of the Theory. Since Allport expresses doubt as to the value of accounting for human behavior by universal laws, he has not been too concerned either with the statement of antecedents and consequences, operations for his higher-order constructs, or the testing of hypotheses. The issue of uniqueness is worth looking into. That each individual is unique is indisputable, but the question of basic significance is whether the laws that govern the individual's behavior or his internal organization are unique to the individual. There seems to be little doubt that such laws are extremely complex, involving many variables, but the assumption of separate laws for separate people goes beyond the bounds of science as it is typically regarded. If you were to examine a thousand pebbles found in nature, it would be easy enough to establish that each pebble was unique, a fact which might be of interest to a collector. But the scientist doesn't deal with pebbles; he deals with abstractions developed for particular purposes of prediction, such as weight, shape, color, chemical composition, etc. Allport is right in saying that the pebble is more than any particular collection of such abstractions, but it is nevertheless the problem of the scientist to make the abstractions and to discover the orderliness of nature.

In regard to the measurement of common traits Allport accepts the more or less conventional techniques of personality measurement. However, for the understanding of idiosyncratic personal dispositions he favors the case study, life history, analysis of literary works, etc. He is also interested in the study of expressive movements and expressive behavior. By the use of holistic matching techniques, where one set of observations of the person can be matched with others without analysis into specific characteristics, Allport feels that the individual can be studied without doing violence to his individuality. An example of such a study was that of Cantril and Allport (1935). However, even in this study nomothetic rather than unique characteristics of the individual were used as the criteria for matching.

Allport and Cantril in eight separate experiments studied the ability to match voices (as an expressive personality trait) with other characteristics of the individual. Five of the eleven characteristics were physical or expressive features. They were (1) age, (2) height, (3) appearance in photographs, (4) appearance in person, and (5) handwriting. The other six characteristics were "inner" aspects of personality. These included (6) vocation, (7) political preference, (8) extroversion-introversion, (9) ascendance-submission, (10) dominant value, and (11) a summary sketch. Introversion-extroversion was measured by the Heidbreder Scale, ascendance-submission by the Allport A-S Reaction Study, and for dominant values the Allport-Vernon Study of Values was used.

Student judges listened to three voices and then matched them against the description of the three individuals for the particular trait. For example, for age they were given the three ages. For handwriting, they were shown handwriting samples. For the personality characteristics, brief descriptions were given. Essentially Allport and Cantril were testing first of all the generalized or unified nature of personality and more specifically the consistency with which judges could predict one specific expressive characteristic from another. The results are shown in Table 12-4. Each Roman numeral indicates a separate experiment. The figures in the table are critical ratios based on the probable error of the difference between chance matching (one third correct) and the actual percentage of correct matching. A critical ratio of 4.0 or greater would be significant. The significant matchings are presented in italics. Negative numbers above 4.0 show where the matching was significantly wrong, positive numbers where the matching was significantly better than chance.

It can be seen from Table 12-4 that twenty-one different speakers were used in the separate experiments. Experiments I and II used the same speakers. The findings for these two studies are strikingly similar, indicating that a great deal of the variance is determined by the speakers themselves and that characteristics which could be significantly judged for these individuals from voice could not be so judged with others. The overall findings indicate that in general, voice characteristics can be judged correctly as relating to certain personality characteristics much better than they can physical characteristics. Handwriting was never matched significantly better than chance. Most of the matchings for speakers A, B, C and E, H, and J were significantly correct, for speakers D, E, F they were significantly wrong or approaching negative significance, and for speakers N, O, P and Q, R, S all the matchings were chance.

While a number of the findings were not significant, there is clearly a preponderance of positive significant matchings over negative significant matchings and more positive significant matchings than could be expected from chance from the total number of correlations computed. These eight separate laboratory studies, however, also indicate that the matching technique is highly dependent upon the nature of subjects being matched, and the skill of the judges, as well as being a test of any general hypothesis under investigation.

Content versus Process. It is readily apparent that Allport has both a process and content theory, although the content theory is more explicitly described and more extensive. His major interest is in the relatively enduring personal characteristics of the individual and in methods of describing these characteristics. The process theory is eclectic, including conventional theories of learning, cognitive learning, and motivation plus his own additions to these such as the role of the proprium in learning and functional autonomy. Allport seems to lie squarely in the middle of a continuum attributing variance to either biological or genetic determinants versus experience. However, he points out that at the present stage of scientific progress it is more helpful on the whole to study personality from a psychological than from a biological point of view.

TABLE 12-4

Results of Matching Various Features of Personality with Voice*

Feature Judged	Experiment Number							
	I	II	III	IV	V	VI	VII	VIII
Physical and expressive features								
1. age					*9.9*		0.0	3.2
2. height			−3.3		*4.3*	−0.6		
3. appearance in photographs	2.8	1.2	3.6	*8.8*	*6.9*			
4. appearance in person			−2.8	1.0	*6.0*	2.8		
5. handwriting	1.9		0.9	2.1	0.7	−2.2		
Interests and traits								
6. vocation	−2.8		*−5.9*	*6.4*	−1.3	0.4	−1.3	*−4.8*
7. political preferences	*7.4*		1.2	3.3				
8. extroversion-introversion	*14.0*	*15.0*	*−5.4*	−3.1	−3.8	−0.8	3.2	*14.4*
9. ascendance-submission	*13.5*	*14.5*	*17.0*	*8.8*	*−4.8*		2.7	*10.7*
10. dominant values	*7.4*	*6.0*	−3.3	0.2	*8.0*	0.3	3.2	*−6.7*
11. summary sketch	*14.7*	*13.5*	−2.0	*5.7*	*8.8*	0.9	−0.6	3.1
Number of judges	55	85	34	46	50	32	33	33
Voice number 1	A	A	D	G	K	N	Q	T
Voice number 2	B	B	E	H	L	O	R	U
Voice number 3	C	C	F	J	M	P	S	V

---

* Cantril, H., and Allport, G. W., *The Psychology of Radio,* New York: Harper & Brothers, 1935, p. 115, by permission.

More so than other writers, Allport recognizes the role of the psychological situation in the determination of behavior. He explicitly indicates the influence of role expectations in different situations as being independent of and interacting with personal dispositions. He also includes the situation, to some extent, in his concept of secondary or lower-order dispositions so that a characteristic of the individual is to behave in a particular way in a particular situation or class of situations. No systematic way of defining situations or determining situational similarity, however, is described.

In dealing with the problem of motivation Allport is clear in denying that drive reduction is the main principle. Rather, he favors functional autonomy as a result of the growing integrated self. Personal dispositions have motivational characteristics, some more than others. The whole process of motivation as conceived by Allport is not clear to this writer. Allport's handling of change is likewise somewhat ambiguous. Change is constant. The process is affected to a large extent by the proprium as well as by experience. But the details of the processes are not always made explicit, and it is difficult to derive methods for instituting specific kinds of changes from Allport's theorizing.

Although Allport's orientation may be said to be weak in the general areas of systematization, predictiveness, and operationality, he has himself rejected these goals as the only or even the major goals of a personality theory. Rather, he has tried to bring back into psychology a strong emphasis on a unified integrated organism whose patterned characteristics make him a unique individual.

SOCIAL LEARNING THEORY

Social learning theory can be briefly characterized as a molar learning theory specifically developed to explain the acquisition of relatively stable personal characteristics. It is an expectancy theory which utilizes an empirical law of effect. The theory was developed with the focus on human behavior in complex social situations rather than on animal behavior in highly controlled impersonal situations. The general theory was developed by the writer (1954) along with his students and colleagues. The general assumptions of social learning theory are explicitly stated.

The first principle of social learning theory is "the unit of investigation for the study of personality is the interaction of the individual and his meaningful environment." This general principle, although appearing to be similar to other approaches actually places the emphasis in personality study not on internal characteristics but on the interaction of the individual with his social environment. Personality is not an internal characteristic of the organism which the person carries around with him, but rather it is the potential to respond to a particular kind of social environment. The emphasis is that of J. R. Kantor (1924) and of Kurt Lewin (1935), both of whom considerably influenced the general formulations of social learning theory. It follows from this principle that the study of personality is the study of learned behavior, that is, of behavior that is modifiable and that changes with experiences. Since the emphasis is on the effect of experience on behavior, it also follows that an understanding of a behavior requires an understanding of previous sequences of effects. The method of personality study is therefore historical.

The second general principle of social learning theory states that personality constructs are not dependent for explanation upon constructs in any other field and rejects reductionism. Scientific constructs for one mode of description should be consistent with constructs in any other field of science, but no hierarchy of dependency exists among them.

The third principle takes a monistic position and rejects dualism. Behavior as described by personality constructs takes place in space and time. Those events which may be described or abstracted by the use of psychological constructs may also be abstracted by constructs in other fields such as physics, chemistry, and neurology. The abstractions regarding the events represent different levels of description but not different events. Such descriptions from other scientific modes of abstraction may be useful for various practical purposes but are not systematically included in social learning theory. This principle expresses a limitation on the convenience or range of explanation of the theory, and at the same time it rejects the outmoded notion that one level of description, i.e., physiological, can "cause" another level of description, i.e., psychological or vice versa. Social learning theory accepts the point of view that explanation consists of description of the present and antecedent conditions necessary for the occurrence of a behavior at any particular cross section of time and rejects the notion that specific events have specific causes or "etiology." Such descriptions of present or antecedent conditions are never ultimate or final. The approach is that of the logical positivist.

A fourth general principle of social learning theory asserts the unity of personality. A person's experiences or his interaction with his meaningful environment influence each other. New experiences are partial functions of acquired meanings, and old acquired meanings or learnings are changed by new experiences. Perfect prediction of acquired behavior would ideally require a complete knowledge of previous experience. This principle, which is held in common by many personality theorists, goes beyond the mere statement of a gestalt or holistic position. It further implies that continuous change is one of the characteristics of personality. Stability is also a major characteristic since previous experience affects new learning.

The fifth principle deals with the nature of motivation. Like Thorndike (1935) and Skinner (1938), social learning theory utilizes an empirical law of effect. Behavior as described by personality constructs has a directional aspect. It may be said to be goal directed. The directional aspect of behavior is inferred from the effect of reinforcing conditions. This point of view, like that of Allport (1961) and of White (1959), does not consider drive reduction to be a sufficient explanation for motivated behavior. The general point of view is supported at the psychophysiological level by the recent work of Olds and Milner (1954) and Miller (1958) which suggests that positive and negative reinforcements are different reactions of the organism resulting from built-in neurological and physiological characteristics. Such reactions form the basis from which later acquired reinforcements derive their value as a result of experience. It is in the area of social, acquired motivation that drive reduction has its great difficulty in explaining behavior.

The final general principle of social learning theory has to do with the concept of expectancy. The potential occurrence of a behavior is considered to be not only a function of the nature and importance of the goal or reinforcements that will result from these behaviors but also of the person's anticipation or expectancy that these goals will occur. Such expectations are determined by previous experience and can be quantified. Expectancy in this point of view is defined as a probability held by the individual that a particular reinforcement will occur as a function of a specific behavior on his part in a specific situation. Expectancies may or may not be verbalized by the individual and are subjective in nature. The concept as used in social learning theory is somewhat different from Tolman's use (1949) which has an all or none quality. It is also different from Brunswik's use of expectancy (1951) which is based on objective or actuarial probabilities but is most similar to Lewin's use of the concept (1951). It differs, however, from Lewin in that it is assumed that sufficient knowledge of previous experience would allow for accurate prediction of expectancies if all the laws involving generalization, patterning, and incremental functions were known.

In social learning theory, a reinforcement is considered to have a value which determines the degree to which it affects behavior in a choice situation. The value of a reinforcement is defined as a degree of preference for that event to occur relative to other possible events in any given situation, class of situations, or time segment of the individual's life.

In one way social learning theory is a far more complex personality theory than most and requires the analysis of four variables in order to make a prediction, where many theories require only one or two. That is, in some theories explanation and prediction are based on identifying a strongest trait or internal characterstic of the individual or the conflict between two traits.

In addition to these general principles, social learning theory hypothesizes specific relationships among its four principal variables. These are the variables of behavior potential, expectancy, reinforcement value, and the psychological situation. The basic formula for the prediction of goal-directed behavior is as follows:

$$\text{B.P.}_{\cdot x, s_1, R_a} = f(E_{x, R_a, s_1} \,\&\, \text{R.V.}_{\cdot a, s_1}) \qquad (1)$$

The formula may be read as follows: The potential for behavior *x* to occur in situation 1 in relation to reinforcement *a* is a function of the expectancy of the occurrence of reinforcement *a* following behavior *x* in situation 1 and the value of reinforcement *a*, in situation 1. It is assumed that expectancies can be measured along a continuum. Such a formula, however, is extremely limited in application, for it deals only with the potential for a given behavior to occur in relationship to a single specific reinforcement. Personality description requires a more generalized concept of behavior and the formula for these broader concepts is given below:

$$\text{B.P.}_{(x-n),s(1-n),R(a-n)} = f(E_{(x-n),s(1-n),R(a-n)} \,\&\, \text{R.V.}_{(a-n),s(1-n)}) \quad (2)$$

This may be read: The potentiality of the functionally related behaviors x to n to occur in the specified situations 1 to n in relation to potential reinforcements a to n is a function of the expectancies of these behaviors leading to these reinforcements in these situations and the values of these reinforcements in these situations. For purposes of simplicity of communication, the three basic terms in this formula have been typically referred to as need potential, freedom of movement, and need value as in the third formula below:

$$\text{N.P.} = f(\text{F.M.} \,\&\, \text{N.V.}) \quad (3)$$

In this formula the fourth concept, that of the psychological situation is implicit. The content of this theory lies then in empirically determined needs, that is, in grouping behaviors which have some functional relationship on the basis of their leading to the same or similar reinforcements. The generality or breadth of such concepts depend on one's purpose. For example, at a very general level we may use terms such as need for recognition and status, need for love and affection, need for dependence, need for independence, need for dominance, and need for physical comfort. At a more specific level typical concepts might be need for academic recognition, need for aggression toward authority figures, need for love and affection from same sex peers, etc. The bases for such groupings do not derive from presumed instincts or drives but are empirically determined and follow from the learning experience of the individuals of a given culture.

A crucial part of this theory is that there are specific hypotheses regarding the behavior of an individual with low freedom of movement and high need value for a particular class of satisfactions. When an individual has low freedom of movement and places high value on some class of reinforcements, he is likely to learn behaviors to avoid the failure or punishments that he anticipates in this area and may make attempts to achieve these goals on an irreal level. That is, the person anticipating punishment or failure may avoid situations physically or by repression, or he may attempt to reach the goals through rationalization, fantasy, or symbolic means. The great variety of behaviors commonly regarded as defenses or psychopathological symptoms are here referred to as avoidance or irreal behaviors. Such avoidance and irreal behaviors themselves may frequently start a vicious cycle and lead to both immediate and delayed additional negative reinforcements. Expectancies for punishment may give rise to a number of implicit behaviors, thoughts, or cognitions that can be observed only indirectly. Such implicit behaviors might include awareness of disturbed body states, fixation on the punishment, narrowing the field of attention, rehearsal of obsessive thoughts, etc., which can seriously interfere with constructive behavior or problem solution. In other words, frequently at the bottom of a problem involving either lack of feeling of satisfaction, conflict, anticipation of punishment, irreal behavior, or lack of constructive activity, is a condition of low freedom of movement and high need value.

Low freedom of movement may result from the person's lack of knowledge or ability to acquire adequate behaviors to reach his goals or may be a consequence of the fact that the nature of the goal itself (such as the desire to have others take all responsibility for one's actions) frequently results in strong punishments in a specific society. Low freedom of movement may also result from "mistaken" evaluations of the present as a consequence of early experience. That is, for a given person sometimes the behaviors, sometimes "erroneous" expectations, and sometimes the nature of the person's goals may be considered to be the primary source of difficulty.

An important aspect of the problem of low freedom of movement has to do with the concept of minimal goal level in social learning theory. In any given situation the possible outcomes of behavior can be ordered from a very high positive reinforcement or goal to a very high negative reinforcement or goal. The theoretical point at which, in this ordering, the outcome changes from one which is positive or reinforcing in nature to negative or punishing is called the minimal goal level. Such a concept can be applied either to a series of goals that are functionally related, e.g., all achievement goals, or to any combination of outcomes possible in a given situation or set of situations. An individual may have low freedom of movement although from the viewpoint of others he appears to succeed often because his reinforcements usually are below his own minimal goal level. Such internalized high minimal goals are frequently involved in problems of low freedom of movement. This idea is similar to Adler's notion of fictionalized goals. It should be stressed that the goals referred to can be of any kind: moral, ethical, achievement, sexual, affectional, dominating, dependent, etc. In social learning theory any functionally related set of reinforcements toward which the individual moves is considered the basis for assuming a need and for which a need potential, freedom of movement, and need value can be determined.

Behavior will change as new experiences (reinforcement) lead to changes in expectancy. Behavior in a situation can change because an alternative behavior has been strengthened by positive reinforcement or because the previously used behavior has been weakened by negative reinforcement. Insight is generally a way to weaken a behavior by negative (verbal) reinforcement. Social learning theory, however, also emphasizes the strengthening of alternatives as an approach to psychotherapy which is frequently neglected.

In order to increase a person's freedom of movement for goals he values highly, one possible approach is to change the values of the goals themselves. This might be necessary under conditions in which the person has two or more goals of high value but of such nature that the satisfaction of one involves the frustration of the other, as in the case of individuals with strong desires for masculinity and dependency satisfactions in the same situations. Another instance would be one in which the goals of the patient, such as the desire to control and dominate others, lead to conflict with others' needs and eventuate in both immediate and delayed punishment. A third instance of changing the value of goals would involve the lowering of minimal goals when they are unrealistically high, such as in the case of an individual who regards any indication of fear in himself as proof that he is not sufficiently masculine.

To understand how minimal goal levels can be changed, one has to consider how reinforcement values, or the values attached to reinforcement, are acquired, maintained, or changed. In social learning theory, the value of a reinforcement in a given situation is hypothesized to be a function of the expectancy that the reinforcement will lead to subsequent reinforcements, and the value of those subsequent reinforcements as in the formula below:

$$\mathrm{R.V.}_{a,s_1} = f(E_{R_a \rightarrow R_{(b-n)},s_1} \ \& \ \mathrm{R.V.}_{(b-n),s_1}) \qquad (4)$$

If a child believes that when he gets an A in school it will lead to affection, then the value of the A is dependent upon the value of the affection and the expectancy that the affection will be forthcoming. If he feels that a B will lead to rejection, a similar analysis holds. For most goals each reinforcement is related to several consequent reinforcements rather than one.

The problem in changing minimal goals, then, or in changing the value of any goal or set of goals, is frequently one of changing expectancies for subsequent reinforcement. Many times the values of goals are maintained over a long period of time with the expectancy for subsequent reward relatively stable, because the relationships have not been verbalized and the subject is not aware of them. In many instances delayed negative reinforcement follows from achieving an earlier reward, but the subject fails to relate these to the prior goal. For example, a woman seeking to control her husband fails to recognize that the consequences of her behavior and her successful attempts at control, although bringing immediate gratification, also lead to subsequent negative reinforcements because of the hostility or rejection on the part of the husband. In other words, the value of goals can be changed sometimes by examining the early rewards with which they were associated but which may no longer be operating, and also by an analysis of present and future consequences which the person has never associated directly with the goal.

While most theories of behavior recognize that behavior is affected by the psychological situation, social learning theory attempts to make more systematic use of the psychological situation. Most personality theories are concerned only with stable traits and regard inconsistency or lack of stability as a kind of error caused by the specific situation. In this theory consistency in behavior is regarded as consistency which takes place in a given situation or class of situations. *Personality is a characteristic way of reacting in an identifiable situation or class of situations.* Helson (1948) has formulated a similar view in applying his adaptation level theory to some problems in social psychology.

In social learning theory, it has been hypothesized that the situation operates primarily by providing cues for the person, which are related to the magnitude of his expectancies for reinforcement for different behaviors. The effect on the value of the reinforcement itself operates through expectancies for associated or subsequent reinforcements which may differ from situation to situation. It has also been hypothesized that situations may be usefully categorized in terms of the predominant reinforcements as culturally determined for any large or small culture group. There are many other possible ways of categorizing situations depending upon the predictive purposes involved.[2] Methods of determining generality or determining the dimensions of similarity among situations have been described in an earlier paper (Rotter, 1955).

Another area of content in social learning theory has to do with problem-solving skills. Generalized expectancies for particular behaviors leading to particular reinforcements define a need. In addition, cutting across different need areas, are generalized expectancies for a set of behaviors resulting in maximum satisfaction or problem solution. Such expectancies or attitudes have been described as higher-level learning skills by Harlow (1949) and sometimes as learning sets. In social learning theory they are of particular importance in psychotherapy and for other problems involving learning of efficient ways of dealing with life problems. Some expectancies of this type which have already been dealt with experimentally are those of "looking for alternatives" (Schroder and Rotter, 1952), and "expectancies for delayed versus immediate gratification" (Mahrer, 1956; Mischel, 1961). A generalized expectancy that one's own behavior (internal control) determines the nature of one's reinforcements versus an expectancy that reinforcement is determined by forces outside of one's own control (external control) has been studied recently. Belief in external control results from a belief in luck, fate, powerful others, or that the world is too complex to understand and predict. Considerable research on this latter orientation has indicated that a generalized attitude of this kind can be measured and relates to behavior involving social action taking, conformity, resistance to subtle influence, and learning in a wide variety of situations (Rotter, 1963).

EVALUATION OF SOCIAL LEARNING THEORY *Systematic Nature of the Theory.* Social learning theory represents an explicit attempt to systematize the study of personality. Consequently it is *relatively* well systematized. Basic postulates are explicitly stated, all of the constructs are systematically linked, and most relationships and constructs are anchored both by antecedent and consequent conditions. Some might criticize the deliberate attempt not to include constructs which cannot be operationalized or linked to the existing constructs of the theory as a limitation favoring systematization over description of "the true nature of man."

One might well ask, for example, how can one have a personality theory without a systematic concept of emotions or anxiety? Social learning theory does have constructs such as low freedom of movement or expectancy for punishment and high freedom of movement or expectancy for gratification—also concepts of positive and negative reinforcement. Many of the psychological characteristics of "emotions" as described by others are conceptualized as behaviors having a potential of occurrence in various situations. It is a question still to be decided whether these constructs can be used to make the predictions adequately or perhaps make predictions better than the older and more familiar constructs of emotions and anxiety as internal states.

---

[2] Several writers have pointed out the difficulty of identifying situations independently of behavior. That is, how can one describe a situation as one would a physical stimulus independently of the S's response? The problem is not different from that of describing stimuli along dimensions of color although perhaps vastly complicated in social or other complex situations. In the case of color stimuli ultimately the criterion is a response of the scientist or observer, sometimes a response to an intermediate instrument and one that is at the level of sensory discrimination and so leads to high observer agreement. In the case of the social situation, the level of discrimination is common sense based on an understanding of the culture rather than the reading of an instrument. As such, reliability may be limited but still be sufficiently high to considerably increase prediction. In this way specific situations could be identified as school situations, employment situations, girl friend situations, etc. For the purpose of generality various kinds of psychological constructs could be devised to arrive at classes of situations which have similar meaning to the person. The utility of such classes would have to be empirically determined depending on the S's response. The objective referents for these situations, which provide the basis for prediction, however, can be independent of the specific S. That is, they can be reliably identified by cultural, common sense terms.

The treatment of a notion of an entitized *unconscious* is similar. The ability of the person to verbalize a relationship between behavior and reinforcement is treated as an important variable in understanding the acquisition and extinction of expectancies. Repression and other behaviors considered to be functions of an unconscious in psychoanalysis are treated as behaviors learned under particular circumstances and having a different potential for different individuals. While such conceptions lack the appeal and dramatic quality of an entitized unconscious, they may well serve to make valid predictions and allow for logical, efficient operations for measurement.

The use of an empirical law of effect provides the motivational basis for this theory. Reinforcements are inferred from the effect of various events upon behavior. As Meehl (1950) has pointed out, this is not necessarily circular, since once the reinforcements for a particular individual or subculture can be identified, they may be used predictively. Prediction is based on the nature (socially defined) of the situation the individual is in as well as on his past experience which is conceptualized in terms of internal characteristics. While both internal and situational characteristics are important in understanding behavior, the theory places more emphasis on the situation than most other personality theories with the possible exception of those utilized by sociologists and role theorists. Psychological behavior is learned behavior, and the theory makes no assumptions regarding the genetic or constitutional determination of behavior. While behavior can be expected to be relatively stable, since past experience plays a heavy role in determining present behavior and acts as a screen on perception and new learning, there is no concept of fixation or disease. Consequently it would be expected that change in personality following significant new experiences or accumulations of new experiences would be natural, and change may be anticipated with or without psychotherapy.

Operational Nature of the Theory. Social learning theory provides three kinds of definitions for each construct. The first, an ideal definition, is in the general common sense language of the culture. The second definition is a working or systematic definition, or several such. The latter are the statements of the relationship of the construct to other constructs in the system. The third is one or more operational definitions for measurement of the construct. The working or systematic definitions provide the antecedents and consequents. For example, the construct of expectancy is defined ideally as a subjectively held probability that a particular behavior will lead to a particular consequence. Systematically there are several definitions for both expectancy and for increments and decrements in expectancy. The general formula for an expectancy is that an expectancy is a function of the past history of reinforcement in the specific situation (E') plus or minus expectancies generalized from related experiences (GE) divided by the number of past experiences in the specific situation. This relationship is shown in the formula below. N in this formula would be a decelerating function of the number of trials.

$$E_{s_1} = E'_{s_1} + \left(\frac{GE - E'_{s_1}}{N + 1}\right) \qquad (5)$$

The formula is constructed so that in a novel situation expectancy is equal to generalized expectancy (GE), and the expectancy derived in a specific situation (E') becomes increasingly more important in the determination of the over-all expectancy with increased experience in the specific situation. The antecedents for an expectancy are previous history of reinforcement and the generalization of related expectancies. The consequence of an expectancy is its effect upon behavior potentials.

There are many operational definitions of expectancy (Rotter, Fitzgerald, and Joyce, 1954). Some typical ones are: verbal statements on a quantitative scale, the amount of money a subject is willing to bet on a possible outcome, and analysis of certain characteristics of TAT type stories as measures of freedom of movement (Crandall, 1951).

The operational quality of social learning theory makes it possible to study a wide variety of problems experimentally. Whether or not the conscious attempt to limit constructs to those which can be operationalized readily is an asset or limitation will have to be ultimately assessed by the fruitfulness of the research that the theory has instigated.

Social learning theory assumes the building up of need potentials as a result of the generalization of expectancies for reinforcement based upon the

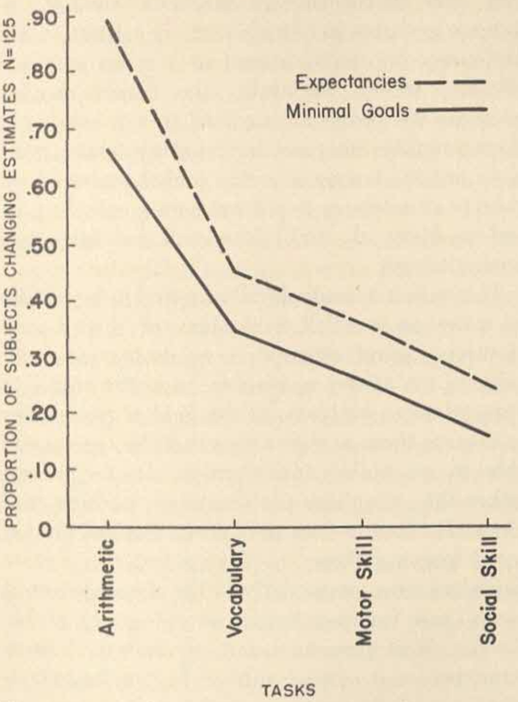


Fig. 12-1. Gradients of generalization of expectancy and minimal goal statements for four tasks. (From Jessor, R. A. Methodological Investigation of the Strength and Generalization of Verbal Reinforcements. Unpublished doctoral dissertation, The Ohio State University, 1951, by permission.)

similarity of the reinforcements. Stated differently, the hypothetical basis for a need is not an instinctual drive but an acquired functional grouping of behaviors as a result of mediated stimulus generalization on the basis of similarity of reinforcements. The degree of similarity of reinforcements in turn can be determined for very early learning on physiological grounds and in later learning on the basis of social or cultural consensus. Several tests of the hypothesis of a gradient of generalization based upon similarity of reinforcement, socially defined, have been made. The study by Jessor (1954) illustrates some of the typical operations employed in this type of research.

Jessor utilized a direct method of measuring expectancy, following the Level of Aspiration paradigm. He employed four tasks: arithmetic, vocabulary, motor skill, and social skill. The arithmetic task was the experimental task, the vocabulary task was an anagram test of increasing difficulty, the motor skill task involved an epicyclic pursuit rotor, and the social skill task was described to the subject as a five-minute interview with one of the girls on the staff to determine general attractiveness to the opposite sex on the basis of a variety of personality characteristics. The subject was asked to estimate beforehand his expected scores on all four tasks on a 0-to-50 continuum and was told that an average score was 25. Jessor also asked the subjects to state a *minimal goal.* This was defined as the lowest score that they might receive and still feel satisfied with their performance. Following the initial statement of expectancy and minimal goal scores, the subject was given the arithmetic task; upon completion of this, he received a prearranged score that was either six points above, or six or twelve points below his minimal goal score for the arithmetic task. Jessor had 125 subjects divided into weak and strong failure groups and one smaller success group.

The subject was then asked to estimate his scores for another form of the same arithmetic task and, since he had not taken the other three tests yet, allowed to change his estimates on those if he wished. Since many of the subjects did not change scores, there was a badly skewed distribution of change scores. Consequently, the results were analyzed in terms of the proportion of subjects who changed their expectancy scores in the direction of the reported score on the four tasks. These results are given in Fig. 12-1.

It may be noted that statistically significant generalization effects took place for both expectancy and minimal goal statements. These changes followed a gradient as predicted by Jessor in terms of the similarity of need or reinforcement based upon "common sense" or cultural knowledge. That is, the arithmetic and vocabulary tests were considered by the investigator to be closest together as two academic recognition tasks; the motor skill task next most similar, since it was a recognition task but of another need area; and the social skill task was most removed as a measure of recognition and also highly related to acceptance and love from opposite sex peers. A secondary hypothesis that minimal goal statements would be more stable, that is, show fewer changes than expectancy statements was also significantly supported for all four tasks.

Process versus Content. Social learning theory is primarily a process theory, but it does have content categories which are systematically related to the theory. A set of behaviors which are functionally related comprise a need potential. The general criteria for functional relationship are generalization of expectancies and substitutability. Reinforcements can similarly be grouped into functional classes based on the same criteria. Psychological situations may be grouped into classes based on predominant reinforcements, the similarity of the behaviors which tend to be elicited in these situations, and in other ways. The problem lies in the fact that a great deal of empirical work has to be done to establish the utility of these functional categories. It would be far easier if one were willing to accept inborn, natural, or arbitrary categories. However, no matter how categories are arrived at in a particular theory, the categories must stand the test of predictiveness. In the clinical application of social learning theory the clinician must use categories which come from his common sense and previous experience until such time that sufficient empirical data have been collected in a given subculture to provide him with the most efficient, least overlapping hierarchy of categories. Such terms would range from highly specific to highly general.

Previous writings from the field of personality, of course, can provide many of these categories that appear to have been useful in the past, but the classification of social situations is more of a problem. Some rudimentary classifications of psychological situations exist in sociology and in role theory. But in general the least progress has been made in the development of useful categories for describing social situations. Threatening versus nonthreatening situations, male versus female social objects, high-reward versus low-reward situations, etc., have been used in psychology as broad categories, but full prediction of complex social behavior will require much more extensive study of the nature of psychological situations in any given subculture.

While the problems involved in developing a satisfactory set of categories to describe psychological situations are still for the most part unsolved, the general emphasis on the psychological situation in social learning theory holds some promise for increasing the predictiveness of personality measures. The work of Crowne and Marlowe (1963) on the social approval motive and its effect on psychological testing as a function of a specific testing situation is a good illustration of this. The theory focuses attention on test responses as behavior which itself is subject to expectancies for reinforcement in a given situation (Rotter, 1960). Hopefully, the theory clarifies many of the problems involved in the validation of personality measures by pointing to the need to regard the testing situation and the criterion or validity situation as important aspects of the general problem of establishing test validities and predictiveness.

The author is undoubtedly limited in his ability to make an over-all evaluation of this theory. However, some attempt is necessary. A major value of the theory appears to lie in the ability to operationalize problems in the field of personality or to state them in such terms that they are amenable to controlled investigation. It complicates rather than simplifies problems but, perhaps, will ultimately lead to finer prediction. For the present social learning theory requires a great deal more empirical data, particularly in the development of useful and functionally sound categories of behaviors, reinforcements, and psychological situations, before it can be applied in practical situations in a wholly systematic fashion.

THE PRESENT STATUS OF PERSONALITY THEORY: CONCLUSIONS

At this time the greatest value of personality theories is heuristic. That is, they serve to focus interest in specific areas of research and to lead to the systematic accumulation of data. In themselves, current theories are not highly predictive. Only under highly controlled laboratory conditions and only for particular kinds of experimental paradigms which have been thoroughly studied is it likely that consistent predictions can be made. Frequently, even in these instances, slight variations in experimental procedures will produce unpredicted results.

This lack of validity is not surprising in that almost all theories lack one or more of the necessary ingredients for a predictive system that can be generally applied to complex social human behavior. These ingredients are: (1) a system of fully operationalized, carefully related constructs; (2) a sound process theory which includes both laws for acquisition and change of relatively stable characteristics; (3) a set of content categories which describe the generality of behavior with a high degree of functionality or utility; and finally, (4) a set of content categories delineating the generality of psychologically abstracted situations which interact with the stable personal characteristics. Until all of these can be encompassed in an integrated system, it is unlikely that consistent prediction in complex situations can be achieved.

The literary tradition is still strong in personality theorizing. This is the tradition most clearly exemplified in this chapter by Freud and Adler, in which the theorist is concerned primarily with global insights into the nature of man. Such theories are more oriented toward vivid description than toward the development of scientific terminology with carefully selected constructs which meet stringent criteria for an ideal systematic construct. Nevertheless, the trend is away from such literary interpretations of personality, and it may well be that solid beginnings are now being made in the development of careful, scientific theory building.

In general, from all quarters there is a recognition that the problem of adequately describing personality is highly complex. A few simple variables based on psychopathological types are not sufficient to describe the complex nature of man, to predict his specific behaviors or even his potential pathological conditions. In addition, many theorists now recognize that the complex situation in which man acts no longer can be ignored. Rather, the psychological situation must be treated as a major systematic variable instead of as a source of error in prediction.

REFERENCES

ABRAHAM, K. The influence of oral-eroticism on character formation. *Selected Papers.* London: Hogarth Press, Ltd., 1942.

ADLER, A. *The Practice and Theory of Individual Psychology.* New York: Harcourt, Brace and Company, Inc., 1924.

———. *The Education of Children.* New York: Greenberg, Publisher, Inc., 1930, 166-190.

———. *Social Interest: A Challenge to Mankind.* New York: G. P. Putnam's Sons, 1939.

ALEXANDER, F., AND FRENCH, T. M. *Psychoanalytic Therapy: Principles and Application.* New York: The Ronald Press Company, 1946.

ALLPORT, G. W. *Personality: A Psychological Interpretation.* New York: Henry Holt and Company, Inc., 1937.

———. *Pattern and Growth in Personality.* New York: Holt, Rinehart and Winston, Inc., 1961.

ANSBACHER, H. L. "NeoFreudian" or "NeoAdlerian?" Report on a survey conducted among members of the American Psychoanalytic Association. *Amer. Psychologist,* 1953, **8,** 165-166.

BRUNSWIK, E. *Systematic and Representative Design of Psychological Experiments.* Berkeley, Calif.: University of California Press, 1947.

———. The probability point of view, in M. H. Marx (ed.), *Psychological Theory.* New York: The Macmillan Co., 1951.

BURT, C. *The Factors of the Mind.* London: University of London Press, 1940.

CANTRIL, H., AND ALLPORT, G. W. *The Psychology of Radio.* New York: Harper & Brothers, 1935, p. 115.

CATTELL, R. B. Personality theory growing from multivariate quantitative research, in S. Koch (ed.), *Psychology: A Study of a Science.* Vol. 3. New York: McGraw-Hill Book Company, Inc., 1959.

CHODORKOFF, B. Self perception, perceptual defense and adjustment. *J. abnorm. soc. Psychol.,* 1954, **49,** 508-512.

CRANDALL, V. J. Induced frustration and punishment-reward expectancy in thematic apperception stories. *J. consult. Psychol.,* 1951, **15,** 400-404.

CROWNE, D. P., AND MARLOWE, D. *The Motive for Approval: Studies in Evaluative Dependence.* New York: John Wiley & Sons, Inc., 1965.

EYSENCK, H. J. *The Structure of Human Personality.* New York: John Wiley & Sons, Inc., 1953.

FREUD, S. *Beyond the Pleasure Principle.* London, Vienna: The International Psycho-analytical Press, 1922.

———. *The Basic Writings of Sigmund Freud.* New York: Modern Library, 1938.

GLOVER, E. Notes on oral character formation. *Int. J. Psychoanalysis,* 1925, **6,** 131-153.

GOLDMAN-EISLER, FRIEDA. Breastfeeding and Character Formation. In Kluckhohn, C., and Murray, H. A. (eds.), *Personality in Nature, Society and Culture.* New York: Alfred A. Knopf, Inc., 1959, p. 155.

HARLOW, H. F. The formation of learning sets, *Psychol. Rev.,* 1949, **56,** 51-65.

HARTMANN, H. *Ego Psychology and the Problem of Adaptation.* New York: International University Press, 1958.

HELSON, H. Adaptation level as a basis for quantitative theory of frames of reference. *Psychol. Rev.,* 1948, **55,** 297-313.

HILGARD, E. R. Experimental approaches to psychoanalysis. In E. Pumpian-Mindlin (ed.), *Psychoanalysis as Science.* Palo Alto, Calif.: Stanford University Press, 1952.

JESSOR, R. The generalization of expectancies. *J. abnorm. soc. Psychol.*, 1954, **49**, 196-200.

JOHNSON, W. *People in Quandaries.* New York: Harper & Brothers, 1946.

KANTOR, J. R. *Principles of Psychology.* Vols. 1, 2. New York: Alfred A. Knopf, Inc., 1924.

KRIS, E. Notes on the development of the ego and on some current problems of psychoanalytic child psychology. In *The Psychoanalytic Study of the Child.* Vol. 5. New York: International University Press, 1950.

LEVY, D. M. The relation of maternal overprotection to school grades and achievement tests. *Amer. J. Orthopsychiat.* 1933, **3**, 26-34.

LEWIN, K. *A Dynamic Theory of Personality.* New York: McGraw-Hill Book Company, Inc., 1935.

———. The nature of field theory. In M. H. Marx (ed.), *Psychological Theory,* New York: The Macmillan Co., 1951.

LINTON, R. *The Cultural Background of Personality.* New York: D. Appleton Century Company, Inc., 1945.

MAHRER, A. R. The role of expectancy in delayed reinforcement. *J. exp. Psychol.*, 1956, **52**, 101-105.

MEAD, G. H. *Mind, Self and Society.* Chicago: University of Chicago Press, 1933.

MEEHL, P. E. On the circularity of the law of effect, *Psychol. Bull.*, 1950, **47**, 52-75.

MILLER, N. E. Central stimulation and other new approaches to motivation and reward. *Amer. Psychologist,* 1958, **13**, 100-108.

MISCHEL, W. Father-absence and delay of gratification: cross-cultural comparisons. *J. abnorm. soc. Psychol.*, 1961, **63**, 116-124.

OLDS, J., AND MILNER, P. Positive reinforcement produced by electrical stimulation of septal area and other regions of rat brains. *J. comp. physiol. Psychol.*, 1954, **47**, 419-427.

NEWCOMB, T. M. *Social Psychology.* New York: The Dryden Press, Inc., 1950.

RANK, O. *Will Therapy.* New York: Alfred A. Knopf, Inc., 1936.

RAPAPORT, D. The structure of psychoanalytic theory. In S. Koch (ed.), *Psychology: A Study of a Science.* Vol. 3, New York: McGraw-Hill Book Company, Inc., 1959.

ROGERS, C. R. A theory of therapy, personality and interpersonal relationships, as developed in the client-centered framework. In S. Koch (ed.), *Psychology: A Study of a Science.* Vol. 3. New York: McGraw-Hill Book Company, Inc., 1959.

———. *On Becoming a Person: A Therapist's View of Psychotherapy.* Boston: Houghton Mifflin Company, 1961.

———. *Client-centered Therapy, Its Current Practice, Implications, and Theory.* Boston: Houghton Mifflin Company, 1951.

ROTTER, CLARA B. The relation of sibling isolation and socio-economic status to reading and arithmetic achievement. Unpublished M. A. thesis, Indiana University, 1942.

ROTTER, J. B. An analysis of Adlerian psychology from a research orientation. *J. Indiv. Psychol.* 1962, **18**, 3-11.

———. *Social Learning and Clinical Psychology.* Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1954.

———. The role of the psychological situation in determining the direction of human behavior. In M. R. Jones (ed.), *Nebraska Symposium on Motivation,* 1955. Lincoln, Nebraska: University of Nebraska Press.

———. Some implications of a social learning theory for the prediction of goal directed behavior from testing procedures. *Psychol. Rev.*, 1960, **67**, 301-316.

———. FITZGERALD, B. J., AND JOYCE, J. A comparison of some objective measures of expectancy. *J. abnorm. soc. Psychol.*, 1954, **49**, 111-114.

———. SEEMAN, M., AND LIVERANT, S. Internal versus external control of reinforcement: a major variable in behavior theory. In N. F. Washburne (ed.), *Decisions, Values and Groups.* Vol. 2, New York: Pergamon Press, 1962.

SCHRODER, H. M., AND ROTTER, J. B. Rigidity as learned behavior. *J. exp. Psychol.*, 1952, **44**, 141-150.

SEARS, R. R. *Survey of Objective Studies of Psychoanalytic Concepts.* New York: Soc. Sci. Council Bulletin No. 51, 1943.

SHELDON, W. H. *The Varieties of Human Physique; An Introduction to Constitutional Psychology.* New York: Harper & Brothers, 1940.

SKINNER, B. F. *The Behavior of Organisms.* New York: Appleton-Century-Crofts, Inc., 1938.

SULLIVAN, H. S. *The Interpersonal Theory of Psychiatry.* New York: W. W. Norton & Company, Inc., 1953.

THORNDIKE, E. L. *The Psychology of Wants, Interests and Attitudes.* New York: D. Appleton-Century Company, Inc., 1935.

TOLMAN, E. C. *Purposive Behavior in Animals and Men.* (2d ed.) Berkeley, Calif.: University of Calif. Press, 1949.

WHITE, R. W. Motivation reconsidered: the concept of competence, *Psychol. Rev.*, 1959, **66**, 297-333.

WITTELS, F. The neo-Adlerians. *Amer. J. Sociol.*, 1939, **45**, 433-445.

CHAPTER 13

The Deviant Personality

E. JERRY PHARES
Kansas State University

INTRODUCTION

Since the advent of Freud the field of psychopathology has progressively increased in importance. Its contribution to the prediction of human behavior has been great and its influence on personality theory quite significant. Indeed, any behavior theorist would be well rewarded by even a cursory attempt at extending his theory to the traditional psychopathological phenomena. As a field, psychopathology possesses a rich store of insights regarding human functioning. However, at the same time, it has, as a field, become a confusing and often contradictory amalgam of theory, clinical insights, value judgments, medical analogies, and sheer superstition. It is our belief that the field suffers primarily from ills that are largely conceptual in nature. In view of this belief, we shall be particularly concerned in this chapter with problems and conceptions that seem to be impeding progress. As a result, there will be no attempt to duplicate chapters of traditional psychopathology for practitioners; such material is available elsewhere (Arieti, 1959). Rather than trying to elaborately, and perhaps tediously, explain every syndrome or symptom we shall select examples which seem to explicate points of view or to illustrate conceptual problems. Because of this goal and also because of space limitations we shall omit certain of the traditional content areas of psychopathology. For example, we shall not deal systematically with mental deficiency, organic psychoses, or psychotherapy, although we will have occasion to discuss specific aspects of them. In effect, we shall concentrate largely on what have come to be labeled as the neuroses and functional psychoses.

Every beginning student in psychology immediately learns that psychology is the study of behavior. Quite obviously then, the study of "abnormal" behavior represents a specific segment of behavior generally. Therefore, before we take up the problems, processes, and variables that have been postulated to account for abnormal behavior, it seems only fair that we meet the obligation of trying to define what we mean by abnormal behavior.

If one attempts to compile a list of abnormal behaviors, the list soon becomes quite imposing. For example, a typical mental examiner's handbook (Wells and Ruesch, 1945) utilizes the following classificatory scheme: simple maladjustment composed of three subclasses; psychoneurosis composed of ten subclasses; behavior problems consisting of four subclasses; major functional psychoses composed of five classes; and mental disorders with structural brain damage consisting of ten subclasses. When one considers all the possible behavioral manifestations of each of the subclasses, the list is exceedingly large. Such a state of affairs suggests a severe problem in the field.

Because of this problem and also because of space limitations we shall assume that the reader is generally familiar with the behavioral phenomena presented in the traditional abnormal psychology or psychiatric textbook.

PROBLEMS IN DEFINITION

A major problem in the field of psychopathology has been the great difficulty in defining the concept. This conceptual problem has, in turn, led to considerable research difficulties (Scott, 1958). In this section we shall review critically some of the major classes of definitions of abnormality and related concepts and attempt to move toward a resolution of the problem. In so doing we shall argue that not only is the whole concept of abnormality an outmoded one but also that continued preoccupation with the term is a hindrance to progress in the field.

Statistical Definitions. According to this class of definitions, anyone who departs from the mean or some area surrounding the mean is to be considered pathological. Thus, one who hallucinates is abnormal since most people do not hallucinate. Or, a person who is extremely aggressive is abnormal because most people are only moderately aggressive. This approach is subject to several major difficulties.

First, it puts us in the peculiar position of asserting that an extremely bright, or an exceptionally thoughtful, or an extremely perceptive individual is thereby pathological. Quickly someone argues that it is not these kinds of deviations that are to be considered pathological. To so argue, however, leads us to the point where it becomes necessary to posit "good" deviations and "bad" deviations—hardly a comfortable position for many who wish to remove pathology from the realm of values. In fact, it was not too many years ago that the prevalent concepts of normality revolved solely around conformity to community standards.

A second problem with statistical approaches is that someone must decide how deviant a behavior must be before it is to be considered as abnormal. In short, what are the cutoff points and what rational justification is there for placing them at any given point? Third, in how many respects must one be deviant before earning the title of pathological? Surely no one conforms to the mean in every respect. This being so, how many different behaviors may deviate from the norm? And finally, is pathology to be inferred from deviant behavior(s) regardless of their temporary or chronic nature?

Cultural Relativism. A second and closely related conception of pathology is only slightly more sophisticated in its outlook. According to this view, all behavior should be judged with respect to the reference group of the individual. This notion is essentially the same as those of the statistical approaches except that the norms are many and a judgment about pathology can only be arrived at after study of the individual's culture. Thus, what is normal in Peoria may not be so in Katanga or even Kalamazoo. This approach would seem laudable except that it tends to place the reference group above reproach. Also, one can reduce cultures into subcultures and even into sub-subcultures—where does one stop? Furthermore, there appears to be a strong implication of the deification of conformity. There is no room for the nonconformist (or how much room there is tends to be vaguely defined). Indeed, it is often the nonconformist who is considered by many to be most responsible for so-called improvements in society. And of course, there are even those who have argued that "excessive" conformity is itself pathological!

Subjective Report. It has been asserted that regardless of whether one conforms or appears to conform or to be adjusted on the basis of the foregoing criteria, the important thing is how one feels. If a person feels anxious or unhappy he is abnormal. Here we have undue emphasis on the phenomenological report. The limitations of phenomenology are too well known to need recounting here. Needless to say, however, most clinicians have talked with bizarre psychotics whose subjective reports of tranquility were somewhat at odds with their institutionalization. Furthermore, most of us experience distress or anxiety. Surely the absence of anxiety or distress is not the criterion. If not, then how much distress is permissible and for how long? Quite obviously, being alive and existing in an environment is bound to bring some frustration or distress. At any rate, it is difficult to envisage an environment that did not produce a certain amount of unhappiness.

Symptom Manifestation. Compilation of a master list of symptomatic behaviors might appear to be a solution to the criterion problem. This approach, however, is simply a reinstatement of the foregoing procedures usually in the guise of a checklist or inventory or perhaps a mental examiner's handbook. At any rate, all the previous objections apply with equal force.

Attempts at Mastery. Other definitions revolve about coping behavior. He who displays coping behavior is normal. Of course, arriving at agreement among independent judges as to what constitutes coping in the face of certain environmental threats may be difficult. Likewise difficult to handle are definitions emphasizing adaptation, fulfillment of one's potentialities, or confrontation of one's problems. One man's adaptation may be another's defense. In other words, how are we to decide what is adaptation, fulfillment, or confrontation? And certainly any attempt to define positive striving is hopeless from the outset.

Theoretical Definitions. At perhaps another level of analysis are those definitions couched in some theoretical frame of reference. For example, to the psychoanalyst the absence of repression would appear to be the epitome of adjustment. Certainly many therapeutic hours are spent in an attempt to make the unconscious conscious. But to argue that because one is aware of his hostile urge to injure his father one is thereby adjusted hardly sits well. In all fairness, the analyst may protest that conscious entertainment of such an impulse presupposes the existence of additional repressions. However, such an argument does not appear very convincing when applied to the many situations where lack of learning rather than repression seems to be responsible for antisocial behavior, unhappiness, or distress.

At the opposite theoretical pole are constructs derived from a social learning theory (Rotter, 1954) which have been at times applied to normalcy or adjustment questions. Thus, it is argued that when a person places a high value on a particular goal and also has a high expectancy that certain behaviors will be successful in attaining it, then a state of adjustment exists. Perhaps such is often the case. Nevertheless, this position leads us to assert that our previous friend who places a high value on hurting his father is adjusted if he expects to be successful in the enterprise.

Examples from these two quite different theories are taken to illustrate the point that a definition of abnormality cannot be derived from psychological theory. That is, to define abnormality is to go outside one's theoretical system.

Mental Illness. All too frequently abnormality is equated with "mental illness." As Szasz (1960) has so cogently argued, mental illness is largely a myth. The concept has gained public acceptance through the attempts of many well-meaning professionals to reduce the public's fear and prejudice toward people with problems in living. Mental illness has come to be equated with physical illness and is said to be nothing to be ashamed of. The parallel attempt to make cancer a less fearful illness might be noted. Although the attempt to gain more tolerance and better treatment facilities for people with problems was and is laudable, it has had the unfortunate side effect of perpetuating a hoax.

Surely the so-called mentally ill person is not suffering from brain damage (excepting certain very specific organic conditions) any more than is someone who espouses highly repugnant attitudes toward racial minorities. What we really mean by mental illness is that the person is deviating in his behavior from certain norms that someone regards as valuable. Furthermore, even should one accept the concept of mental illness, the very same practical problems of definition, cutoff points, norms, agreement among judges, etc., exist. The whole concept of mental illness is closely allied to the disease entity approach in psychiatry as well as to the notions that physiological or chemical bases exist as the fundamental explanation for all types of psychopathology. These topics will be dealt with in detail in a following section.

The inevitable conclusion from the foregoing is that a definition of abnormality (maladjustment, pathology, etc.) is possible only with reference to a set of value judgments. To characterize someone as abnormal is to assert that he needs treatment. In short, someone has decided that the patient needs help in changing his behaviors—a relative, a court, or perhaps the patient himself. Certainly a psychiatrist or clinical psychologist is in no position to decree that an abnormality exists. Once someone decides that the patient needs treatment, then our psychiatrist or psychologist can deliver an opinion on how best to effect the desired changes. But the decision for treatment as a function of abnormality must be based on someone's value system—it does not reside in psychiatry or psychology.

That abnormality cannot be defined apart from a value system appears to distress many psychologists and psychiatrists. They strive to arrive at definitions of a nonethical quality. However, all too often their definitions tend to become subtle extrapolations from some set of values, notably the Protestant ethic. What is needed, however, is not a valueless definition but rather an explicit recognition that values do operate in one's relations with patients. What the clinician ought to concern himself with is more precise techniques of diagnosis and treatment. As noted before, the clinician should strive to be an expert in the prediction of human behavior. This position allows him to predict which therapy or which environmental forces will affect his patient's behavior. The decision to institute therapy and thus tacitly admit that an abnormality exists must come from an explicit set of values, be it the clinician's, a relative's, or society's. The clinician may be more sensitive than the layman to the prodromal signs of cultural deviations but the decision to institute treatment is still a value judgment.

Given the foregoing analysis, the question arises whether or not the whole concept of abnormality is a particularly salient one. Certainly expending many hours arriving at a simple diagnosis of abnormality has little predictive merit in itself. Such a label, in the absence of more information, tells us little if anything. If we mean by abnormal only that someone regards the person's behavior at odds with some set of values or standards, then we might well eliminate the word altogether because of its unfortunate surplus meaning.

Thus, regardless of what kind of definition we start with, it soon devolves into a judgment based on a particular set of values. The danger in this is not so much that values enter into these decisions but that we delude ourselves and others into thinking that values are not operative. One is reminded of the clinician's cherished belief that values do not enter into his therapy relations with patients. No one conducts therapy without implicitly or explicitly allying himself with a set of values (Glad, 1959). Merely saying to the patient, "I will not tell you what to do but I will help you examine the alternatives so you can make your own decision," hardly qualifies as a means of ruling out values.

Explicit recognition of what we are doing would seem the "healthier" alternative. We are, however, still saddled with the problem of standardizing definitions of some sort. Lack of such standardization has led to an obvious confusion in generalizing and coordinating research findings (Scott, 1958).

THE DISEASE ENTITY APPROACH

The predominant view in the field of psychopathology is the psychiatric one. More specifically, it is, explicitly or implicitly, the disease entity approach. Historically, we may trace the origin of this view to Kraepelin (1913), while currently its embodiment, little changed, is represented in the official nomenclature of the American Psychiatric Association (1952).

Essentially, this view takes certain behaviors that are considered pathological by some set of standards and labels them as symptoms. This allows one to impose upon these behaviors a medical orientation. The medical orientation, of course, involves a procedure whereby symptoms are related to specific disease processes or entities. Disease entities are listed followed by the symptomatology associated with each. This same procedure was adopted by Kraepelin in his pioneer work in the development of a classificatory scheme for so-called mental disorders. That is, an attempt was made to apply the medical model to the field of psychopathology. Since it was primarily medical men who were active in the field at the time, it was perhaps only natural that they approached problems with a medical conceptual scheme. Historically, then, we have been left saddled with the dichotomy of healthy versus diseased. What early workers, and many present ones as well, failed to recognize was that the "symptoms" being dealt with were social behaviors. Therefore, as Szasz (1956) notes, the current emphasis on normal–abnormal is a function of the attempt to force social behaviors into a medical conceptual system.

Many people seem to confuse conceptual systems with reality. Conceptual systems, be they medical or otherwise, are merely convenient ways of abstracting reality for predictive purposes. The question is not whether certain behaviors are symptomatic of disease processes, but whether such a view is a useful way of abstracting reality. There are other ways of construing the world—social, psychological, and even religious. As Kelly (1955) points out, each mode of description has a particular focus of convenience and utility. We thus propose to question whether the medical model is a useful one to impose on the phenomena typically dealt with under the label of psychopathological.

The major limitations of the psychiatric diagnostic approach are several. First of all, the very language and structure of the system encourage a reifying process. The constant reference to "disorders," "symptoms," and "conditions" seduces even the careful student into a view which reifies. The analogy might be drawn between the foregoing and the tendency within psychoanalysis to reify concepts. For example, even though one recognizes "ego" to be a construct, constant reference to it as something which experiences anxiety, initiates repressions, executes superego demands, etc., leads one unavoidably to consider it as a thing which exists in the brain someplace.

Basically, psychiatric categories represent a typological approach. Based on certain symptoms, a patient is classified as a schizophrenic, sociopath, or just anxious. All too often, as Lewin (1935) pointed out, such systems seem to seek to explain by classifying. That is, by assigning one to some class, the implication is that his behavior has somehow been explained. Likewise, Allport (1937) has criticized typologies for their failure to emphasize the idiosyncratic aspects of behavior. Perhaps, however, the most compelling criticism of typologies as represented by psychiatric diagnosis is their failure to take account of situational determinants in the prediction of behavior (Rotter, 1954). Instead, complete reliance is placed on internal characteristics or the assignment to class membership.

It is obvious that behavior is not static. It varies as the situation does. However, the psychiatric-medical model seemingly provides no basis for incorporating a situational analysis. Liverant (1963) states the case quite well when he asserts that once a patient is invested with the qualities of paranoia, there are few conceptual rules for predicting in which situations paranoid behavior is likely to take place. The role of the psychological situation as a determinant of goal-directed behavior has been demonstrated many times previously (Masling, 1960; Phares and Rotter, 1956). Likewise, Rotter (1960) has carefully pointed out the difficulties with systems that rely almost entirely on internal characteristics at the expense of situational factors. At an observational level, one sometimes wonders whether the adjustment status of a chronic schizophrenic on the back wards of a hospital is a function of some presumed disease process or largely a response to the situation in which he finds himself.

At another level, Zigler and Phillips (1961*b*) have discussed the oft-repeated criticisms of psychiatric diagnosis which state that the categories lack homogeneity, reliability, and validity. These are basic criticisms which have been repeatedly documented. However, it seems that each new generation of clinicians must learn the three basic lessons of psychiatry: (1) people within the same diagnostic category are notoriously dissimilar from one another; (2) psychiatrists have never been able to agree among themselves as to who belongs in which category; and (3) categorization of patients has little to do with their treatment and/or disposition.

That the phenomena subsumed under a given psychiatric label are exceedingly diverse has been discussed and demonstrated repeatedly (King, 1954; Wittenborn, 1952; Wittenborn and Bailey, 1952; Wittenborn and Weiss, 1952). Although one can agree with Zigler and Phillips (1961*b*) that homogeneity of classes and reliability of categorizations are separate concepts, it seems difficult to escape the conclusion that in this case a system which allows such heterogeneity to exist will be very unlikely to demonstrate significant behavioral correlates accompanying class membership. This seems especially likely when diagnoses are based presumably on presenting behavior to a large extent rather than dynamics. In a recent study, Zigler and Phillips (1961*a*) have demonstrated the frequent observation that class membership conveys almost no information regarding symptoms and that a wide variety of symptoms may be related to more than one diagnostic category.

A serious criticism of psychiatric nosology centers around the concept of reliability. Boisen (1938), Mehlman (1952), Roe (1949), and others have demonstrated the considerable unreliability of psychiatric diagnosis. Few psychiatrists agree on what weights to assign given symptoms in the complex presented by a particular patient. The result is a situation where one psychiatrist is especially influenced by the patient's gesturing or posturing—thus a diagnosis of catatonia. Another psychiatrist looking at the same patient is impressed by the patient's grimacing or giggling—thus a diagnosis of hebephrenia. The practicing hospital clinician is often confronted with patients who have been re-admitted to the hospital repeatedly—each time with a different diagnosis. Such a situation attests either to the unreliability of diagnosis or to the remarkable plasticity of human behavior.

In this connection it appears unusual that more research has not been undertaken to relate lack of reliability to the concept of situationalism. Thus, it may be possible that what ordinarily emerges as psychiatric unreliability is in part a function of the patient's differential response to varying cues in the diagnostic situation. In short, the stimulus value of the diagnostician, the conditions under which the patient is being diagnosed, etc., all contribute to alter his behavior and thus perhaps partially create unreliability of diagnoses. As Jackson (1962) points out, schizophrenics often show a remarkably obliging responsiveness to the theoretical biases of the investigator. A great deal of data bearing on the foregoing already exist in the field of testing (Lord, 1951; Masling, 1960).

Nevertheless, whatever the reasons for unreliability, the fact of its existence is such as to render the whole system nearly useless. If diagnoses are to represent something more than mere intellectual exercises, specific behavioral correlates—ward behavior, response to therapy, projective test responses, etc.—must accompany them. But how can such correlates ever be identified as long as the system cannot demonstrate satisfactory reliability? In other words, as long as we cannot agree on the principles by which individuals are assigned to categories, how can we possibly make valid deductions about the behavioral correlates of these categories? Furthermore, in view of the suggestion that widespread geographical differences in the predilections for certain diagnoses exist (Boisen, 1938), validity studies from different hospitals and geographic regions offer considerable generalization problems.

Thus far the validity aspect of psychiatric diagnosis has been little touched in research. One is amazed that a system can command such widespread use with hardly a particle of research evidence concerning its validity or utility. In the absence of research evidence, we are left with the inescapable conclusion that no satisfactory basis for the use of diagnostic categories exists.

Several additional difficulties remain for the Kraepelinian disease entity approach. First of all, the bases or principles of diagnoses are several. Principles of classification based on symptoms, prognosis, social history, and etiology (including tissue damage and foreign organisms) all exist side by side in the same system. This kind of inconsistency leads to considerable confusion. Also, an assumption, either explicit or implicit, that often accompanies the Kraepelinian approach as well as many other typological approaches is that of an ultimate constitutional or somatic cause for abnormal behavior. Although we shall deal with this topic more fully later, suffice it to say at this point that there is considerable evidence to show that many so-called syndromes can be more usefully handled with psychological constructs.

A final problem centers around our definition of symptoms. As Zigler and Phillips (1961*b*) emphasize, the number of behaviors which can potentially be included under the label "symptom" is practically infinite. Particularly when we are dealing with the milder, so-called neurotic category, almost any behavior can possibly be viewed as neurotic depending on associated conditions. This fact raises considerable problems for one bent on constructing a better classificatory system.

Currently, the diagnostic system in use is essentially a descriptive one. Its predictive values are extremely limited except in certain legal and administrative senses—hardly impressive testimonials for its value. Certainly its prognostic and etiological implications are minimal. And research reports which describe populations studied as schizophrenic, brain damaged, or manic-depressive often defy comparison with other supposedly similar studies because of the inherent uncertainty in population characteristics.

As noted earlier, a prerequisite for the validation of the current diagnostic approach is the establishment of a reliable classificatory scheme. If the behavioral correlates of this approach are to be determined so as to test the efficacy of this conceptualization once and for all, such reliability is imperative. Working within the traditional diagnostic framework, Overall and Gorham (1962) have developed an instrument, the Brief Psychiatric Rating Scale, which provides an evaluation of patient symptomatology in sixteen relatively independent factor areas. In subsequent work, Overall and Gorham (1963) have provided some preliminary evidence that psychiatric types have associated discrete symptom profiles and that a statistical model for psychiatric classification is possible. The particular merit of this research program seems to be its attempt to achieve reliability in psychiatric diagnosis. Such reliability, even though achieved in the foregoing work by means of using stereotyped cases, seems promising. Likewise, Lorr, McNair, Klett, and Lasky (1962) have provided some evidence for the reliable classification of ten psychotic syndromes. Although such work in no way yet sheds much light on the validity of current psychiatric diagnostic procedures, it does provide a step toward the achievement of reliability.

Of course, even should procedures such as the foregoing ultimately result in reliable techniques and should we be able to reduce the heterogeneity which currently exists within categories, we have no assurance that we are dealing with anything other than phenotypical similarity. That is, should we be able to place three behaviorally similar individuals into the manic-depressive category, it may turn out that they behave similarly for different reasons. In all probability, this likelihood is responsible for our current inability to relate symptomatology to prognosis and etiology. Nevertheless, reliable means of classifying patient behavior is a prerequisite for adequate research in this field.

Thus far we have been talking about validity as if it were a unitary phenomenon. Actually, no single classificatory system can be expected to exhibit validity for every predictive purpose. Such schemes of classification would be expected to be valid for some purposes and invalid for others. An analogy might be drawn in connection with the field of projective testing, where for too many years clinicians have been utilizing "omnibus" tests that are supposed to predict everything from overt behavior to marriage suitability. What is needed are several systems each developed with a different purpose in mind. The scheme that predicts successful ward assignment or legal competence is unlikely to predict response to psychoanalytically oriented therapy as administered by Dr. X.

Granting the potential usefulness of several classificatory schemes, each designed with a specific predictive purpose, both for practical and research purposes, a significant reservation must be held. Even should we divorce our scheme of classification from any implications of disease entities, and even should we recognize that our scheme is purely descriptive in nature, basically it remains a typology. As a typology it will provide no systematic way of incorporating situational variables into the scheme. Particularly when we hope to predict such things as response to treatment, prognosis, or vocational assignment, variables such as needs, expectancies, and the situational cues which arouse such needs and expectancies, are of crucial significance. Phenotypical similarity, as revealed by most diagnostic schemes, is a poor basis on which to infer such concepts as needs, expectancies, and ego strength.

The foregoing discussion constitutes, perhaps, an essentially destructive rather than constructive analysis of current concepts in the field of psychopathology. However, after discussing some of the bases of psychopathology in the following sections, we shall return to these initial observations and attempt to suggest some potential resolutions. In the meantime, it may be useful to summarize the ground already covered.

We have noted that all attempts to define abnormality ultimately devolve into value judgments. The two most frequent definitions, statistical and relativistic, only temporarily evade the problems of values. Likewise, definitions based on various theoretical constructs prove unsatisfactory when applied to the whole spectrum of human behavior. Another prevalent notion involves the concept of mental illness. Although usually applied to the grosser deviations, its actual range of application is rather uncertain. At any rate, its inadequacies are no less than the previous notions.

A closely allied point of view is perhaps the disease entity approach as represented historically by Kraepelin and currently by the official nomenclature of the American Psychiatric Association. This approach would seem to be the dominant one currently in the field, if one can judge by the number of psychiatric and abnormal psychology texts organized on the basis of psychiatric categories. Several basic limitations of this model were discussed.

First, the language and structure of the system encourage a reifying process. Second, the system is a typological one that leaves out situational variables and often seems to explain by naming. Third, the categorical approach is an unreliable one in the sense that there is little agreement as to who belongs in which category. Some current factor-analytic approaches are being pursued in an attempt to demonstrate reliability. Fourth, an associated criticism is that individuals within a given category represent an extremely heterogeneous group. Fifth, and perhaps because of the foregoing limitations, the system has yet to demonstrate significant validity in any important predictive areas. Sixth, by utilizing a "symptom" approach, the system must perforce define its symptoms. In practice, this becomes an extremely difficult matter since the list of possible symptoms is nearly infinite in number.

GENETICS

One of the oldest hypotheses broached to explain deviant behavior is that of heredity. That such an hypothesis should occur is understandable when one observes the frequency with which deviant parents seem to beget deviant children. However, such a simple hypothesis turns out, upon careful analysis of data and method, to be, more often than not, a singularly inadequate one.

Intelligence. In the field of intelligence the genetic hypothesis has received some of its greatest acceptance. As long as intelligence was conceived to be a unitary, unmodifiable entity solely determined by genes as laid down at conception and nurtured by "proper" environmental conditions, a genetic explanation seemed to offer great potential. As commonly stated, heredity set the limits within which environmental forces could play a strictly secondary role. Such classic investigations as those of Goddard (1912), which traced the accomplishments of the Kallikak family, seemed to reinforce notions of a fixed intelligence. Likewise, twin studies, which showed that identical twins manifested marked similarity in intelligence test scores while fraternals showed a lesser degree of similarity, seemed to indicate the role of heredity inasmuch as environment was presumably being held constant and degree of genetic relationship varied (Newman, Freeman, and Holzinger, 1937). A related view has been to consider that intellectual *development* is predetermined on genetic grounds. The child's development unfolds on a predetermined, invariable basis. Such a notion gained considerable credence from Coghill's observations (1929).

However, such notions have recently been undergoing considerable revision (Hunt, 1961; Liverant, 1960). Both Hunt and Liverant have marshaled evidence to show that ideas of fixed intelligence and predetermined development are outmoded ones. No characteristic occurs as a sole function of either heredity or environment. There is an interaction between the two. The occurrence of any specific characteristic is a function of genetic factors operating within a specific environmental milieu. Within any environment there are perhaps fixed potentials, but the net result is that in practice genetic potential becomes a function of all possible environmental variations, just as the limits of environmental influences are moderated by genetic potentials or limits.

Entirely consistent with this view of genetics is the work of several investigators. Harlow's work on learning sets (1949) suggests that intercourse with the environment enhances problem solving ability. One's ability to solve a problem is a function not only of genetics but also of previous experience with similar problems. Likewise, the work of Hebb (1949) and of Piaget (see Hunt, 1961) are consistent with the view that a person's behavioral repertory is greatly a function of his opportunities for perceptual-motor experience with his environment.

Thus, as Liverant (1960) suggests, the more feasible course would appear to be investigations designed to point up the situational conditions which either facilitate or inhibit one's acquisition and expression of problem solving behaviors.

Functional Psychosis. In the area of the behavior disorders, the most concerted effort in researching the role of genetics has occurred with psychotic reactions. Because of the striking behavioral manifestations occurring in psychoses the disease entity model was quickly adopted and with it came the genetic hypothesis. Similar incentive also came from studies of organic psychosis which linked structural defects with genetic factors. *Huntington's chorea* is a striking case in point, where atrophy of structures in the cerebrum and corpus striatum is accompanied by progressive deterioration in the emotional and intellectual spheres. Such cases occur only where a parent has been similarly affected.

As representative of the work in this area we shall take Kallmann's investigations of the role of genetics in schizophrenia (1959). Kallmann presents data which show that there is an "increased frequency of schizophrenia rather than any other type of disorder, with the magnitude of the increase proportional to the degree of consanguinity to the schizophrenic relative" (Kallmann, 1959, pp. 190-191). In support of this assertion Kallmann reports the incidence of schizophrenia in the general population to be about 1 per cent. However, the rates of congruence among relatives are as follows: full siblings (14.3 per cent); half-siblings (7.1 per cent); stepsiblings (1.8 per cent); children who have two schizophrenic parents (68.1 per cent); children who have one schizophrenic parent (16 per cent).

The concordance rates between two-egg twins is 14.7 per cent, while the corresponding rate for one-egg twins is 85.8 per cent. Clearly, as the degree of relationship to a schizophrenic relative increases, so too does the probability of becoming schizophrenic. The problem here seems less a matter of facts than the interpretation of the facts. Kallmann, of course, sees his percentages as being irrefutable proof for the genetic explanation of schizophrenia. However, other interpretations of his data are possible for several reasons.

First, it is impossible to determine from data like Kallmann's whether the inheritance is on a genetic basis or a social learning basis. Both genetic inheritance and environment during the formative years are provided by the parents. That schizophrenic parents would provide an environment likely to lead to gross behavioral deviations on the part of their children also seems irrefutable. Thus, Kallmann's data seem to be just as much proof for the environmental hypothesis as for the genetic one.

Kallmann has made great use of the twin method in investigating the influence of genetics in psychopathology. He interprets the extremely high concordance rates (85.8 per cent) for one-egg twins in comparison to less closely related siblings to be genetic evidence. However, one should remember that the environmental forces affecting identical twins are much more similar than in the case of other siblings. One might also note as Jackson (1962) does that the psychological relationship between identical twins is something special. He points out that any two individuals sharing their lives as closely as do identical twins could be expected to share delusions and symptoms, and they might even manifest such a syndrome as that of *folie à deux*. That such phenomena occur is thus not automatically evidence for a genetic position. Pastore (1949) has provided a methodological critique of some of Kallmann's studies.

Other investigators suggest that what is inherited is a predisposition to become schizophrenic, given the appropriate environmental stimulation. However, the term "predisposition" is vague with no operations for its measurement extant. Its value seems more often postdictive rather than predictive. Too often, it seems, writers comment that there is no compelling evidence that schizophrenia is inherited as such, but the possibility remains for a hereditary predisposition. Such statements do not seem to rest on any factual basis. Until someone can provide operations for the reliable measurement of a predisposition, such explanations would seem premature.

Our earlier points concerning the re-examination of intelligence apply equally here as well. Just as intelligence is a function of genetics *and* one's encounters with an environment, so too may be the so-called schizophrenic syndromes. Genes may set limits, but the limits vary with every conceivable environmental eventuality. Thus, the most profitable course would seem to be the investigation of the various situational conditions which produce schizophrenic behavior. Unless the notion of an innate schizophrenic potential can also specify the conditions under which schizophrenia occurs (which it cannot now do), the genetic hypothesis does not appear very promising.

Also, like intelligence, schizophrenia is not a unitary phenomenon. Perhaps the major difficulty in pursuing the genetic hypothesis of schizophrenia is the tacit assumption that it is a unitary thing. Actually, as noted in our previous discussions, the category of schizophrenia subsumes a very heterogeneous group of people, so that it seems unlikely that one could reasonably talk about the inheritance of schizophrenia.

Although we shall deal more fully with the area of psychogenic causality later, suffice it to say at this point that there is evidence that parental attitudinal factors play a considerable role in the development of schizophrenic reactions (Mark, 1953; Garmezy, Clarke, and Stockner, 1961).

In the case of manic-depressive reactions, Kallmann (1953), utilizing twin data, reports that one-egg twins show a concordance rate of 95.7 per cent, while in two-egg twins the figure is 26.3 per cent. Similarly, the congruence rate for half-siblings is 16.7 per cent and for full siblings, 22.7 per cent. These figures are compared with the general population frequency which is about 0.4 per cent. Again, Kallmann sees these rates as evidence for the genetic hypothesis. However, the reservations and alternatives discussed relative to schizophrenia apply here also.

Male Homosexuality. Both the data of Ford and Beach (1951) and Kinsey, Pomeroy, and Gebhard (1948) strongly suggest that homosexuality is not a disease but, rather, a general capacity of all human beings. The expression of homosexual behavior is essentially controlled by learning, experience, and situational conditions.

However, again Kallmann (1959) finds that in the case of 45 one-egg homosexual twins, only one was discordant, while for 51 pairs of two-egg twins nearly 60 per cent discordancy prevailed. Even though Kallmann asserts that all concordant twin partners in his sample denied any mutuality in overt sex relations, it is possible that this denial reflects social taboos. Logically, one might expect identical twins to show a higher homosexuality rate based on the intimacy, both physical and psychological, that they share during their sexual maturation.

Certainly, as in the case of the psychoses, similarity of environments and reaction patterns of parents and significant others cannot be excluded as major determinants. Thus, at the present time, the genetic evidence appears something less than overwhelming.

Neurosis. Here the problem in demonstrating a simple genetic relationship is quite difficult. At least in the case of many psychotic syndromes we are dealing with relatively gross behavioral aberrations. In the case of neurosis, however, what constitutes neurotic behavior is a severe stumbling block. A good case can probably be made for calling everyone neurotic. Certainly all of us show some neurotic-like behaviors. It may literally be true in many cases that to say that neuroticism runs in the family is like saying behavior runs in the family. At least in the case of schizophrenia there seems to be some evidence that relatives of schizophrenics are more than likely to become schizophrenic, but not particularly likely to become manic-depressive or to develop other psychotic reactions. However, in the case of anxiety neurosis, relatives of such people are more prone to anxiety neurosis but also to other neuroses as well (Shields and Slater, 1961). To account for this, Shields and Slater state that in the psychoses single genes of large effect are responsible, while for the neuroses there are polygenic, nonspecific factors involved. This does not appear to be a very tight argument, given the unreliability of psychiatric diagnosis. Indeed, what is attributed to polygenic factors may turn out ultimately to be simple unreliability of diagnosis.

Eysenck and Prell (1951, 1952) found that the neuroticism factor scores of fifty one-egg twins were significantly more alike than similar scores of fifty two-egg twins. They interpret such results as establishing the role of genetics in this field. Such results seem as readily attributable to the environmental position on the same grounds we have covered previously. Of course, Eysenck (1957) takes a generally hereditarian position when it comes to psychopathology. His position is basically a typological one with all the inherent defects we noted earlier. People apparently inherit certain traits. Although described as a learning theory, the conditions under which learning takes place or is expressed is given little attention. Indeed, Eysenck's research is largely oriented around the position of predicting learning from the person's status on a trait dimension. Other investigators would more likely predict standing on a trait dimension from prior learning.

BODY TYPES

Kretschmer (1925) believed that mental patients could be separated into four major body types, each associated with particular forms of or tendencies toward certain mental illnesses. For example, there was: (1) the *asthenic* type (slight, long-boned, slender persons) with the associated *schizoid* and schizophrenic personality type; (2) the *pyknic* type (round, stocky, heavy individuals) with the associated manic-depressive and *cyclothymic* personality; (3) the *athletic* type (strong, muscular, broad-shouldered people) with individuals more likely to become schizophrenic than manic-depressive; and (4) the *dysplastic* type (persons showing disproportionate physical development), the latter again more likely to become schizophrenic than manic-depressive.

The most sophisticated modern proponent of this general orientation is Sheldon (1940, 1942). According to his procedure an individual is given a rating, on a seven-point scale, on each of three dimensions. These are: (1) *endomorphy*, highly developed and massive visceral structures; (2) *mesomorphy*, predominance of musculature; and (3) *ectomorphy*, the delicate, fragile, poorly muscled type. Sheldon can reliably provide ratings for individuals on these body type dimensions. However, he goes further and states that there are highly correlated temperaments that correspond with these somatotypes. Thus, ectomorphs are said to be *cerebrotonic* (restrained, inhibited, and somewhat withdrawn). Mesomorphs are presumably *somatotonic* (active, assertive, and vigorous). Endomorphs are *viscerotonic* (relaxed, comfort-loving, and sociable).

Humphreys (1957) has attacked Sheldon's typology on both statistical and methodological grounds—an attack that seems well-founded. However, even leaving aside these telling points, it remains yet to be demonstrated that there are any *meaningful* relationships between social behavior and this typology. Furthermore, even should there someday be demonstrated low-level correlations between somatotype and behavior, such a relationship would not necessarily be evidence for a constitutional hypothesis. One could argue, as have Cameron and Magaret (1951), that body build may well lead to certain encounters with the environment or stimulate reactions from others such as peers which lead to certain consistencies of behavior. Thus, constitutional predisposition is no more likely an explanation than social learning. Certainly, complex and variable social behavior requires a more complex predictive formula, including situational factors, than the formula that merely equates somatotype with temperament.

BIOCHEMISTRY

We come now to an area of research that has generated an increasing amount of attention over the last few years. This is the attempt to demonstrate some pathological element in the biochemistry or metabolic products of psychiatric patients. This kind of goal is closely associated with the genetic hypothesis, inasmuch as it seems probable that genetic agents would operate through biochemical defects.

Generally speaking, research in this area has been largely empirical with a minimum of deduction from theory involved. As a result, the research field tends to be quite disordered with relationships among empirical investigations difficult to establish. Likewise, the number of conflicting results is quite imposing.

Before discussing some of the empirical data in this field, let us first make some general observations. It seems to the writer that biochemical investigations often make certain assumptions, albeit tacit in some cases, that are worth examining. For example, most biochemical studies are carried out with psychotic patients. The reason for this appears, among others, to be due to the fact that psychosis is seen as a disease. Naturally, if it is a disease, it follows that biochemical investigations would likely be fruitful just as they have been in other disease forms. If one who emits schizophrenic behaviors is thought to be "suffering" from a disease, it is entirely reasonable to expect the biochemist to help us. Such an assumption has led many investigators to search for that elusive biochemical and physiological entity responsible for psychotic behavior.

Of particular note is that once a cluster of behaviors is seen as the outgrowth of a disease process, the investigative efforts tend to switch levels of abstraction. Thus, neurosis is not so frequently attributed to a disease process, therefore, investigations are likely to be pursued at the psychological level—maternal smothering, sexual trauma, etc. But in the case of psychosis there is often a tendency to shift investigative gears and go off looking for the entity. There are probably several reasons for this. Perhaps one is that since extreme psychotic behavior is so bizarre, so self-punishing in many cases, or so far removed from what reasonable men expect, that it could not possibly be learned or maintained at a psychological level. Therefore, it must be a function of a disturbed chemical balance or the presence of an unknown chemical ingredient.

If the foregoing observations are valid ones, an immediate problem arises in the attempt to spell out the differences behaviorally between neurotic and psychotic persons. Such definitions are difficult to achieve. Furthermore, what one classes as a symptom is essentialy a matter of values or cultural norms. Many behaviors which we would label psychotic may be routinely engaged in by members of other cultures. Does this mean that they, like our psychotics, are suffering from a disease even though their culture sanctions the behavior? Or is any *extreme* behavioral deviation in any culture to be considered a product of a disease process? In other words, are certain absolute clusters of behaviors, defined without reference to cultural surroundings, evidence for a disease, or is any extremity of behavior in any culture (even though the behavior differs from culture to culture) evidence?

Another characteristic of the biochemical approach is reminiscent of the old problem of dualism in psychosomatics. For example, the tenor of many arguments seems to be that while the behavior of normals or even neurotics is caused by psychological variables, the cause of psychosis lies in physiology or biochemistry. This seems little different from ascribing the ulcer of a normal person to *physical* causes and the ulcer of a patient in a mental hygiene clinic to *mental* causes.

The question is not whether psychosis is mental or physical. It is obviously both. The difficulty seems to be that many investigators confuse the description of an event with the event itself. So-called psychosis, neurosis, or normalcy may be described from any number of levels of abstraction—biochemical, psychological, economic, religious, etc. The event itself is none of these, it is merely itself. Thus, psychosis is neither psychological nor biochemical but may be described from either of these vantage points. Of course, the conditions antecedent and subsequent to the event should also be described at the same level as the event. Thus, dualism enters when we couch our descriptions of the event at one level and the antecedent conditions at the other. This is like saying that one mode of description causes another. For example, saying that a lesion in the stomach is *caused* by unfulfilled dependency strivings is a case in point. Certainly events or constructs from different modes of description should be expected to correlate with one another and also be consistent. Of course, investigations from one level of abstraction *may* serve as an impetus to new insights and developments at another level.

The point of correlated constructs raises another issue. Is the correlation to be accepted as such, or should one consider levels as showing hierarchical dependencies? The issue is essentially that of reductionism. Must sociological descriptions be reduced to psychological levels and thence to neurology and perhaps to chemistry and further? Or is reductionism essentially without merit? In the area of mental deficiency Cantor and Cromwell (1957) have discussed the issues thoroughly and cogently, and their arguments seem to apply as much to the field of psychopathology generally as to mental deficiency. They discuss several basic difficulties with a reductionistic approach. First, there is often the implication that as we move from more molar levels to more molecular ones we get closer to the "real" or "true" cause of behavior. No theory or description is true, however. They are simply constructions imposed on reality in the sense of being instruments of utility in the hands of scientists. This point is consistent with our previous statement that it is fallacious to look for psychological causes for physiological events or vice versa. Secondly, the assumption that reduction to smaller and smaller units located within the organism will necessarily improve prediction is not borne out. As noted earlier, each descriptive mode has a focus of convenience in that it is maximally useful only for problems construed at that level. To expect the physiologist with his constructs to handle such predictive problems as the expression of prejudice is as patently absurd as asking a social psychologist to predict thyroid secretions using an expectancy construct. It might be added, however, that Cantor and Cromwell are not arguing against the future possibility that it may be necessary to consider certain physiological variables to enhance prediction by providing more comprehensive laws—only that it is not necessary to reduce all concepts to a physiological level. It could also be noted parenthetically as well that reduction to more molecular levels seems to be an infinite process. It is rather doubtful that the "really basic" level will ever be found.

In concluding these introductory comments it might be said that the question is not whether there are biochemical causes for schizophrenia or any other aberrant behavior. Of course there are. The antecedent conditions of all human behavior are logically capable of a biochemical conceptualization just as they are logically capable of a psychological or sociological conceptualization. The real question is that of utility of conceptualization, not reality.

At this point we see no reason to invoke a different conceptual model for extreme as opposed to more normal behavior. Just as the expression and maintenance of attitudes can be dealt with at a psychological level, so too can delusions, withdrawal, or inappropriate giggling. Few expect the biochemical model to handle attitudes, and there appears as yet only negligible evidence to demonstrate that its focus of convenience lies in the realm of psychotic behavior. The fact that behavioral changes can be induced by drugs hardly proves the case for biochemistry since such changes can also be induced psychologically. Furthermore, it is becoming increasingly apparent that drug effects are complex ones that depend a great deal on such psychological variables as expectations, previous mode of adjustment, and physician's hopes (Pollard and Bakker, 1960). Currently it is undoubtedly true that which model one invokes for explanation is largely a function of the kinds of questions which need to be answered.

In general, the evidence regarding biochemical hypotheses is derived from two classes of studies: (1) studies in which the biochemistry of normals is compared with that of some pathological group, and (2) drug studies wherein the differential effect of a drug is observed with respect to normals and pathological groups. As exemplifying the general biochemical approach we shall again select schizophrenia, since it has served as the major arena for research in this field.

Over the past few years enthusiasm has waxed and waned over many biochemical leads. Investigators have successively studied a number of biochemical mechanisms, including a decrease in basal metabolism, anti-insulin mechanisms, altered phosphorous metabolism, and disordered amino acid metabolism. In each case, after an initial surge of enthusiasm and research activity the negative reports and failures in replication have discredited the initial high hopes. Similarly, in the case of taraxein, Kety (1959) argues that instead of being crucial to the development of schizophrenia it is, instead, a secondary phenomenon that can be manipulated in both normals and schizophrenics merely by altering dietary variables. Likewise, reports that schizophrenia was a disorder of serotonin metabolism have given way to a realization that its role in the nervous system remains unknown (Jackson, 1962).

At this point it might be appropriate to quote Horwitt (1956) who states:

> Year after year, papers appear which purport to distinguish between the state of schizophrenia and that of normalcy. The sum total of the differences reported would make the schizophrenic patient a sorry specimen indeed: his liver, brain, kidney, and circulatory functions are impaired; he is deficient in practically every vitamin; his hormones are out of balance, and his enzymes are askew. Fortunately, many of these claims are forgotten in time with a minimum of polemic, but it seems that each new generation of biologists has to be indoctrinated—without the benefit of the experience of its predecessors (Horwitt, 1956, p. 430).

We could, however, undoubtedly make the same kind of comments regarding the psychological bases for schizophrenia that have been offered over the years.

The reason for this state of affairs, wherein enthusiastic reports seem to be inevitably followed by negative ones, seems intimately tied up with several sources of methodological and conceptual error. Some of these involving the disease entity concept of schizophrenia and the difficulty in defining schizophrenia have already been discussed. These and additional points have been discussed by Kety (1959) as follows:

1. Looseness of the concept of schizophrenia which leads to extremely heterogeneous populations. The probability of replicating results is thus extremely remote.

2. Many biochemical studies are carried out with patients who have been hospitalized for many years under crowded conditions. Thus, what initially is thought to be a biochemical basis for schizophrenia turns out to be a function of the spread of chronic infections of the digestive tract.

3. Diets of hospitalized schizophrenics are not comparable to those of normals. This always raises the possibility that any observed biochemical differences are a function of dietary differences between groups.

4. Long history of hospitalization in the case of schizophrenia often means a long history of electroshock, insulin coma, and hypnotic and ataractic drugs. This means we cannot preclude the operation of residual effects from these interventions in any observed differences between groups.

Of course, the critical question in this whole biochemical approach is that of cause and effect. Forgetting for the moment all the methodological problems, reliability of findings, and replicability, it is not possible to state whether observed biochemical phenomena are causes of the schizophrenic process or follow from it. For example, does an altered phosphorous metabolism cause schizophrenia or does it develop as a result of schizophrenic behaviors? This is a nagging question that has as yet been inadequately disposed of.

A parallel line of research has been pursued by investigators working with psychotomimetic drugs. Thus, investigations have been reported in which psychotic-like behavioral effects could be produced by lysergic acid diethylamide (LSD-25). However, it is quite a jump in logic, as Rubin (1957) points out, to consider toxic psychoses and endogenous psychoses as equivalent states. To say that a toxic state is the essence of psychosis is no more plausible than to say that because a blow on the head produces a sleep-like state, such a trauma is the essence of sleep. The effect of the various drugs, both psychotomimetic and ataractic, is so variable and dependent upon so many factors, including the personality of the subject and his expectations for the drug action, that unequivocal statements are difficult to make at this time. In effect, psychopharmacology is beset with problems much like those we have discussed in other areas.

PSYCHOLOGICAL THEORY

The basic goal for psychology has long been the prediction of response from stimulus. To aid in this quest the psychologist has cast about for concepts or, in some cases, physical structures to help him predict from stimulus to response. What sets apart the psychopathologist from this scheme of things has been his concentration on a particular class of responses. In other words, he has been interested only in certain kinds of responses or behavior. Often there has been insistence that these behaviors are qualitatively different from usual behavior and therefore they require a special handling or set of concepts by which to be explained. In many instances this insistence and the tacit acceptance of it by many others has served to remove the field of psychopathology from the mainstream of personality theory and general psychological theory as well.

We have already seen that one of the problems on which psychopathology has foundered has been its inability to specify the behaviors over which it presumably has exclusive domain. Particularly as long as we assume that abnormal behavior is abnormal because it contradicts some set of values, it would appear untenable to argue that a special system of principles is necessary to encompass psychopathology. What is needed is the systematic development of existing concepts (be they psychoanalytic ego concepts, social learning, or self-theory) so as to encompass the behaviors traditionally dealt with by the psychopathologists. This should have a salutary effect on both general psychological theory and psychopathology. What we are arguing for is a more general *rapprochement* between psychopathology and general psychological theory.

At the psychological level of abstraction the concept of anxiety has played a central role in the explanation of psychopathological disorders. This is true both historically and currently. At the same time, the concept has generated a great deal of confusion. For, although most clinicians are certain that they cannot get along clinically without such a concept, they are not nearly so certain as to the nature of the concept. Indeed, the role of anxiety has been defined in the physiological as well as in the psychological sense. It is widely regarded as a response, but then it has also been considered to be a stimulus. In other cases it is defined in terms of an intervening variable. At any rate, it has assumed a place of great motivational power along with such primary motivators as hunger, thirst, and sex.

In the following pages we shall consider first psychoanalytic theory and then learning theory as regards their handling of deviant phenomena. It would appear that these two approaches constitute the dominant orientations of psychopathologists. Although some might argue that psychoanalytic theory has outlived its usefulness and that learning theory is on the ascendance, the fact remains that analytic theory has played and continues to play a highly seminal role in the thinking of many clinicians.

No attempt will be made to "explain" every psychiatric syndrome from each theoretical vantage point. Instead, we shall attempt to illustrate the several theoretical views by applying them to selected examples. In so doing it should be borne in mind that when using traditional psychiatric terminology we are using it in a purely descriptive fashion. That is, there should be no implication that class membership is being used in any explanatory way or, indeed, that reliable behavioral classes have as yet been specified.

PSYCHOANALYTIC THEORY

Although the concept of anxiety is an old one, the systematic utilization of it and the description of the diverse conditions under which it arises must be attributed to Freud. Indeed, broadly speaking, current usage of the concept is perhaps little different from Freud's.

Psychoanalysis was not developed as a point for point dynamic explanation for the static psychiatric categories. However, psychoanalysis has had an obvious and profound effect on psychiatry, and psychoanalytic explanations have been offered for all the traditional neuroses, psychoses, and character disorders. In fact, Fenichel's (1945) book has become a classic exposition of the psychoanalytic position as regards the neuroses and psychoses.

The essence of neurosis resides in conflict—a conflict between the instinctual forces of the id and ego forces striving to maintain the integrity of the organism. Of course, everyone experiences conflict of this sort. Pathology presumably occurs when the degree of conflict becomes excessive, and the usual techniques the organism has developed to handle the conflict become inadequate. Again, utilization of a pathology model presents the usual problems in defining precisely or specifying exactly the neurotic behaviors to be considered. There are no logically specifiable cutoff points apart from a value judgment.

Anxiety plays a central role in the development of neurotic conflicts. Neurotic behaviors are regarded as symptoms, and as symptoms they are motivated by a need to avoid the unpleasant sensations that constitute anxiety. Anxiety becomes a signal for the initiation of behaviors designed to reduce the anxiety and thus bring about a more pleasant state of affairs. Presumably the danger signaled by anxiety refers to a threat from within in the sense that an instinctual force is about to break into consciousness. The presence of the superego complicates the situation inasmuch as it often determines which instinctual discharges will be permitted by the ego. However, in some cases it happens that the ego wards off impulses from the superego (guilt feelings) in much the same manner that it handles id impulses.

Of central importance in understanding psychoanalytic notions of neurosis are the defense mechanisms. The genius of Freud was particularly apparent in his exposition of these behaviors and the sensitivity with which he could identify their behavioral consequences. Consistent with Freud's contention of the central role of childhood in personality development, it was his view that pathological defenses as well as neurotic conflict generally were rooted in the past. Indeed, Anna Freud (1946) believes that there is probably a correlation between defense mechanism and the stage in childhood wherein it became necessary to invoke defensive measures. The understanding of defense mechanisms has become a central task of psychoanalysis, since it can readily be seen that through these mechanisms we are getting at the dynamics of the functioning personality. In fact, the current emphasis on ego-psychology can be traced to this concern.

There is no absolute or "official" list of defense mechanisms. The ones that we shall briefly sketch are usually described in most psychoanalytic expositions, although there is no reason why additional ones could not be abstracted or that those below could not be further subdivided. For example, some recent work based on social learning theory (Phares, 1962; Rotter, Seeman, and Liverant, 1961) is reminiscent of a psychoanalytic-like mechanism wherein certain individuals may overgeneralize and believe that obtained reinforcements are either exclusively a function of skill factors on their part, or, on the other hand, are an exclusive function of chance factors. In many cases these beliefs would appear to represent ways of defending the self against anxiety. The importance of the mechanisms for pathology lies in the central role they play in the symptomatology of the neuroses and psychoses.

Repression refers to the unconscious, purposeful banishment (or not becoming aware in the first place) of instinctual impulses or ideation as well as the external referents for such material. The function of such a mechanism is the avoidance of the painful affect associated with the material, and its accomplishment is mediated through the ego in the service of its basic goal—the avoidance of pain and the striving for gratification. Also, repression is an active ingredient in all other mechanisms.

Projection begins as an infantile method for avoiding pain by attributing noxious stimulation to the external world, and its ultimate manifestation is represented by the tendency to attribute unacceptable instinctual impulses to outside agents. Thus, "I do not hate you, it is you that hates me." Naturally, as in the case of all mechanisms, it operates unconsciously.

Reaction formation is exemplified by the conscious entertainment of an idea or impulse in exaggerated form that is quite the opposite of the actual unconscious urge or wish.

Undoing represents a kind of magical mechanism whereby one behavior or act nullifies another. It is almost as if the second act cancels out the first and, indeed, as if neither had taken place. This kind of behavior is often seen quite clearly in children where the magical qualities are evident.

Isolation describes situations where anxiety-provoking material is not forgotten, but instead, the associated affect is stripped from the memory, and associative elaboration is prevented. Thus, what would ordinarily be an extremely traumatic thought to entertain becomes the object of prolonged rumination, but the customarily associated emotions are missing.

Displacement is evident when the original unconscious impulse is represented in consciousness by an associated substitute. For example, a preoccupation with gadgets might, in some cases, represent a displacement of genital urges.

Denial occurs when reality becomes particularly unpleasant and the individual denies that reality in a kind of wish-fulfillment fashion. Generally speaking, this mechanism is given up as maturation of the ego proceeds so that systematic denial in adults is essentially a psychotic reaction.

Rationalization is a prime human activity and refers to the finding of socially acceptable bases for behavior really generated by other factors. In other words, we avoid the anxiety that would arise from recognition of our inadequacies or failures by finding "reasonable" excuses for ourselves.

Of course, dealing with psychopathology from a psychoanalytic perspective involves several systems. The meaning of a neurotic symptom becomes clear only when we consider not just the defense mechanism involved, but also the developmental elements represented by the psychosexual stages and the structural components of id, ego, and superego.

Perhaps the clearest examples of the psychoanalytic explanation of neurotic behavior come from the so-called *hysterias*. Although the symptoms of hysteria are legion, ranging from paralysis to blindness, they have a common root. The conversion symptom represents an impulse (sexual) that has been repressed. Typically, the symptom is associated with a forgotten memory. Thus, the choice of symptom is determined by association, and the mechanism involved is principally that of repression. The development of hysterical symptoms is intimately tied up with the phallic stage of psychosexual development. The hysterical behavior may be considered a compromise between the sexual wish and guilt feelings. This also in part explains why many hysterical patients are relatively unconcerned about their disability. Although it is unpleasant, it is not nearly so unpleasant as recognition of the repressed impulse would be. Control of anxiety thus becomes the essential purpose of the hysterical symptom.

Compulsions and *obsessions* are illustrated by complicated rituals and persistent, unwelcome thoughts, respectively, that create marked anxiety if interfered with. A patient may report horribly aggressive thoughts but the thoughts are devoid of any of the expected affect. This illustrates the isolation mechanism noted earlier. Thus, the thought is not repressed as in the case of hysteria—it is simply isolated from its affect. Likewise, in compulsions, the element of undoing is apparent wherein one act nullifies a preceding one. Again, the role of anxiety prevention or reduction is of prime importance. For example, a patient who obsessively ruminates about murdering his child protects himself against committing the act. By isolating the affect the chances of a motor expression are almost nil. In other cases, the guilt that would be experienced if an impulse were consciously entertained is checked by the onset of an obsessive train of thought or the initiation of compulsive behaviors.

Phobias likewise illustrate the role of anxiety in neurotic behavior. A patient may displace unconsciously his impulses from one object to another. Thus, a fear of admitting erotic impulses is displaced by a fear of open places. To avoid open places is to reduce the likelihood of encountering a situation that would lead to intolerable anxiety. Or, highly aggressive impulses are kept in check by a displacement to pointed objects which arouse the anxiety. Then, anxiety is reduced whenever such objects are avoided.

In summary, Freud's position on the psychoneuroses was that such symptoms are motivated out of a desire to avoid intolerable states of anxiety. Neurotic anxiety is a signal that danger from within is imminent—a reinstatement of an infantile state of affairs. Each neurotic solves the problems of his instinctual demands in his own way, but all ways can be traced to the common element of anxiety reduction.

For neo-Freudians the role of anxiety is also central but more frequently treated in the context of cultural pressures. For Sullivan (1950) the need for approval is essential for growth and its presence is experienced as a state of euphoria. Loss of euphoria is brought about by disapproval and is experienced as anxiety. Fromm (1941) sees anxiety growing out of the conflict between the need for closeness and approval from others and the need for independence. In the case of Horney (1945) basic anxiety is equated with the child's feeling of being helpless and isolated in a potentially threatening world. This view stemmed from Adler's (Ansbacher and Ansbacher, 1956) conception of anxiety as a fear of being defeated or proved worthless.

A more comprehensive evaluation of psychoanalytic theory is presented in Chap. 12, pp.

469-475. However, it is perhaps fair to say that nowhere is psychoanalytic theory so adequate as in its treatment of the neuroses which are composed essentially of focal symptoms. Patients manifesting a simple hysterical symptom or clear-cut obsession or compulsion seem very often to lend themselves readily to a psychoanalytic interpretation. The question arises, however, whether these kinds of cases occur very frequently. Seemingly they do not. Most patients do not conform to the traditional psychoanalytic description of symptomatology. The patients now do not seem to be so very often "merely" afraid of sharp objects or open spaces. Rather, they just do not get along with their spouses, or they are chronically failure avoidant, or perhaps they have difficulty making friends. In short, the prevalence of the so-called character disorders seems on the rise. Thus, the problem of libidinal development has given way to a recognition of the crucial importance of ego development—or learning. Munroe (1955) has commented in a similar vein.

For an explanation of psychotic reactions the psychonanalyst typically turns to regression, constitutional factors, or both. In the psychotic individual there is presumably an extensive regression to pre-Oedipal levels—levels at which the ego was not fully developed. For example, in the case of depressive states oral factors assume extensive importance. Psychotic depressions are thought to occur in persons who did not successfully negotiate the oral stage—they remained fixated at that level. Their relationships with others remain dependent and their self-esteem is maintained only by receiving repeated gratifications from without. Thus, a depressed state represents the predicament into which these individuals get themselves when their sources of gratification are cut off. Such predicaments could stem from failure experiences, loss of money, death of a loved one, etc. Even success can be a stimulus for depression if it serves as a cue for the likelihood that future demands will be made on the person. Such a situation is reminiscent of a recent finding by Aronson and Carlsmith (1962). Working within a cognitive dissonance framework they found that subjects who expected to perform poorly and then performed well attempted to reduce the ensuing dissonance by altering their previous success trials in the direction of failure. *Post hoc*, such a result is "predictable" from the analytic position noted above.

Also associated with psychotic depressions is the strong element of hostility. Hostility toward the persons or objects upon which the individual is dependent becomes redirected toward the individual himself. Much self-deprecation ensues and the melancholia deepens. Neurotic depression presumably follows similar ground rules, but since the neurotic individual has an ego that is more fully developed, personality disorganization and loss of contact with reality do not occur.

Cohen, Baker, Cohen, Fromm-Reichmann, and Weigert (1954) have described the manic-depressive character as one who experienced parental indulgence early in life. This was later replaced, however, by approval which was forthcoming only if the child rigidly conformed to the parents' demands and also exhibited achieving behaviors. Such demands are rigidly enforced and, as an adult, the manic-depressive is torn between high need for achievement (the need to satisfy the demands of others) and fear of alienating others by successes. Such a conceptualization from a psychoanalytic framework invites the investigation of conformity in manic-depressives from the standpoint of the achievement concepts of McClelland and his associates (McClelland, Atkinson, Clark, and Lowell, 1953; Atkinson, 1958). Such an investigation was carried out by Becker (1960) who found that manic-depressives performed like "high value Achievers" as regards achievement striving, attitude toward authority, and conventional beliefs. In both the manic-depressives and "high value Achievers," achievement-oriented behavior as well as conformity behavior seemed to serve the purpose of gaining approval from others.

Another major functional psychosis is that of schizophrenia. Although textbook discussions of the dynamics or etiology of this disorder often are quite explicit, it is difficult to envisage the postulation of a uniform set of dynamics for a category which contains such a heterogeneous population. The variability which such a unitary explanation must subsume is tremendous. However, from an analytic viewpoint, schizophrenic behavior is characterized most frequently as an extremely regressive disorder wherein reality testing is minimal and where somatic predisposition is quite likely. Such a characterization is applied to a wide variety of presenting symptoms including world-destruction fantasies, feelings of depersonalization, feelings of grandeur, hebephrenic reactions, catatonia, hallucinations, and delusions (Fenichel, 1945). All the diverse manifestations of schizophrenia, however, are considered to be elements of the regressive process. This latter process is exceedingly profound and the individual returns to a level wherein reality and self are undifferentiated. Basically, the id is in conflict with reality. The psychotic ego breaks with reality and thereby hopes to avoid the temptations which reside there. The basic difference between neurosis and schizophrenia from the analytic point of view (Fenichel, 1945) would seem to be in the manner in which the person defends himself against instinctual demands. The schizophrenic gives up reality, the neurotic does not. Such a manner of defense seems to depend upon the fact that fixation occurred before reality was adequately conceptualized. Why such fixation occurs is thought by some to represent constitutional factors, while others suggest that traumatic experiences at an early infantile level are responsible.

LEARNING THEORY

A basic proposition of many learning theorists is that disturbed behavior is learned and maintained by anxiety reduction. This notion is not, on the surface at least, much of a departure from classical Freudian theory. Learning theory does, however, contain concepts that relate to the acquisition of behaviors. Thus, although the ego supposedly preserves the organism through memory, perception, defense, and adaptation, these functions cannot be accounted for adequately by analytic theory. Rapaport (1959) has commented on this psychoanalytic "embarrassment." Many, if not most, Freudian propositions are far from operational, and both Mowrer (1950) and Dollard and Miller (1950) have attempted what is essentially a translation of psychoanalytic concepts into the operationism and language of learning theory. The central role of anxiety remains unaltered.

As an exemplification of this general position we shall consider several neurotic behaviors which will serve to illustrate Dollard and Miller's (1950) central factors of drive, cue, response, and reward. In the case of phobias strong fear is learned as a response to a set of cues. Although the origin of the fear is not often immediately apparent, it usually centers around sex and aggression. Once the fear is learned, it generalizes from the cues of the specific situation to those of similar ones. Thus, avoidance of the specific situation along with avoidance of similar ones is reinforced by anxiety reduction. The process of generalization is embodied in the notions of primary stimulus generalization, higher-order conditioning, and secondary generalization. The social conditions under which fear is learned are highly similar to Freud's concepts of oral, anal, and phallic psychosexual stages and the conflict they engender between parents and child.

Dollard and Miller also relate compulsions directly to anxiety reduction. A compulsion is reinforced by anxiety reduction and is learned on a trial and error basis, but often the responses are unwittingly reinforced by parents during childhood. For example, a rather overmeticulous mother may overemphasize cleanliness and thus provide a readily accessible set of responses for the eventual compulsive hand washer.

Hysterical symptoms again provide learned ways of reducing anxiety. Fear of sexual intercourse, for example, may be reduced by the development of a hysterical symptom which prevents intercourse. The reason why a specific symptom develops rather than another can only be determined by an analysis of the life circumstances of each individual patient. However, the role and function of such symptoms are clearcut.

At perhaps a less operational level but from a nevertheless similar position, White (1956) relates the development of neurotic behaviors to blockage or distortion of the learning process. White adopts a developmental position in that neurosis does not necessarily arise from one traumatic event. Rather, the individual fails to learn adjustive techniques in one situation because of anxiety. This failure, in turn, breeds new failures and defensive measures so that the individual never learns new or alternative modes of behavior. Such a view is similar to Mowrer's treatment of Freud's dilemma, the neurotic paradox (1948). To explain the repetitive and self-destructive behavior of the neurotic Freud (1920) postulated *thanatos* or the death instinct. Mowrer, however, asserted that the individual's expectancy for punishment remains high because he fails to learn that what he fears is no longer realistic, since he avoids the very situations in which re-learning is possible. Thus, anxiety-reducing defensive measures or behaviors persist.

In sum, White regards symptom syndromes as being determined by learned and strongly reinforced adjustive techniques whose function is essentially the control of anxiety. The particular nature and quality of the symptoms may be considered a function of experiences at varying psychosexual stages of development. Also, the possibility of contributing constitutional factors should not be overlooked. Thus, the basic similarity to the Freudian position is clear.

Wolpe (1962) has, in recent years, evolved a therapeutic technique which rests on the assumption that neurotic behavior is essentially based on persistent habits of learned or conditioned unadaptive behavior acquired in situations which generate anxiety. From such a position the deduced therapy is the counterconditioning or unlearning of the neurotic behavior. Thus, Wolpe also assigns a central role to anxiety in the creation of neurosis.

Generally speaking, most learning theorists depend on several basic mechanisms to account for behavioral modification. These mechanisms, discussed by Bandura (1961), include conditioning, extinction, generalization, reward techniques, punishment, and social imitation. Additional views on the role of learning in the development of maladaptive behaviors will be discussed subsequently.

Although the comparative virtues and deficiencies of several personality theories are discussed more fully in Chap. 12, a brief discussion may be useful here. As applied to psychopathology, learning theory provides a more operational approach to clinical events than the older so-called dynamic approaches. Because of the many values an operational approach contains, it can serve as a useful representational model for clinical phenomena. Thus, the conceptual language of learning theory may ultimately lead to more fruitful ways of construing such phenomena. Bandura's (1961) discussion of psychotherapy as a learning process illustrates the point that casting psychopathological data in a learning theory mold can lead to therapeutic techniques which are quite different from the traditional highly verbal interaction between patient and therapist. In addition, learning theory is by definition concerned with the individual's acquisition of behaviors. Such a concern leads inevitably to a closer scrutiny of situational conditions which can either aid or inhibit such acquisition. This tends to de-emphasize the exclusive role of internal factors as the sole variables in mediating pathology. We tend to more carefully study environmental conditions rather than just instinctual urges. Finally, as suggested in previous sections, deviant behavior becomes not the consequence of morbid disease processes but simply represents learned ways of achieving gratification or minimizing pain. The far-reaching consequences of such a view should not be underestimated!

On the other hand, learning theory models often generate problems when applied to the field of psychopathology. Certainly learning theory as now constituted provides no universal explanation for clinical phenomena. It suffers from several defects which may be remedied if a more systematic *rapprochement* between learning theory and psychopathology occurs.

In the interests of parsimony and operationism much of learning theory has been justifiably criticized by practicing clinicians as being a gross oversimplification. It can be argued that much of learning theory boils down to a rather barren brew of conditioning, extinction, and generalization. Although these processes are potent ones that provide a firm skeleton, this skeleton is not yet filled out. That is, learning theory typically offers little in the way of content. Other theories possess much content (needs, traits, etc.) but offer little in the way of process by which such content is acquired, changed, or eliminated. An associated difficulty is the mechanistic and, frequently, totally anti-phenomenological stance taken by learning theory. Many Hullian-like constructions seem far-removed from the personal conceptualizations of experience. The point is not that such constructions are wrong—only that they are perhaps unnecessarily circumscribed. Currently, the inclusion of an expectancy construct in learning theory seems to be occurring with increasing frequency and may help wed the operational virtues of learning theory with the more experiential level of expectancy theory (Rotter, 1954).

Frequently, too, it seems that the focus of convenience of many learning theories lies primarily with focal symptoms. Thus, phobias, compulsions, or perhaps the drinking response on the part of an alcoholic can readily be fitted into the conditioning model. Much of the reported success of counterconditioning procedures such as Wolpe's (1962) may very well be an example of this observation. That such theory or techniques can be as successfully applied to the so-called character neuroses or to patients in which there is little focalization of anxiety remains to be convincingly demonstrated.

Many theories which rely heavily on the concept of anxiety seem to assume that most of the patient's difficulties will be automatically resolved once anxiety is abolished and its inhibitory effects thereby dissolved. Such a view seems to assume that the "correct" responses are in the patient's repertory and await only the removal of anxiety in order to appear. This view is most clearly apparent (but not exclusively so) in psychoanalytic techniques of therapy, where almost sole emphasis is placed on "analyzing" the patient in the hope that such analysis will lead to insight and thus improvement. Both Rotter (1954) and Bandura (1961) have commented on the inadequacies of such a view. Many individuals such as delinquents, psychopaths, etc., seem to exhibit failures in learning rather than just anxiety-generated reactions. Furthermore, this point need not be reserved solely for such patients. Many so-called neurotic, anxiety-ridden patients also display glaring deficiencies in learning. They have failed to learn many of the subtleties of interpersonal relations that most take for granted. How, then, can a reduction in anxiety result in the expression of a behavior which has never been learned in the first place? That such anxiety reduction can pave the way for new learning is a truism, although most therapies seemingly do not avail themselves greatly of this truism.

Most anxiety theories of psychopathology are energy models. Such models seem to conceive of pathology as the product of anxiety pressures. As the anxiety mounts, so too does the pressure until some neurotic or psychotic (depending on the level of anxiety) behavior occurs. The analogy is that of an exploding boiler. Such views too often focus on the internal pressure build-up and neglect the external cues which serve to generate certain expectancies for learned behavior-reward sequences. Thus, it is not the sheer amount of anxiety that is important in pathological reactions. Rather, it is the inadequacy in handling such anxiety which is often crucial. Whether a person becomes neurotic or psychotic depends not just on drive level but on learning history in the sense of developing ways to cope with anxiety. As an illustration, studies by Phares (1961) and Goldstein (1959) taken together, suggest that subjects, even though matched as regards anxiety, may display behaviors which differentially fit them to cope with environmental threats. Thus, even though people may not differ as regards chronic drive level, their learned ways of handling such anxiety may well determine how adequately they function. Many human beings are very anxious, have suffered extreme trauma, etc. However, not all these people ultimately are labeled neurotic or psychotic. Should we investigate to determine differences in kind of conflict, or should we examine their pasts for evidence of differences in conditions under which certain learnings failed to occur? The presence or absence or even degree of specific conflicts are perhaps not so important as are the presence or absence of the conditions which facilitate the learning of adjustive, coping behavior. This point will be further elaborated in our discussion of psychotic reactions.

In discussing psychosis and, specifically, that heterogeneous cluster of syndromes labeled schizophrenia, White (1956) presents a cogent argument for regarding it as a developmental disorder. His major thesis is that a defensive withdrawal is responsible for the ultimate development of a schizophrenic adjustment. Experiencing pain or threat of danger when in the company of others, the child learns to avoid interpersonal situations as much as possible. The particularly malignant aspect of this state of affairs is that by avoiding others the child never learns the multitude of things that are necessary for adequate social functioning; things that can only come from social participation. Naturally, withdrawal breeds more withdrawal and the child is thus denied the opportunity to develop social competencies. Other children are progressively developing more and more behavioral pathways through social participation, while the preschizophrenic child is being forced more and more to rely on one behavior—withdrawal. Certainly, this is poor training in the development of coping behaviors which can later serve to handle anxiety-arousing situations. Evidence is accumulating that schizophrenics are especially disrupted by the social features of stimuli (Johannsen, 1961).

Furthermore, as Cameron (1943) states, lack of social participation denies the child the chance to critically evaluate himself and his thought processes. A capacity for autocriticism fails to develop and the seeds of schizophrenic or paranoid thought are planted. Thus again, the failure of this developmental capacity excludes more objective and realistic coping behaviors from occurrence when subsequently he may need them most. That is, as noted in an earlier section, when an anxiety situation arises, the individual cannot solve it since he has never learned rational modes of thought. Another more fortunate individual may become just as anxious but because of a history of social participation and the capacity for more realistic and flexible thought, he is able to solve his problem more effectively.

From a basically Hullian position, Mednick (1958) has proposed that the peculiarities of schizophrenic thought be analyzed from essentially an anxiety-drive reduction standpoint. For Mednick, the acute schizophrenic is extremely anxious or in a heightened drive state. Such a condition raises the generalization gradient, which involves the rendering of more and more stimuli capable of arousing anxiety. This, in turn, generates more anxiety. The net effect is a disruption in thinking through the constant intrusion of irrelevant thoughts. The end product of all this is the patient's tendency to fixate on remote associations which are reinforced by a reduction in anxiety.

The difficulty with this view appears to be its exclusive emphasis on drive level. The implication is that everyone who becomes psychotic does so because of drive level. This position seems to ignore the point that people learn different modes of reacting to anxiety. The preschizophrenic individual may suffer a severe jolt to his self-esteem. In defending himself against the consequent anxiety he casts about for an explanation and will come up with an explanation based on his experience. The explanation or adjustive technique will likely be somewhat bizarre. Such a reaction was predictable not so much as a sole function of drive level, but from his history of social isolation and failure in consensual validation mechanisms. A preneurotic individual may also become very anxious over loss of self-esteem, but his learning history may predispose him to a repressive mechanism or to an avoidance, obsessive-like technique. Thus, schizophrenic thought may follow in the face of a heightened drive state as the enhancement of already existing defensive behaviors rather than being a simple product of anxiety level. Or, put another way, one cannot predict the kind of adjustment behavior solely on the basis of drive level; prior learning history must also be considered. A behavior occurs not only as a function of drive level but also as a function of the expectancy that such a behavior will reduce the anxiety. And such expectancies are developed out of previous failures and successes in dealing with threatening stimuli.

In discussing the immediate causes of schizophrenic breakdowns one is confronted by two seemingly different situations. In the one case the development of schizophrenic behaviors is a slow, progressive, and insidious process. There may be no sharp changes in the patient's behavior which led to institutionalization. Instead, the patient's behavior finally reaches a level where something has to be done, or perhaps he finally comes to the attention of someone in authority. In other cases, however, the patient has made a "reasonably" good adjustment until quite suddenly he begins behaving in a bizarre fashion. In many cases the onset can be definitively traced to a specific circumstance such as the onset of puberty, a bad failure, or perhaps a homosexual experience.

The foregoing very closely parallels a currently developing diagnostic dichotomy. The extreme heterogeneity of schizophrenic patients so categorized under the traditional nomenclature and the notable failure of traditionally based psychiatric diagnoses to predict much of value regarding patient behavior have led to a process-reactive classification in schizophrenia (Herron, 1962). Although the defining characteristics of the dichotomy are somewhat variable depending upon the investigator, Zigler and Phillips (1962) have outlined the following common factors. The process schizophrenic tends to display poor premorbid social adequacy, an insidious onset, no clear precipitating event, and poor prognosis. The reactive schizophrenic more often shows a relatively good premorbid level of adjustment, a sudden onset, a specific precipitating event, and a more favorable prognosis. For some investigators there is also the implication that the process cases represent an organic process, while for reactives the basis is functional. Furthermore, the generally accepted view seems to be that presenting psychiatric symptoms are the same for both classes.

The particular merit of this approach to classification is that it breaks with the traditional approach of basing diagnosis largely on presenting symptoms. Instead, diagnosis is based on premorbid social competency and, implicitly at least, on adequacy of previously learned adjustive techniques. Such a view is partially supportive of the notion developed earlier that at best drive level must be supplemented by abstractions based on the patient's prior learning history in explaining the development of psychotic behaviors.

Zigler and Phillips (1962) have also presented evidence which suggests that schizophrenic patients who have relatively good premorbid levels of social competence tend to show symptoms of a "turning against the self" nature, while poor premorbid patients are characterized by behaviors described as "avoidance of others, self-indulgence, or turning against others." Furthermore, they find the same thing in a nonschizophrenic but still pathological population. The authors, in commenting on these findings, state:

> The implication here is that the process-reactive distinction is reducible to the social competence dimension, which is continuous in nature, and which is applicable not only to schizophrenia but to all of psychopathology. This approach makes of psychopathology a unitary phenomenon rather than a collection of discrete entities, each conceptualized in terms of unique dimensions and parameters. In such a schema the various disorders are viewed as inappropriate solutions to the problems of living at various levels of development (Zigler and Phillips, 1962, p. 220).

Such a view is entirely supportive of the general thesis which we have been trying to develop throughout this chapter. In short, rather than pursuing research based on the disease entity philosophy we need research designed to elucidate the conditions under which people fail to learn effective ways of coping with problems or else learn inappropriate or avoidant ways. Although the specific mechanisms may still be rather obscure, a number of empirical investigations have disclosed several of these conditions.

Farina (1960) found that paternal dominance is associated with good premorbid adjustment in male schizophrenics. In poor premorbid adjustment cases, however, the mothers were dominant. This finding is also consistent with the data of Garmezy, Clarke, and Stockner (1961). Rodnick and Garmezy (1957) have suggested a relationship between family background differences and differential responsiveness to censure or rebuff. This view is now supported in a study by Baxter and Becker (1962), who found that poor premorbid schizophrenics produced more anxiety-related imagery in response to mother stimuli than to father stimuli. A group of good premorbid patients showed the reverse effect. Furthermore, the poor premorbids displayed more avoidant responses to both figures than did the good premorbids.

In summarizing some of the child-rearing environmental antecedents of schizophrenia Harvey, Hunt, and Schroder (1961) comment:

> Generally, the training environment described involves (1) the parents' compulsive requirement that the child behave in a particular fashion, (2) the parents' restriction of alternative means of behaving or thinking, (3) unilateral relationship between parent and child, which is lacking in mutuality, (4) restrictive and controlling practices, and (5) the "double bind" notion or the simultaneous demand and prevention of a course of action, for example, "always love your mother even when she punishes you for doing something you didn't do" or "don't speak to strangers but be friendly and polite to everyone" (Harvey, Hunt, and Schroder, 1961, pp. 288-289).

Another form of schizophrenia has been variously described as childhood schizophrenia and infantile autism (Kanner, 1944). Although relatively rare, it occurs in children as young as two years of age and is manifested by poor speech, tantrums, inability to tolerate changes in routine, etc. Although variously and vaguely ascribed to everything from cold, aloof mothers to constitutional factors, Ferster (1961) has recently attempted to explicate this syndrome from a Skinnerian point of view. Ferster argues that the social behaviors of the child are only intermittently reinforced by parents. Since the effects of behavior on the parents are weak, the social behaviors themselves are likewise weakened. In contrast, the physical environment reinforces continuously and reliably. This explains why such children are so preoccupied with their physical environment and often show great skill in manipulating it but yet are so isolated and withdrawn in the social, communicative sphere. Factors which affect the parents' frequency of reinforcement of the child's performance include such things as (1) general disruption of parental repertoires (for example, a parental depression), (2) prepotency of other performances (such as overinvolvement with a career, clubs, housework, etc.), and (3) categorization of the child by the parents as interfering with more rewarding activities. Such conditions, in sum, lead to a reduction in the frequency of parental reinforcement and thus lowers the rate of the child's emission of social, communicative behaviors.

CONCLUDING DISCUSSION

In this concluding section we shall try to pull together many of the points already discussed and to suggest a tentative outline or orientation that seems promising as a future course for the field of psychopathology. Our basic position is perhaps best stated in capsule form by a quotation from Bandura (1962):

> According to the social learning theory, so-called symptomatic behavior is viewed not as emotional disease manifestations but as learned reactions which can be modified directly by the provision of appropriate models, and by the manipulation of response-reinforcement contingencies. Once the maladaptive behavior is altered, it is unnecessary to modify or remove an underlying pathology (Bandura, 1962, p. 298).

This position rejects the disease entity concept or allied notions of mental illness in favor of a view which considers symptoms as simply learned solutions to problems of living. We are dealing with behaviors which are subject to the same laws of acquisition, modification, and extinction as any others.

Along with this we would also abandon the categorical approach which has for so long dominated the field of psychiatry. The de-emphasis of the abnormal-normal dichotomy is long overdue. Inferences from a reliable classificatory scheme, or any other base, need not be made regarding abnormality. Rather, it should be recognized that decisions about treatment, institutionalization, or other interventions are made on the basis of cultural values. Such a recognition should facilitate the scrapping of the traditional mental illness model and hasten the demise of the medical model as applied to so-called psychopathological phenomena. That characterization of an individual as abnormal carries no predictive freight should be obvious at this juncture.

Implicit in the foregoing is also the rejection of the customary dichotomy of neurosis and psychosis. In general, these terms are differentially applied on the basis of severity of symptoms, or more precisely, degree of deviation from cultural norms. Other criteria typically include the number of areas of the personality affected and the extent of social and personal danger (O'Kelly and Muckler, 1955). In practice, however, differentiations are difficult to make and show little predictive potential. Although extreme reactions are easily classifiable as psychotic, the borderline areas are large and ill-charted with classification principles. Furthermore, most clinicians would be extremely wary of making any behavioral predictions about a person, just knowing he was either psychotic or neurotic. Thus, the dichotomy is about as functional as that of tall versus short. There may be some low-level, descriptive merit to the dichotomy, but that is about all. The real danger is that once we begin using the terms, they very readily become reified and soon take on an existence much larger than life.

From the preceding it follows that there is no *necessity* for invoking explanations at different levels of abstraction for neurotic and psychotic reactions, respectively. If we consider so-called neurotic reactions to be learned ways of reacting on the basis of certain expectations for punishment and/or gratification, there seems little basis for invoking other principles for the psychotic reactions. That such reactions are more deviant or esoteric or defiant of simple explanation does not rule out the likelihood that they too are learned ways of handling problems. Perhaps the extremity of such reactions has made it easier for some investigators to look to disease processes as a mode of explanation. Of course, certain of the "organic" psychoses are not easily dealt with from a psychological level of abstraction. However, as noted previously, any theory or level of abstraction has a range of convenience. To assert that it cannot be applied to all phenomena with equal success is not to assert that it is meaningless. Furthermore, most clinicians learn not to automatically attribute all the behavior of organics to structural damage.

We are not arguing that physiological or biochemical investigations of psychopathology be abandoned. We are, however, suggesting that those who are pursuing such research, while holding to a simple view that schizophrenia is a disease entity, are not likely to be greatly rewarded in their efforts. Pursuance of the physiological and biochemical correlates of behavior and learning are essential. But to reify learning processes into disease entities is hardly this. Such investigators should likewise take a long, hard look at the reliability of the classificatory scheme they are utilizing.

Finally, the reification of learned behaviors into disease entities makes it more plausible than it otherwise would be to attribute such entities to hereditary factors. Hereditary hypotheses have yet to demonstrate significantly their predictive usefulness. Studies supportive of such views turn out, too often, to be equivocal as regards interpretation. Furthermore, in view of the fact that any genetic potential seems to be limited in terms of the environmental milieu in which it occurs, the wisest course currently would appear to be more systematic efforts to pinpoint the situational conditions which give rise to pathological functioning.

Consistent with the rejection of the foregoing categorical and disease entity approaches is the necessity for a greater and more systematic *rapprochement* between the traditional subject matter of psychopathology and personality theory. Although the establishment of empirical relationships between various classificatory systems and such things as social history, response to psychotherapy or tranquilizers, and length of hospital stay is probably necessary and even desirable, exclusive emphasis on empirical procedures seems rather inefficient in the long run. Too often the purely empirical approach implies a neglect of the more basic variables responsible for demonstrated relationships, although some argue that this approach is the only reliable one currently to take (Bindra, 1959). As long as the demonstration of relationships serves as a stimulus for more basic investigations, both practical and heuristic values are served, however.

Currently, the typical textbook approach is to organize chapters or sections on the basis of nosological categories. There is a chapter on neuroses broken down into anxiety reactions, phobias, conversion hysteria, etc. Likewise, psychosis is separated into schizophrenia with all its subcategories, manic-depression, paranoia, etc. Each syndrome is elaborately described followed by the inevitable caution that in practice there is much overlap among groups. Following the behavioral description comes the more dynamic explanation for each category. In view of the heterogeneity within groups it is little wonder that such pat explanations seldom fit real cases and thus make predictions tenuous. Unfortunately, the very act of categorizing an individual often results in his taking on all the characteristics of the class in the eyes of the clinician. The writer has had the experience, for example, of observing clinicians who, having great difficulty in construing the dynamics of a patient, finally decide that he must be paranoid. If he is paranoid, they then reason, it follows that homosexuality must be the dynamic force behind his problems. And their problem is thereby solved.

What is being suggested here is the devising of more systematic and reliable techniques for making inferences from behavior regarding the individual's status on various constructs within a given theoretical system. More explicit manuals could be constructed that will enable us to reliably assess patients relative to such constructs as prototaxic distortions, superego strength, dependency, achievement motivation, permeability of constructs, growth potential, and so on. Good theories provide a network of relations among their constructs, so that gauging the patient's status on one variable often tells us something about his status on another. The very word psychopathological, as applied to a patient, suggests his need for treatment. If we are to base our techniques of treatment on an explicit theory of personality or behavioral change, it would seem much more efficient to describe the patient initially from that theoretical viewpoint. Indeed, the clinician's very theoretical description of his patient will usually suggest treatment procedures.

Current and potential systems of classification could be utilized to abstract patient expectancies or ego-strength rather than to type him merely with a static nosological label. Certainly the development of reliable interview schedules is feasible (Rotter, 1954), and in the field of projective testing the successful utilization of manuals for judging dependency (Fitzgerald, 1958), achievement motives (Atkinson, 1958), and aggression (Lesser, 1959), to cite just a few examples, has been repeatedly demonstrated.

Although this is not the place to discuss the comparative merits of various personality theories (see Chap. 12), a more systematic attempt to wed learning theory with psychopathology seems promising. Liverant (1963), in discussing the role of learning theory in clinical psychology, points out that learning theory does not at present offer a completed set of principles which can be applied directly to psychopathological phenomena. However, we might note parenthetically that other current approaches in the field of pathology have yielded only modest success thus far, so that perhaps a concerted application of learning theory to the field of psychopathology is worth a try.

More important is Liverant's suggestion that learning theory may provide a representational model for clinical phenomena, so that such phenomena may be translated into the language of learning. Obviously, however, such a translation will be valuable only insofar as it results in empirical consequences.

It is our belief that a social learning theory offers the greatest potential in the area of psychopathology. As applied to so-called pathological phenomena, a social learning theory can provide a bridge between the operationism and crispness of the older learning theories and the observational sensitivity and clinical astuteness of psychoanalytic systems. Seemingly, one of the more obvious difficulties in the past has been the tendency of those who used learning theories to ignore or derogate their clinical sensitivity and thus offer superficial, sketchy analyses of behavior. Likewise, but in reverse, the theory of many analytically oriented individuals offered no curb on their behavioral analyses, and so interpretational excesses became commonplace.

One such social learning theory is that proposed by Rotter (1954). A fuller exposition of this system is presented elsewhere in this volume (see Chap. 12) and will, therefore, not be repeated here. Briefly, however, this is an expectancy learning theory which utilizes an empirical law of effect. Groups of functionally related behaviors (*Need Potential* or *NP*) are seen as occurring in specified situations as a function of (1) the expectancies (*Freedom of Movement* or *FM*) of these behaviors leading to certain reinforcements in these situations and (2) the values of these reinforcements (*Need Value* or *NV*). The fourth concept, that of the situation, is implicit in the foregoing. Stated more simply in terms of a formula we have:

$$NP = f(FM \;\&\; NV)$$

Contentwise, this theory deals with empirically abstracted needs. Thus, all reinforcements are, for an individual, functionally related when the occurrence or nonoccurrence of one of them affects the expectancy for occurrence of the others. Likewise, we might group together under one *NP* those behaviors which have some functional relationship on the basis of their leading to the same or similar reinforcements. How many needs or classes of behavior one abstracts becomes a function of one's purposes. At general predictive levels we might discuss the need for approval. For specific purposes this could be broken down into approval from peers, authority figures, females, etc. The derivation of the needs of an individual is an empirical procedure based on the learning experience of the person rather than on a series of innate or instinctual drives.

This system, then, is not based on an internal energy model but, rather, sees behavior occurring because particular situations serve as cues to stimulate learned expectations for particular behavior-reward sequences. What we generally choose to call abnormal behavior may usually be considered as avoidant behaviors, which occur as a function of low expectancies for need gratification or high expectancies for punishment in certain need areas.

Such a system does not emphasize classification of people as paranoid or neurotic. Instead, it attempts to deal with learned expectancies and to abstract the relevant need areas in persons. Furthermore, it proposes to deal systematically with situational variables and their effect on human behavior. Thus, the question becomes not, "Is this patient paranoid?" but rather, "What are the conditions under which he displays such behavior?" We are interested in identifying the cues which lead him to categorize situations in a certain manner. And further, we want to know the conditions under which he learned such expectations. Thus, the result is a classification not so much of people, but a method of abstracting functional classes based on prior learning. Also, as noted previously, this theory makes an attempt to develop personality content categories that will mesh with learning as a process. This last point is particularly relevant in view of the earlier noted observation that many learning theories have been criticized for their failure to account for individual differences.

Social learning theory would abolish the use of traditional categories, such as schizophrenia in the disease-entity sense of the term, as explanatory concepts and instead abstract from behavioral manifestations status on various theoretical constructs. This should encourage us to focus our research on those situational conditions which typically facilitate or inhibit the expression of the various behaviors ordinarily labeled pathological. This is essentially the same suggestion made by Liverant (1960) in his critique of current notions regarding the nature of intelligence.

In dealing with pathological behaviors, social learning theory has no special principles apart from those which apply to behavior generally. Culturally deviant behavior is learned on the basis of the same principles as that of nondeviant behavior. Furthermore, the decision to apply social learning theory with the goal of altering a person's behavior in certain specified directions is a matter of social values rather than being deducible from the theory.

Perhaps in most cases, but by no means in all, deviant behavior may be considered as avoidant behavior. Such behavior occurs in an individual with low freedom of movement or expectancies for gratification of certain needs to which he ascribes a high value. Such low expectancies typically lead the person to learn behaviors which will avoid failures, rejections, or punishments which he anticipates in that need area. Such behaviors society often regards as irreal or perhaps undesirable. These so-called irreal, avoidance, or undesirable behaviors may include such things as the analytic defense mechanisms—projection, repression, or perhaps various fantasy activities. In other cases the avoidance may be more direct and on a physical level. At any rate, such behaviors all too often lead to further negative reinforcements, albeit delayed ones. The net result is a failure in the development or learning of coping behaviors. Indeed, a vicious cycle may easily be initiated wherein avoidance leads to more avoidance.

A high expectancy for punishment may lead to the initiation of most of the traditionally labeled "symptomatic" behavior. Thus, a person who expects failure or has severe doubts about his competency may learn to substitute obsessive-like thoughts of an irrelevant nature in order to postpone or avoid such doubts. Another individual may avoid competitive games because of a high expectancy for punishment (ridicule, failure, etc.). Such avoidance behavior may work or at least delay ridicule, but it will likely also lead to a failure to learn certain skills that ultimately will bring more punishment—such is the tragedy of most avoidant behavior. Of course, there are some instances wherein society may decide certain behaviors are deviant or undesirable and yet the individual who utilizes them is not doing so out of any avoidance reasons. He may simply not have learned (or introjected) certain standards that our culture deems necessary. Such behavior would still need altering, but the means might differ from those cases noted earlier. However, the vast majority of cases the clinician encounters (be they neurotic, psychotic, psychosomatic, etc.) reflect a basic problem in which the individual expects little gratification in a need area which he has come to value. Although his complaints may involve conflict, anxiety, lack of satisfaction, or perhaps excessive fatigue, they usually reduce themselves to a condition of low freedom of movement and high need value.

Where does low freedom of movement come from? In some cases it may simply arise out of lack of knowledge or inability to acquire it in order to behave in such a way as to reach the goals he has learned to value. In other cases, the nature of the goal itself seems to lead to punishments from others and thus engenders low freedom of movement. For example, one's goal of dominating others may lead to repeated punishments. Naturally, a great deal of low freedom of movement results from the person's evaluations of the present in terms of the past. Too often, these evaluations turn out to be "faulty." Indeed, therapy may often focus initially on such faulty generalizations as a first step in increasing freedom of movement. Such an increase in freedom of movement will enhance the potentiality of the occurrence of more reinforcing behaviors.

A major difference between this and other learning theories is the lack of an anxiety construct. However, such a construct does not appear necessary for this theory inasmuch as physiological descriptions are not a part of the theory and also, the theory is not committed to a drive-reduction position (Rotter, 1954). The behaviors which frequently follow from noxious stimulation typically might be labeled implicit behaviors and/or dealt with just as in the case of any other behavior. Perhaps the closest analogue of anxiety might be low freedom of movement or else high expectancy for punishment.

A social learning theory seems to offer many of the advantages of learning theory generally. Thus, it is operational; it emphasizes the importance of principles of acquisition of behaviors; it provides a highly useful representational model for construing clinical events—a model which often leads to ways of altering behavior that are quite different from the traditional methods which rely almost exclusively on highly verbal techniques (Rotter, 1954 and 1962); it emphasizes the role of situational factors in determining pathology;

and it rejects the notion of disease processes as the cause of pathology in favor of the view which regards deviant behaviors as learned ways of achieving gratifications and avoiding or delaying punishments. Furthermore, such a theory also handles some of the difficulties which seem to be inherent in the older drive-reduction models. It emphasizes, for example, the role of environmental cues in leading to learned expectancies for certain behavior-reward sequences rather than relying on an internal energy model. In addition, the inclusion of an operational expectancy construct provides a highly useful blending of the experiential level with the operationism of learning theory. Finally, its attempt to abstract relevant need areas on the basis of the directional aspect of behavior provides content areas that can synchronize with learning as a process.

Such a theory provides no completed set of principles and is, indeed, no panacea for the problems inherent in the field of psychopathology. It does, however, seem to offer a way of looking at clinical phenomena that may be highly rewarding.

REFERENCES

Allport, G. W. *Personality: A Psychological Interpretation.* New York: Henry Holt and Company, Inc., 1937.

American Psychiatric Association, Mental Hospital Service, Committee on Nomenclature and Statistics of the American Psychiatric Association. *Diagnostic and Statistical Manual: Mental Disorders.* Washington, D.C.: APA, 1952.

Ansbacher, H. L., and Ansbacher, Rowena R. *The Individual Psychology of Alfred Adler.* New York: Basic Books, 1956.

Arieti, S. *American Handbook of Psychiatry.* Vols. 1 and 2. New York: Basic Books, 1959.

Aronson, E., and Carlsmith, J. M. Performance expectancy as a determinant of actual performance. *J. abnorm. soc. Psychol.*, 1962, **65**, 178-182.

Atkinson, J. W. *Motives in Fantasy, Action, and Society.* Princeton, N.J.: D. Van Nostrand Company, Inc., 1958.

Bandura, A. Psychotherapy as a learning process. *Psychol. Bull.*, 1961, **58**, 143-159.

———. Punishment revisited. *J. consult. Psychol.*, 1962, **26**, 298-301.

Baxter, J. C., and Becker, J. Anxiety and avoidance behavior in schizophrenics in response to parental figures. *J. abnorm. soc. Psychol.*, 1962, **64**, 432-437.

Becker, J. Achievement related characteristics of manic-depressives. *J. abnorm. soc. Psychol.*, 1960, **60**, 334-339.

Bindra, D. Experimental psychology and the problem of behaviour disorders. *Canad. J. Psychol.*, 1959, **13**, 135-150.

Boisen, A. Types of dementia praecox: a study in psychiatric classification. *Psychiatry*, 1938, **1**, 233-236.

Cameron, N. The development of paranoic thinking. *Psychol. Rev.*, 1943, **50**, 219-233.

———, and Magaret, Ann. *Behavior Pathology.* Boston: Houghton-Mifflin Company, 1951.

Cantor, G. N., and Cromwell, R. L. The principle of reductionism and mental deficiency. *Amer. J. Ment. Defic.*, 1957, **61**, 461-466.

Coghill, G. E. *Anatomy and the Problem of Behavior.* Cambridge: Cambridge University Press, 1929.

Cohen, Mable, B., Baker, Grace, Cohen, R. A., Fromm-Reichmann, Frieda, and Weigert, Edith V. An intensive study of twelve cases of manic-depressive psychosis. *Psychiatry*, 1954, **17**, 103-137.

Dollard, J., and Miller, N. E. *Personality and Psychotherapy.* New York: McGraw-Hill Book Company, Inc., 1950.

Eysenck, H. J. *Dynamics of Anxiety and Hysteria.* London: Routledge & Kegan Paul, Ltd., 1957.

———, and Prell, D. The inheritance of neuroticism: an experimental study. *J. Ment. Sci.*, 1951, **97**, 441-465.

———, and ———. A note on the differentiation of normal and neurotic children by means of objective tests. *J. clin. Psychol.*, 1952, **8**, 202-204.

Farina, A. Patterns of role dominance and conflict in parents of schizophrenic patients. *J. abnorm. soc. Psychol.*, 1960, **61**, 31-38.

Fenichel, O. *The Psychoanalytic Theory of Neurosis.* New York: W. W. Norton & Company, Inc., 1945.

Ferster, C. B. Positive reinforcement and behavioral deficits of autistic children. *Child Developm.*, 1961, **32**, 437-456.

Fitzgerald, B. J. Some relationships among projective test, interview, and sociometric measures of dependent behavior. *J. abnorm. soc. Psychol.*, 1958, **56**, 199-203.

Ford, C. S., and Beach, F. A. *Patterns of Sexual Behavior.* New York: Harper & Brothers, 1951.

Freud, Anna. *The Ego and the Mechanisms of Defence.* New York: International Universities Press, 1946.

Freud, S. Beyond the pleasure principle. In *Standard Edition.* Vol. XVIII. London: Hogarth Press, 1955. (First German edition, 1920).

FROMM, E. *Escape from Freedom*. New York: Rinehart & Company, Inc., 1941.

GARMEZY, N., CLARKE, A. R., AND STOCKNER, CAROL. Child rearing attitudes of mothers and fathers as reported by schizophrenic and normal patients. *J. abnorm. soc. Psychol.*, 1961, **63**, 176-182.

GLAD, D. D. *Operational Values in Psychotherapy*. New York: Oxford University Press, 1959.

GODDARD, H. H. *The Kallikak Family: A Study in the Heredity of Feeblemindedness*. New York: The Macmillan Co., 1912.

GOLDSTEIN, M. J. The relationship between coping and avoiding behavior and response to fear-arousing propaganda. *J. abnorm. soc. Psychol.*, 1959, **58**, 247-252.

HARLOW, H. F. The formation of learning sets. *Psychol. Rev.*, 1949, **56**, 51-65.

HARVEY, O. J., HUNT, D. E., AND SCHRODER, H. M. *Conceptual Systems and Personality Organization*. New York: John Wiley & Sons, Inc., 1961.

HEBB, D. O. *The Organization of Behavior*. New York: John Wiley & Sons, Inc., 1949.

HERRON, W. G. The process-reactive classification of schizophrenia. *Psychol. Bull.*, 1962, **59**, 329-343.

HORNEY, KAREN. *Our Inner Conflicts*. New York: W. W. Norton & Company, Inc., 1945.

HORWITT, M. K. Fact and artifact in the biology of schizophrenia. *Science*, 1956, **124**, 429-430.

HUMPHREYS, L. G. Characteristics of type concepts with special reference to Sheldon's typology. *Psychol. Bull.*, 1957, **54**, 218-228.

HUNT, J. McV. *Intelligence and Experience*. New York: The Ronald Press Company, 1961.

JACKSON, D. D. Schizophrenia. *Scientific Amer.*, 1962, **207**, 65-75.

JOHANNSEN, W. J. Responsiveness of chronic schizophrenics and normals to social and nonsocial feedback. *J. abnorm. soc. Psychol.*, 1961, **62**, 106-113.

KALLMANN, F. J. *Heredity in Health and Mental Disorder*. New York: W. W. Norton & Company, Inc., 1953.

———. The genetics of mental illness. In S. Arieti (ed.), *American Handbook of Psychiatry*. New York: Basic Books, 1959.

KANNER, L. Early infantile autism. *J. Pediat.*, 1944, **25**, 211-217.

KELLY, G. A. *The Psychology of Personal Constructs*. Vols. 1 and 2. New York: W. W. Norton & Company, Inc., 1955.

KETY, S. S. Biochemical theories of schizophrenia. I, II. *Science*, 1959, **129**, 1528-1532 and 1590-1596.

KING, G. Research with neuropsychiatric samples. *J. Psychol.*, 1954, **38**, 383-387.

KINSEY, A., POMEROY, W. B., AND GEBHARD, P. H. *Sexual Behavior in the Human Male*. Philadelphia: W. B. Saunders Company, 1948.

KRAEPELIN, E. *Lectures on Clinical Psychiatry*. London: Bailliere, Tindall, and Cox, 1913.

KRETSCHMER, E. *Physique and Character*. New York: Harcourt, Brace and Company, Inc., 1925.

LESSER, G. S. Population differences in construct validity. *J. consult. Psychol.*, 1959, **23**, 60-65.

LEWIN, K. *A Dynamic Theory of Personality*. New York: McGraw-Hill Book Company, Inc., 1935.

LIVERANT, S. Intelligence: a concept in need of re-examination. *J. consult. Psychol.*, 1960, **24**, 101-110.

———. Learning theory and clinical psychology. In L. E. Abt and B. F. Reiss (eds.), *Progress in Clinical Psychology*. Vol 5. New York: Grune and Stratton, Inc., 1963.

LORD, EDITH. Experimentally induced variations in Rorschach performance. *Psychol. Monogr.*, 1950, **64**, (10), 1-34.

LORR, M., MCNAIR, D. M., KLETT, C. J., AND LASKY, J. J. Evidence of ten psychotic syndromes. *J. consult. Psychol.*, 1962, **26**, 185-189.

MCCLELLAND, D. C., ATKINSON, J. W., CLARK, R. A., AND LOWELL, E. L. *The Achievement Motive*. New York: Appleton-Century-Crofts, Inc., 1953.

MARK, J. C. The attitudes of the mothers of male schizophrenics toward child behavior. *J. abnorm. soc. Psychol.*, 1953, **48**, 185-189.

MASLING, J. The influence of situational and interpersonal variables in projective testing. *Psychol. Bull.*, 1960, **57**, 65-85.

MEDNICK, S. A. A learning theory approach to research in schizophrenia. *Psychol. Bull.*, 1958, **55**, 316-327.

MEHLMAN, B. The reliability of psychiatric diagnosis. *J. abnorm. soc. Psychol.*, 1952, **47**, 577-578.

MOWRER, O. H. Learning theory and the neurotic paradox. *Amer. J. Orthopsychiat.*, 1948, **18**, 571-610.

———. *Learning Theory and Personality Dynamics*. New York: The Ronald Press Company, 1950.

MUNROE, RUTH. *Schools of Psychoanalytic Thought*. New York: The Dryden Press, Inc., 1955.

NEWMAN, H. H., FREEMAN, F. N., AND HOLZINGER, K. J. *Twins: A Study of Heredity and Environment*. Chicago: University of Chicago Press, 1937.

O'KELLY, L. I., AND MUCKLER, F. A. *Introduction to Psychopathology*. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1955.

OVERALL, J. E., AND GORHAM, D. R. The brief psychiatric rating scale. *Psychol. Rep.*, 1962, **10**, 799-812.

———, AND ———. A pattern probability model for classification of psychiatric patients. *Behavioral Science*, 1963, **8**, 108-116.

PASTORE, N. The genetics of schizophrenia: a special review. *Psychol. Bull.*, 1949, **46**, 285-302.

PHARES, E. J. TAT performance as a function of anxiety and coping-avoiding behavior. *J. consult. Psychol.*, 1961, **25**, 257-259.

———. Perceptual threshold decrements as a function of skill and chance expectancies. *J. Psychol.*, 1962, **53**, 399-407.

———, AND ROTTER, J. B. An effect of the situation on psychological testing. *J. consult. Psychol.*, 1956, **20**, 291-293.

POLLARD, J. C., AND BAKKER, C. What does the clinician want to know about psychoactive drugs? In L. Uhr and J. G. Miller (eds.), *Drugs and Behavior*. New York: John Wiley & Sons, Inc., 1960.

RAPAPORT, D. The structure of psychoanalytic theory: A systematizing attempt. In S. Koch (ed.), *Psychology: A Study of a Science*. Vol. 3, New York: McGraw-Hill Book Company, Inc., 1959.

RODNICK, E. H., AND GARMEZY, N. An experimental approach to the study of motivation in schizophrenia. In M. R. Jones (ed.), *Nebraska Symposium on Motivation, 1957*. Lincoln: University of Nebraska Press, 1957.

ROE, ANNE. Integration of personality theory and clinical practice. *J. abnorm. soc. Psychol.*, 1949, **44**, 36-41.

ROTTER, J. B. *Social learning and clinical psychology*. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1954.

———. Some implications of a social learning theory for the prediction of goal directed behavior from testing procedures. *Psychol. Rev.*, 1960, **67**, 301-316.

———, SEEMAN, M., AND LIVERANT, S. Internal versus external control of reinforcements: a major variable in behavior theory. In N. Washburne (ed.), *Decisions, Values, and Groups*, Vol. II. London: Pergamon Press, 1961.

———. Some implications of a social learning theory for the practice of psychotherapy. The Ohio State University, mimeographed, 1962.

RUBIN, L. S. The psychopharmacology of lysergic acid diethylamide (LSD-25). *Psychol. Bull.*, 1957,**54**, 479-489.

SCOTT, W. A. Research definitions of mental health and mental illness. *Psychol. Bull.*, 1958, **55**, 29-45.

SHELDON, W. H. *The Varieties of Human Physique*. New York: Harper & Brothers, 1940.

———. *The Varieties of Temperament*. New York: Harper & Brothers, 1942.

SHIELDS, J., AND SLATER, E. Heredity and psychological abnormality. In H. J. Eysenck (ed.), *Handbook of Abnormal Psychology*. New York: Basic Books, 1961.

SULLIVAN, H. S. Tensions interpersonal and international: a psychiatrist's view. In H. Cantril (ed.), *Tensions that Cause War*. Urbana, Ill.: University of Illinois Press, 1950.

SZASZ, T. S. Malingering: "Diagnosis" or social condemnation? *Arch. Neurol. Psychiat.*, 1956, **76**, 432-443.

———. The myth of mental illness. *Amer. Psychol.*, 1960, **15**, 113-118.

WELLS, F. L., AND RUESCH, J. *Mental Examiner's Handbook*. New York: Psychol. Corp., 1945.

WHITE, R. W. *The Abnormal Personality*. New York: The Ronald Press Company, 1956.

WITTENBORN, J. The behavioral symptoms for certain organic psychoses. *J. consult. Psychol.*, 1952, **16**, 104-106.

———, AND BAILEY, C. The symptoms of involutional psychosis. *J. consult. Psychol.*, 1952, **16**, 13-17.

WITTENBORN, J., AND WEISS, W. Patients diagnosed manic-depressive psychosis-manic state. *J. consult. Psychol.*, 1952, **16**, 193-198.

WOLPE, J. The experimental foundations of some new psychotherapeutic methods. In A. J. Bachrach (ed.), *Experimental Foundations of Clinical Psychology*. New York: Basic Books, 1962.

ZIGLER, E., AND PHILLIPS, L. Psychiatric diagnosis and symptomatology. *J. abnorm. soc. Psychol.*, 1961(*a*), **63**, 69-75.

———, AND ———. Psychiatric diagnosis: a critique. *J. abnorm. soc. Psychol.*, 1961(*b*), **63**, 607-618.

———, AND ———. Social competence and the process-reactive distinction in psychopathology. *J. abnorm. soc. Psychol.*, 1962, **65**, 215-222.

CHAPTER 14

Social Psychology: Some Selected Topics

JOHN T. LANZETTA AND JOAN E. SIEBER
Dartmouth College *Stanford Center for Research and Development in Teaching*

Social psychology has been variously defined as: "the science of the behavior of the individual in society" (Krech, Crutchfield, and Ballachey, 1962), "the experimental study of individuals in their social and cultural settings" (Lambert and Lambert, 1964), and "the study of individual behavior in a social setting" (Sanford, 1961).

The distinctive focus of the social psychologist is on the *individual* in interaction with his social world, a focus which strengthens his affinity with general psychology more so than with sociology, anthropology, or the other behavioral sciences. Further supporting their affinity, and in sharp contrast with other social science disciplines, social psychologists have increasingly come to exploit laboratory experimentation as a preferred method for scientific advance. This preference is, of course, tempered by an appreciation of the limitations of the method for the study of certain classes of problems, but on the whole increased sophistication and experimental ingenuity have broadened the range of phenomena capable of being studied in the laboratory, and experimental techniques are now becoming widely employed.

The diversity of phenomena, theory, and methods encompassed by social psychology precludes comprehensive and systematic coverage of the field in a brief space. Without a unifying theoretical system or the bond of common methodology there is little rational basis for sifting, ordering, and organizing the mass of data accumulated. One can impose an arbitrary organization to suit some individual bias or one can sample the diversity in the hope that the flavor and character of the field will thereby be revealed. Both approaches have been employed in this chapter in that a sample of topics has been selected for examination, but the selection reflects individual preference rather than the "importance" or state of development of the area. Because social psychology is still somewhat self-conscious about its selection of research strategies, and more especially its use of experimental methods, we turn first to a consideration of some general issues in the selection of an experimental strategy before proceeding to an examination of the theories and findings in several substantive areas.

THE EXPERIMENTAL APPROACH

What Is an Experiment? Scott and Wertheimer (1962), in a recent work on research methods, distinguish between three common research strategies in the behavioral and social sciences: naturalistic observation, systematic assessment with controlled stimuli, and experiments. The three procedures differ primarily in the degree to which research variables are actively controlled. Naturalistic observation, as the name suggests, eschews control over situational conditions or behavior, and concentrates on obtaining a valid and reliable record of events as they unfold in the "real world." Systematic assessment with controlled stimuli involves a moderate degree of control over a stimulus situation but avoids direct interference with or manipulation of responses. This strategy is typically associated with the investigation of interindividual variability in response to a fixed stimulus situation, i.e., in response to tests, questionnaires, or structured interviews. In experiments, the focus is on the manipulation of conditions under which people behave in order to assess the effects of a presumed determinant of behavior. "To 'experiment' is so to manipulate the conditions under which some event is made to occur that the effects produced by different sets of initial conditions can be contrasted" (Zelditch and Hopkins, 1961, p. 465). Stress is placed on achieving maximum control over the variable(s) of interest as well as over potentially contaminating variables.

Thus, experiments differ from the other techniques in the degree of control permitted the experimenter. Only in an experiment can control be exerted over the conditions under which observations are made, over the time and place of making the observations, and over the characteristics of the sample being observed. Also, and possibly of most importance, only experimental methods permit refined control over extraneous variables that may otherwise affect the behavioral event of interest. In short, experimental methods are elegant —at times too elegant for the problem at hand.

When and Why Experiment? In this psychological age it is no longer heresy to suggest that scientists very likely differ in their personal goals as scientists. The primary source of motivation may be the possibility of a solution to a practical problem, the social prestige and moderate security associated with performance of the scientist's role, or the stimulation and satisfaction intrinsic in confronting and sometimes solving challenging problems. "In all these cases scientific activity appears as a means to the attainment of ends that are not defined exclusively in terms of the scientific process. There are, however, proximate goals of every scientist qua scientist, namely, to answer pertinent questions" (Kaufmann, 1958, p. 67). These goals can be labeled theoretical goals of scientific inquiry and ". . . they are defined exclusively in terms of deductive logic and empirical procedure" (Kaufmann, 1958, p. 67).

Pertinent questions may be raised by two sorts of problems, logical and empirical problems. To solve the former is to explicate an implicit meaning, to solve the latter is to perform a series of steps terminating in the verification of an answer to a question of fact.

Four different situations with respect to the explanation of facts may be distinguished:

1. The explanation may be logically entailed in already established knowledge.
2. The laws referred to in the explanation may have been established, but not the facts required.
3. The facts referred to in the explanation may have been established but not the laws.
4. Neither the laws nor the facts required may have been established.

This classification applies to prediction as well, since the logical structure of explanation is identical to that of prediction (Kaufmann, 1958, p. 69).

Unfortunately, in social psychology we find ourselves most often confronted with situation 4; that is, with problems in which neither the laws nor the facts required have been established. To retreat one step further, we are often not even sure that we are posing the right questions—questions whose answers may transcend the questions. For the objectives of science are not the accumulation of facts nor even the accumulation of limited empirical generalizations. Ideally the search is for laws arranged in a hierarchy at the apex of which is a small number of "universal laws." This is to say, the search is for a theory or theories with sufficient generality and precision to encompass the phenomena of interest and to predict new phenomena.

But the search for universal laws must be considered an ideal goal, since there is no way of judging beforehand what "facts" or limited propositions will prove most useful in achieving this objective. From the point of view of the logician, the procedures of an empirical science must consist in the acceptance or elimination of *specific propositions* in accordance with given rules. From the point of view of the empirical scientist the same thing is true, but he is less self-conscious about the logical foundation of the "rules" and would generally equate them with his procedures. The important point to note, however, is that rules or methods can only be evaluated with respect to their efficiency in testing specific propositions. Too often, a method is criticized as overly rigorous and, therefore, capable of stifling creative theorizing when the difficulty is not with the method per se but with the proposition selected for testing. Methods may differ in heuristic value since they differ in the range and richness of the observations permitted but they cannot be formally differentiated with respect to their relevance for constructing general theories.

Establishing the correct answer to a question may require a long chain of steps, and considerable acumen in envisaging such a chain is required; the "rules" are not well enough explicated to permit a routinized approach to verification of a scientific hypothesis. Certain general criteria can be advanced, however, in terms of which the appropriateness of a procedure can be assessed.

Possibly of first importance, *a method should be efficient for answering the question posed.* Hypotheses about behavior differ in kind, and such differences should be taken into account when selecting a method for testing an hypothesis. March and Simon (1958) distinguish between three types of propositions:

1. Propositions stating the dependence of one variable on one or more other (independent) variables. There are two variants of this type of proposition: those containing variables capable of assuming a range of values and propositions in which one or more variables are dichotomous or take on discrete values that are not ordered. An example of the first variant is the hypothesis: The greater the uncertainty of a decision, the greater the amount of information sought before making a choice. The dependent variable here is "amount of information seeking," the independent variable "the level of uncertainty of a. decision." An example of the second type is the proposition: Democratic leadership leads to greater efficiency and satisfaction than autocratic leadership. "Style of leadership" is the independent variable here and "efficiency" and "satisfaction" the dependent variables.

2. Propositions embodying a qualitative descriptive generalization. Example: One of the important kinds of activities that go on in groups is tension reduction behavior.

3. Propositions asserting that a particular . . . structure or process performs a particular function (using function, now, in its biological or sociological sense—the function of the heart is to circulate the blood). Example: Rigidity of behavior increases the defensibility of individual action.

We would add to these three types a fourth:

4. Propositions embodying a generalization about elements of a class (most often about samples of a population of organisms). Example: Women have stronger affiliation motives and weaker achievement motives than men.

Propositions of types one and three are most appropriately tested by experimental methods, whereas propositions of type two are most appropriately tested by naturalistic observation and type four hypotheses by systematic assessment. It is possible, of course, to test each type of proposition by any one of the research strategies, but considerations of cost, efficiency, and feasibility would dictate in most cases a match such as is suggested above. To take but one example, consider the type one hypothesis, "The greater the uncertainty of a choice the greater the amount of information sought by an individual prior to taking the decision" (like all such general propositions this tacitly includes the proviso "other things being equal"). It is possible to test this proposition by systematically observing under real life conditions the search behavior of individuals for a large number of decisions, subsequently classifying or ordering the decisions in accord with a measure of "uncertainty" and the search behavior in accord with a measure of "amount of information seeking" and then relating differences in search behavior to differences in uncertainty of the decision tasks. But such a strategy of naturalistic observation would present innumerable practical and theoretical difficulties and would almost certainly produce data which were equivocal or impossible to interpret. For example, many extraneous variables which might well be related to the behavior under investigation could not be controlled; the kinds of decisions being made, the importance of the decisions, and the urgency of the decision, would no doubt vary over time for any one decision maker and for any one period would be different for a sample of decision makers. In addition, the sample of decision makers would not be controlled and could thus be biased with respect to a variable of major importance, i.e., possibly the organization favors for promotion individuals who are quick decision makers so that the sample observed would be characterized by strongly reinforced habits of not acquiring information before decisions. Such tendencies would seriously distort the results, since the average level of information seeking might be so low as to preclude distinguishing between levels of uncertainty. An experimental test of this hypothesis would permit control over the extraneous variables cited as well as others which, though potentially important sources of variance in results, had not been identified a priori by the experimenter.

The type of hypothesis under test is not, however, the sole criterion for selection of a research strategy. There are other advantages and disadvantages associated with different strategies that are of equal importance to a choice. Strategies differ for example in the degree of precision possible by use of the method, in the ease with which the study is repeatable, and in the degree of generalization possible. Experiments are clearly superior to the other strategies in the degree of precision possible and in repeatability, but generally weaker in terms of permitted generalizations. It is on the latter point especially that experimental studies in social psychology have been criticized. After all, the argument runs, although a study is necessarily conducted using a particular group of subjects, with specific measuring techniques and under special conditions, the results of the study are of no value unless they yield information about the real world phenomena which presumably suggested the study in the first place: ". . . a major objective of all research is induction from the observed to the not-yet-observed. In order to make the inductive leap reasonably convincing, some kind of assurance is needed that the sample of events actually studied represents, in essential respects, the larger population to which results are generalized" (Scott and Wertheimer, 1962, p. 63).

Laboratory studies with their controlled artificial environments, college sophomore subjects, and arbitrary control of the level of extraneous variables may be precise and repeatable, but they are rarely representative of the situations to which a social psychologist wishes to generalize. Naturalistic observation, on the other hand, if based on an adequate sampling of the situations under study, avoids difficulties of induction by its focus on the real world. The investigator can thus be sure that the relationships observed do occur in the subjects' natural environment, and at the same time he can assess the importance of an event or relationship by noting the relative frequency with which it occurs. Systematic or controlled assessment procedures fall somewhere between experiments and naturalistic observation; although typically, the subject is observed in his natural environment, the process of measuring itself may lead to a distortion of results.

In defense of laboratory experiments, however, it should be noted that criticisms such as those paraphrased above are most appropriately leveled at experiments as typically performed. Experiments can be designed so as to permit more confidence in generalizing, although the cost of a study often increases in direct proportion to the degree of realism achieved. Field experiments, in which the natural environment provides the setting, and experimental manipulations and controls are introduced in the same fashion as other administratively controlled changes, provide one response to the problem of achieving generalizations without too great a sacrifice in precision and potential repeatability—but at high cost and with other attendant problems that limit its widespread use. In recent years, manned simulation has been developed and refined as an experimental tool for accomplishing much the same objectives as field studies without some of the attendant difficulties, especially those arising from unwanted administrative changes during the conduct of the experiment. But this technique, too, has proved enormously expensive and somewhat unwieldy. However, the techniques are new and one may reasonably anticipate that with further experience both the costs and inflexibility of manned simulations will be reduced—they may become as important a tool to the social psychologist as the cyclotron is to the physicist.

On the whole, then, the selection of a research strategy cannot be based on any single criterion. Scott and Wertheimer very admirably summarize the issues:

> Controlled assessment makes it possible to elicit responses and measure them precisely; but it only permits inferences concerning correlations, not 'causes,' and it may influence the very phenomenon being studied. Experimentation

> can provide more convincing evidence about 'causal' relationships by virtue of its better control over extraneous influences, but it may distort the attribute under study, and elaborate controls may impose an excessive degree of 'unreality' on the phenomenon. Further, experimentation in some areas may be out of the question on practical or ethical grounds. Naturalistic observation techniques have the advantage of providing 'rich' records of behaviors and relationships in their natural habitats; but causal or even correlational inferences from them are often exceedingly tenuous, and many kinds of relationships cannot be studied in this fashion at all because the critical stimuli and responses are not normally observable.
>
> Choice of design must therefore depend to a large extent on the stage of investigation, the resources available, and the specific goals of the research. In the earliest phases of a study it is perhaps wise to 'get a feeling' for the phenomenon by observing it in its natural habitat. Systematic assessment and experimentation can then follow to help clarify the critical variables and relationships. The conceptual scheme derived from this work can again be referred to a natural setting to determine its applicability there.
>
> Experimentation on a large scale is expensive, as is systematic assessment, especially if the theoretical population is to be sampled adequately and the experimental conditions are to be made as similar as possible to those of the natural habitat. So perhaps over-all research efficiency is initially furthered by small pilot studies on selected groups, until sufficient confidence can be developed in hypotheses and measuring procedures to warrant the considerable expense of a large-scale study.
>
> Finally, the goal of the researcher is an important factor, perhaps the chief one, in making a selection among the various available strategies. Virtually any goal can constitute, from one point of view or another, a legitimate objective for psychological research. It would seem inappropriate to legislate the objective or restrict the method; rather, an adequate meeting of the two should be encouraged, so that whatever the investigator's goals they can be substantially furthered by his research design (Scott and Wertheimer, 1962, p. 63).

We turn now to an examination of three broad areas of social psychological inquiry—socialization, perceptual-cognitive phenomena, and social interaction. In all three areas there have been recent theoretical advances which provide some structure for diverse findings but, more important, hold some hope for further theoretical and empirical progress. The areas also provide some contrast in the typical research strategy employed for collecting data. For reasons covered later, studies of socialization have largely depended on naturalistic observation and systematic assessment procedures—experimental studies have recently been increasing in frequency, but there are ethical and practical limitations to their extended use. In contrast, studies of perceptual-cognitive phenomena and social interaction have been largely experimental—conducted in university laboratories using college subjects. As a consequence the findings on socialization are rich in their implications and, though often logically suspect, have high "face validity," whereas those on perception-cognition and social interaction, though more firmly established, appear "artificial" and lacking in generality.

SOCIAL LEARNING: THE SOCIALIZATION PROCESS

Only the most avowed nativists would today dispute the importance of early experiences in the development of human personality. Even among subhuman species contact with other members appears necessary for the development of the behavior patterns and emotional responsiveness characteristic of the species (e.g., Melzack and Scott, 1957) and certainly among humans, contact with other people from an early age appears essential for the development of normal adult human behavior. The few recorded instances of children raised in relative isolation (e.g., Davis, 1949) show that the absence of human contact for the first five or six years prevents the development of human capabilities and responses, and studies of hospitalized and institutionalized children who experience a prolonged separation from the mother and home environment indicate that such separation leads to serious emotional and intellectual retardation (Bowlby, 1952).

Human behavior is primarily learned behavior—learned principally from others—and the learning starts in infancy and continues throughout life. Thus the process of socialization starts when the organism is first exposed to its human environment and continues until its death, producing in the process a behaving, thinking, feeling, and evaluating organism remarkably like others in its environment. As Lambert and Lambert (1963) have remarked, ". . . societies continue in large part to rebeget their social kind from generation to generation" (p. 8), though they also have the unfortunate (and at times fortunate) tendency to beget a fair proportion of deviate types.

Thus the process we call socialization is ubiquitous and obviously lies at the core of social psychology. Performance in a social situation is determined not solely by the nature of the situation but in large part by the habits, beliefs, attitudes, and affective states of the performing individuals —in short, by just those aspects of personality which are the products of socialization. An understanding of the processes and major consequences of socialization should thus not only enable us to specify the "how" and "what" of human personality but should assist in identifying the social variables and the mechanisms that account for the diversity and complexity of adult social behavior.

Methodological Problems. However, as is often the case in science, conviction regarding the importance of a process is not necessarily accompanied by knowledge of the details of the phenomena. In the case of socialization current information is contradictory and inconclusive. There is no core of well established and interrelated principles, no theory, to guide and focus the study of socialization. This reflects, in large part, the great complexity of the subject but also the difficulty of utilizing, comprehensively and systematically, experimental methods.

In an attempt to uncover principles of social development, the investigator wishes to identify covariations between early experiences and later behavior. Ideally, he would like to be able to control and manipulate the nature of the experiences and to control the conditions under which the consequent behavior is exhibited. Only with such control will he be able to unequivocally establish the causal chains between early social events and subsequent behavioral events. But experimentation in this area, with human subjects, is severely circumscribed for several reasons. First, ethical considerations preclude manipulations that could conceivably produce harm or result in deviant behavior. One could not justify, for example, raising children in relative isolation, removing infants from their parents, or exposing children to intellectually impoverished environments, in order to assess the effects of such deficits on personality development. Second, many of the most interesting and socially significant behavior patterns, e.g., delinquency, undoubtedly reflect the joint effects of a large number of features of early training and such complex interdependencies would be impossible to reproduce through experimental procedures. Thus investigators in this area have been forced to rely heavily on field studies and to utilize correlational methods to determine the strength of the relationships between antecedent and consequent behavioral events. Such studies have uncovered many covariations but in most cases we do not know as yet whether the covariations do, in fact, represent causal relationships.

Bandura and Walters (1963) have illustrated how covariations may be a function of variables other than those identified by an investigator:

> For example, parental preference for non-aggressive psychological forms of discipline has often been regarded as an antecedent of guilt in children. However, a strongly moralistic non-aggressive orientation in a parent may both inhibit him from using physical and verbally aggressive methods of discipline (thus limiting the range of techniques at his disposal) and at the same time, through modelling, produce guilt in his children.
>
> In other cases, the causal links may be even more remote. Aggressively demanding fathers frequently encourage their sons to adopt an orientation similar to their own as a means of getting ahead in life, and reward them for aggressive and demanding behavior, particularly in their social interactions outside the home. Such fathers are also likely to demand a great deal of their wives' time and attention, thus forcing them to reduce the time and attention they give to their children. In such cases, maternal frustration of dependency would be associated with aggressive orientations in children, but it would be erroneous to conclude that the former was an antecedent of the latter (Bandura and Walters, 1963, pp. 39-40).

Thus, although the field approach has the advantages of permitting one to observe complex interactions and allows one to examine the effects of extreme environments, there is always the danger that the investigator who relies on this method will wrongly conclude about the direction of a causal relationship or about the importance of a presumed causative variable. The laboratory-experimental approach as noted earlier is clearly superior to field investigation in that, if well applied, it "generates relatively precise statements of cause and effect which are remarkably unambiguous" (Bijou and Baer, 1960, p. 141). But, since for the ethical and practical reasons discussed, it should not be extensively applied, experimental studies alone cannot be depended upon to provide the data for a theory of socialization.

> Theory-based naturalistic and longitudinal studies are thus indispensable adjuncts to laboratory methods. Consequently, it is important to seek out conditions in nature in which certain stimulus and response variables are present and others in which they are absent and to note how these covary. Ideally, a concerted attack on problems of social development should include both field and laboratory studies; and the latter should be designed in such a way as to reproduce as closely as possible the social stimuli and responses that occur in real-life situations (Bandura and Walters, 1963, pp. 42-43).

Whether by design or accident such an "ideal" mix of laboratory and field studies has characterized the study of socialization and we turn now to a consideration of some of the empirical findings and theoretical notions that have emerged from these investigations.

Some Empirical Findings. Investigators in this area have examined the effects of a wide range of early social experiences and a large array of complex social responses. In some cases the study has been stimulated by concern over the effects of a particular environmental event, e.g., maternal deprivation, on many aspects of later development, while in other cases the investigation has been concerned with uncovering the causal antecedent events for a socially significant behavior pattern, e.g., aggression. The findings are consequently difficult to organize or synthesize, and one can only select from the literature on an *ad hoc* basis and, at that, only a small sample of the important and significant studies.

General Conditions in the Early Environment. Several large field studies on human infants and more recent research on primates and other animals have supported the widespread assumption that early social deprivation has widespread consequences for personality development. Studies by Bowlby (1952) and Goldfarb (1943) support Bowlby's assertion that such investigations ". . . make it plain that, when deprived of maternal care, the child's development is almost always retarded—physically, intellectually, and socially—and that symptoms of physical and mental illness may appear. Such evidence is disquieting, but sceptics may question whether the retardation is permanent and whether the symptoms of illness may not easily be overcome. The retrospective and follow-up studies make it clear that such optimism is not always justified and that some children are gravely damaged for life" (Bowlby, 1952, p. 15).

Less equivocal though less dramatic evidence has come from an ingenious series of experimental studies on primates. Studies of monkeys raised in varying conditions of isolation and with several varieties of surrogate mothers have demonstrated that:

> Total isolation for two years resulted in failure to display social or sexual behavior in the next two years, spent in a joint living cage. Results on six months of such isolation are still being gathered and suggest severe, but not complete, social deficits. Only mild effects have been observed thus far in monkeys isolated through the first 80 days of life. Partial isolation has produced behavioral aberrations in many monkeys and sexual inadequacy in all males and in all but one female.
>
> . . . .
>
> Infants raised by live mothers were more advanced in social and sexual behavior than infants raised by surrogate mothers in a controlled playpen situation.
>
> . . . .
>
> Over-all, it appears that the longer and the more complete the social deprivation, the more devastating are the behavioral effects (Harlow and Harlow, 1952, p. 224).

Apparently such early deprivation experiences also have the potential of perpetuating their undesirable consequences. Correlational studies of humans and the primate studies both indicate that organisms that experience early deprivation are themselves less capable of properly caring for their offspring. Poorly mothered female monkeys, for example, show definite unconcern for a threatened offspring and sometimes cruelly rebuff and attack their children. Such rejection and hostility presumably provides an inadequate social environment for the social development of the offspring, perpetuating a vicious cycle in which socially deprived children become parents who themselves socially deprive their children, leading to another generation of rejecting parents. But such a somber conclusion goes well beyond the present data.

Social deprivation per se is apparently similar in its impact to persistent general rejection by parents. The pervasive emotional tone of the home environment (loving or rejecting) seems to have greater consequence for subsequent development than the permissiveness or punitiveness of parents or the stability of the marital unit (Kluckholm, 1953). Rejection apparently results in slow maturation, apathy, unresponsiveness, and dependency. In addition, as noted above for social deprivation, the unloved child tends to be an unloving adult providing the conditions for a self-perpetuating cycle (Berelson and Steiner, 1964).

Much less extreme interference with normal social interaction than is involved in social deprivation or rejection over an extended time period may also result in deficiencies in development. Recent studies of imprinting in animals suggest that deprivation during certain critical periods can result in the failure to develop "normal" responses or may result in the development of deviate behavior patterns. Although there is currently no evidence of such imprinting in humans, Gray (1958) has speculated that a similar process occurs in infancy. According to one version of his views (Berelson and Steiner, 1964), the bond is formed between the infant and others of its kind between the ages of six weeks and six months; hence, social deprivation in this period may have long-term deleterious effects.

Though imprinting in humans has yet to be demonstrated, the possibility of such a process makes infinitely more complex the task of uncovering the causal antecedent conditions for social behavior. One is now forced to consider not only whether a particular condition existed or did not exist in the early experience of an organism, but also the particular time periods during which the variable operated. Thus certain contradictory results concerning the effects of early experiences may be reflecting a failure to control for the time at which the experience occurred. The phenomena of imprinting open new vistas but present additional difficulties for investigators in this area.

Socializing Agents and Settings. Although such general social deprivation as that discussed is clearly a determinant of later behavior, we usually mean by socialization procedures the explicit "influence" practices and settings more or less characteristic of a culture group. An individual in our complex society is simultaneously and successively exposed to a number of different socializing agents, e.g., mother, teacher, minister, and to social settings, e.g., family, school, religious group, socio-economic group. Each of these agents and settings provides an interlocking series of learning situations characterized by a particular system of rewards and punishments and certain models for "proper" behavior. As Barker and Wright (1954) have shown, it is possible to distinguish between social settings in a community and to count the number of distinctive settings, i.e., the number of settings potentially requiring distinctive response patterns. In one comparison between an English and American community he found that the American town had more settings (579) for its fewer inhabitants than the English town had (494) and, in consequence, the American community placed on its inhabitants more demands for fulfilling a variety of roles. The effects of this diversity of demands on personality formation have yet to be demonstrated, but the results indicate the enormous diversity of the socializing forces to which an individual is exposed.

From the point of view of social learning, however, such settings may differ along only a few dimensions. For example, a recent study of family child rearing practices (Whiting, 1963; Lambert and Triandis, 1961) suggests that family settings differ in the following ways:

> (1) the demands for responsibility made on children, that is, the number and kind of duties expected of them; (2) the emotionally positive behaviors of mothers to their children, such as praise, absence of physical punishment, and general warmth; (3) the degree of control demanded over aggression toward peers both inside and outside the family; (4) the degree of control over aggression and disobedience towards parents; (5) the extent to which the mother does the caretaking of babies; (6) the extent of her caretaking of older children; (7) the degree of the mother's emotional stability (does she blow hot and cold?) (Lambert and Lambert, 1964).

It is likely that settings other than the family could be ordered along several of these dimensions, and that different settings would occupy different positions. Play situations, for example, demand less control over aggression toward peers than school settings, and family situations place more responsibility demands on members than clubs or other social organizations. Further efforts to dimensionalize environmental settings and the behavior of socializing agents will certainly help to uncover the variables that make a difference to the social development of the individual. At present no generally accepted dimensional analysis exists; important factors have, on the whole, been identified only with respect to specific classes of social responses, several of which are considered below.

Aggression. Most societies devote considerable attention to the control of aggression presumably because its overt expression in extreme forms is generally disruptive and interferes with the attainment of important societal goals. Whatever the techniques used for attaining such control, they are obviously only moderately successful as is apparent from the prevalence of institutionalized means of punishing and controlling individuals who deviate too far from defined norms. Such failures of social controls may reflect the fact that aggression is a "primordial reaction" to the frustration of pleasure-seeking or pain-avoiding responses (Freud, 1920), and so long as socializing agents or social practices frustrate individuals, aggression should be a natural and expected consequence. Though evidence has generally supported the frustration-aggression causal chain (Doob et al., 1939), it is equally clear that aggressive habits are not primordial but are acquired through learning. Sears et al. (1957), for example, have shown that aggressive children are likely to come from homes where rules about aggressiveness are permissive and conversely, that less aggressive children have been confronted with strong rules against aggression. Davis (1943) and Davis and Havighurst (1947) have reported that lower-class parents encourage and reward aggression to a greater extent than middle-class parents, while at the same time they impose fewer frustrations on their children's impulses. Thus the oft-noted higher incidence of aggression in lower-class children presumably reflects differences in training conditions rather than variation in the frustration potential of lower- and middle-class home environments. Bandura and Walters (1959) in a study of aggressive and nonaggressive boys also found that the parents of aggressive boys more actively encourage and condone aggression than do the parents of nonaggressive boys.

Such correlational studies have been considerably bolstered by recent experiments which show that reinforcement for aggression during an experimental period increases the frequency of aggressive responses in a test period (Patterson, Ludwig, and Sonada, 1961; Cowan and Walters, 1963; Walters and Brown, 1963). The latter studies also show that learned aggressive responses are similar to other nonsocial responses in that they extinguish slower when trained on an intermittent reinforcement schedule and generalize to new situations. It appears clear that aggressive responses can be learned and that observed differences in aggressive behavior most likely are due in large part to differences in parental attitudes toward aggression and not to differences in the frustration inherent in a social environment.

Dependency and Achievement. The human infant is almost totally helpless at birth and remains relatively so for a number of years. During this interval he must depend on others in his environment for the satisfaction of most of his physical and psychological needs. In North America, at least, this period of total dependency is short lived, and very early a great deal of energy is directed to the task of teaching the child to clothe, feed, and otherwise care for itself, and later, to master the skills that permit in adulthood an independent status. Thus independence in the performance of a variety of "tasks" is encouraged and reinforced from a very early age, whereas "person-oriented" dependency in moderation continues to be expected at all stages of an individual's development and is permitted toward a wide variety of people, e.g., siblings, acquaintances, teachers, and officials (Bandura and Walters, 1963).

Person dependency of children appears to be related to the affection and warmth shown the child by parents (Bandura and Walters, 1959). Parents who are warm, affectionate, and spend time in caring for their sons have children who show a high degree of dependency behavior (Bandura, 1960), and extreme degrees of mothering can apparently create very strong dependency in children (Levy, 1943). Several experimental studies suggest that such generalized parental behaviors as affection and warmth have an affect on dependency because they serve as reinforcers for dependency behavior. For example, Nelsen (1960) has shown that reinforcement of dependency by approval results in an increase in dependency responses in a subsequent social interaction situation, whereas verbal rebukes for dependency responses reduce the incidence of subsequent dependent responses.

A study by Cairns (1962) supports this finding and further suggests that children who have a history of reward for dependency are more responsive to social reinforcement in general. Thus, high dependency would result in greater ease in socialization of other response patterns, presumably a desirable goal from the point of view of socializing agents. But strongly developed dependency habits would make more difficult the ultimate achievement of an independent sociovocational adjustment. Thus from the point of view of the socializing agents, there is a conflict between encouraging high dependency so as to facilitate social training and discouraging dependency to enhance the prospects of a satisfactory adjustment to an adult role. The conflict is often revealed in the ambivalence of both parent and child toward dependent and independent behavior which sometimes becomes especially acute in mid- and late adolescence.

Closely related to dependency-independency responses are the complex behavior patterns from which we infer a drive for achievement or ambition. McClelland and his associates (1961), who have pioneered in the measurement of this drive and in the search for its antecedents, define achievement as a drive to do well for its intrinsic satisfaction as contrasted with a striving for success for material or social rewards.

The origins of this "drive" appear to be early in life and it is well developed before the teens. Rosen and D'Andrade (1959) have shown that parents of high-need achievement boys set higher goals for their children and react more positively to their performance. In addition, the mothers of high achievers are more domineering than the mothers of low achievers, while the converse is true for the fathers. Parental pressure for achievement, however, must be consonant with the child's capacities; if the demands are reasonable thus ensuring a high ratio of successes to failures, strong achievement results, otherwise not (McClelland, 1961). Also, if successes are accompanied by physical demonstrations of affection, e.g., hugging, kissing, the later drive for achievement appears to be stronger, as is also the case if parental demands and rewards are initiated early in the child's life (McClelland et al., 1953).

PRINCIPLES OF SOCIAL LEARNING As noted earlier, social development is primarily a product of learning—learning which takes place in social contexts and is more or less controlled by socializing agents. That parents, peers, and other socializing agents play an important role in shaping social behavior is strongly indicated by the sample of studies to which we previously referred: either some specific pattern of behavior of an agent was shown to be an antecedent of the response, or there was a strong association between the socializing agent's behavior and that of the subject. But how do parents, peers, and teachers effect a modification of behavior? How are new patterns of behavior acquired? Are there general principles of learning which apply to complex social responses as well as nonsocial responses, and can they be extended to account for antisocial as well as social behavior?

Until recently, psychologists have not devoted sufficient attention to a clarification of such questions. Attempts to account for social development have leaned heavily on poorly defined concepts such as imitation or on a simple extension of learning principles developed and tested on the basis of animal and human studies in nonsocial contexts. These latter efforts have provided a valuable starting point but, "Because of their neglect of social variables, these attempts have been particularly ineffective in accounting for the acquisition of novel social responses" (Bandura and Walters, 1963, p. 43). These authors have recently presented a brilliant analysis of the social learning process, which leans heavily on traditional reinforcement theory but accords a much more prominent role to imitative or observational learning and vicarious reinforcement. In a search for principles we turn first to a brief consideration of reinforcement theories of learning and especially the concept of social reinforcement, reserving for later a discussion of the role of imitation.

Reinforcement Theories and the Concept of Social Reinforcement. Social learning and the concept of social reinforcement have generally been treated within the context of "effect theories" of learning. Effect theories stress the instrumental value of responses in producing reinforcement as the primary determinant of learning.

> It seems to be a fact that some stimulus situations are much more effective than others in determining whether or not the responses that lead to them will be learned and performed. Thus, all theorists are forced to assign the empirical law of effect some role in their theories.

> So as not to be burdened with a long list of stimulus situations that can serve as rewards, most experimenters limit themselves to relatively few situations which are known to function as effective rewards. . . . Some theorists have looked for a simple general principle or principles that will allow them to determine whether an event should be listed as a reward or not (Miller, 1959, p. 256).

One attempt at such a principle is Hull's drive reduction hypothesis of reinforcement (1943). Hull postulates that any stimulus-response sequence that is followed by a reduction in strength of a drive will be reinforced. Thus, when the rat sees the lever (in a Skinner box) and depresses it, the food which follows reduces the hunger drive and strengthens the tendency to press the lever the next time it appears.

One difficulty with this formulation is that it is often difficult in controlled situations, and more so in social situations, to realize just what drive reduction results from a particular response. For example, the drive reduction that results from a rat's eating one pellet of food must be essentially nil. In fact, there is evidence that one or two pellets will act as an appetizer, actually increasing drive (Woodworth and Schlosberg, 1955, p. 551).

To account for the reinforcing effect in this and similar situations, Hull evokes the concept of secondary reinforcement. Any stimulus that has been repeatedly associated with a reinforcing state of affairs (drive reduction) will itself acquire reinforcing properties. That neutral stimuli can acquire reinforcing capacity has been demonstrated repeatedly (Cowles, 1937; Saltzman, 1949; Smith, 1939).

In Hull's system, a need is an organic state of deficiency. Needs give rise to drives which can be corrected by appropriate behavior such as eating and drinking. In simple terms, a drive is a *tendency* to behave in a manner that corrects an organic need. Thus, need activates drive, and drive activates behavior. However, animal studies have demonstrated there are needs without corresponding drives (Jukes, 1938; Young, 1941; Young, 1948), and drives for which there are no identifiable needs (Morgan, 1947; Stellar, 1951). This holds true for humans as well. People are not born with the tendency to strive for money, for the discovery of scientific truths, or for symbols of social status and security. Such motives (drives) appear to be learned during socialization.

Many writers have used the concept of secondary or learned drives and rewards. Woodworth (1918), Allport (1937), and Flugel (1948) expressed the belief that drives can sustain themselves in the adult, even though they may have been acquired as means for satisfying organic needs. Davis (1948) indicated that the scholastic inferiority of lower-class children is due partly to a failure to acquire interests that provide motivation and reward for school work. Translating Freud's concept of anxiety into stimulus-response terms, Mowrer (1939) demonstrated that anxiety can motivate trial-and-error behavior and anxiety reduction can reinforce the learning of new habits.

Dollard and Miller (1950) assigned great importance to learned drives and learned reinforcements in the higher mental processes, personality, and psychotherapy. Like Mowrer (1939) they demonstrated that fear can be learned and once learned, it can act as a drive and motivate the learning and performance of new responses in the same way as hunger, thirst, or other drives.

External sources of stimulation (hours of deprivation) and overt responses (pushing a bar to obtain food) can be observed directly. At present, however, learnable drives such as fear cannot be observed directly. On the basis of his studies, Miller (1951, p. 439) advanced two hypotheses: (1) that learned drives, such as fear, obey the same laws as do overt responses; and (2) that they have the same drive and cue properties as strong external stimuli. With regard to the first hypothesis, learnable drives can be called responses, and with regard to the second hypothesis, they can be called stimulus-producing responses.

The above discussion indicates that there exist a host of stimuli that may acquire both drive and reinforcing value. Further, these learned drives and rewards should obey the same laws as primary drives and rewards. The notion that people themselves acquire reinforcing value is a simple extension of this basic notion.

Can human behavior, verbal and gestural, function as reinforcement in interpersonal situations? Common sense would lead us to suspect that it can. If we laugh at a person's joke, chances are good he will tell another. Certain types of behavior (e.g., a gesture, a word, a phrase) certainly appear to be able to reinforce various response patterns. That is, certain kinds of overt human behavior are capable of initiating, maintaining, and extinguishing selected activities in others.

Two tasks confront the psychologist interested in this problem. First he must show that at a qualitative level there exist aspects of interpersonal situations that are capable of "controlling" behavior. Second, he must identify the various behaviors which serve as reinforcing stimuli, isolate them, and develop methods to determine whether they obey the same laws as the primary drives and rewards. The literature indicates that the first task has been accomplished. Unfortunately, the second task appears to lie in the future.

Thorndike (1932, 1933), for example, demonstrated that words spoken verbally could act as rewards. Thus, if a subject's response was followed by the experimenter saying "Right," that response was learned. If, however, the subject's response was followed by the experimenter saying "Wrong," that response was not learned. It is interesting to note that learning occurred without the subjects being aware that they were involved in a learning situation.

The thirty-one studies reviewed by Krasner (1958) demonstrated that verbal stimuli such as "Right," "Good," "Mum-hum," and "Uh-huh," seem to function as reinforcers during verbal conditioning paradigms.

However, can these verbal stimuli be properly called social reinforcers? If so, what drives do they reinforce? Does there exist some need deprivation which is relieved by hearing these words?

Skinner (1957, p. 53) partially answers these questions when he speaks of "generalized conditioned reinforcers."

> Instead of a great variety of reinforcements, each of which is relevant to a given state of deprivation or aversive stimulation, a contingency is arranged between a verbal response and a generalized conditioned reinforcer. Any event which characteristically precedes many different reinforcers can be used as a reinforcer to bring behavior under the control of all appropriate conditions of deprivation and aversive stimulation. A response which is characteristically followed by such a generalized conditioned reinforcer has dynamic properties similar to those which it would have acquired if it has been severally followed by all the specific reinforcers at issue.
>
> A common generalized conditioned reinforcer is 'approval.' It is often difficult to specify its physical dimensions. It may be little more than a nod or a smile on the part of someone who characteristically supplies a variety of reinforcements. Sometimes it has the verbal form: *Right!* or *Good!* Because these 'signs of approval' frequently precede specific reinforcements appropriate to many states of deprivation, the behavior they reinforce is likely to be in strength much of the time.

The notion of approval and its parameters has caught the interest of other psychologists. Hartup (1958), for example, postulates that during the socialization process of children, adult approval (nurturance in their terms) often occurs in close temporal proximity with reduction of a basic need. Because of these associations, a learned drive develops. Thus, a child may become anxious when approval is absent or withdrawn. Approval, then, would reduce feelings of anxiety and be reinforcing. Two groups of nursery school children were compared with respect to the rate at which they learned simple responses which would elicit adult approval. One group was consistently nurtured for ten minutes before the learning situation. The other group was nurtured for five minutes, during which the female experimenter nurtured the children by speaking and playing with them in a friendly and helpful way. They were then rebuffed (nurturance withdrawn) for five minutes, i.e., during nurturance withdrawal, the experimenter withdrew from the child's immediate environment to another part of the room, and interacted with him as little as possible, except to answer his questions in a brief, but friendly manner.

The girls subjected to nurturance-withdrawal showed more efficient performance on learning tasks than those who experienced constant nurturance. The boys, however, did not act as a group. Those who were rated highly dependent by the experimenter during the period of nurturant interaction, were found to respond much as the girls, while low dependent boys responded in the reverse fashion.

Following the same line of thought, Gerwitz and Baer (1958*a*) asked the question: "Does the incidence of behavior for social contact with other people increase following a period of social deprivation?" They were interested in knowing whether social drives function in a manner similar to appetitive drives, the reinforcing stimuli of which acquire maximal reinforcing value for an organism only subsequent to its recent deprivation of them.

Nursery school children made the response of dropping marbles into one of two holes in a "game." It was found that when an adult experimenter made words and phrases like "Good" and "Hm-Hum" contingent upon an arbitrarily chosen response, the response was reinforced (i.e., conditioned). It was found also that the reinforcing effect of "approval" could be increased when the children experienced a twenty minute period of social isolation, relative to its effectiveness for the same children when they had not been isolated. The authors conclude that there are social drives that respond to need deprivation as do the primary appetitive drives.

In a subsequent study, Gerwitz and Baer (1958*b*) were interested in knowing if the behaviors maintained by social reinforcers are responsive also to a condition of relative satiation for such reinforcers. The procedure was the same as the earlier study (Gerwitz and Baer, 1958*a*). In place of the twenty minutes of isolation, however, the children engaged in twenty minutes of free-play activity with the experimenter. During this time the experimenter was friendly toward the child and made verbal and gestural approval responses. Because the children's behavior in the game situation was less influenced by the experimenter following the period of satiation, it was concluded that social reinforcement responds to satiation in a manner similar to the primary appetitive drives.

The authors were well aware that isolation might increase the vigilance of a child, set up conditions of frustration or annoyance, give the impression of reproof or disapproval, and still yield the same results. However, they found that social deprivation increased reliably the reinforcing power of adult approval for the children as a positive function of the degree to which they typically sought approval as judged by nursery school teachers. Further, following satiation, the reinforcing effect of approval increased as a result of the degree to which the children typically sought approval.

Such studies indicate that the relative frequency of an arbitrarily chosen response of an adult or child can be increased by social reinforcement, i.e., her dispensing certain social stimuli (words and phrases) contingent upon that response. In particular, the complex social reinforcer *approval* is readily dispensed through the use of simple words and phrases like "Good," "Fine," and "Mm-hmm." Also it is believed that many complex stimuli provided by the positive social responses of people (e.g., attention, approval, affection) function essentially as positive reinforcers.

Though such studies indicate that social responses can function as reinforcers, they say nothing about the "reward value" of these responses. Kanareff and Lanzetta (1961, 1962) and Lanzetta and Kanareff (1961) have shown that in some learning situations social reinforcers are weak relative to objective indicators of performance adequacy and also that such reinforcers must be linked to important goals before they acquire reinforcing value. These findings suggest that social responses are not generalized conditioned reinforcers of much strength; unless the social agent dispensing the reinforcers is a mediator of other rewards his responses have little influence on the trainee's behavior. Lanzetta and Kanareff have also demonstrated that with adults the effects of social reinforcers of different intensity are dependent on cognitive factors. Presumably through earlier social experiences adults have expectations regarding the appropriateness of reward or punishment for certain classes of behavior (one does not expect lavish praise for performing correctly a simple task nor severe punishment for a minor transgression), and if the amount of reward or punishment exceeds these expectations it may lose its efficacy as a reinforcer.

It is apparent that an understanding of the role of social reinforcers has not yet been achieved. In animal studies, even though theorists disagree as to reasons why reinforcement is effective, the independent variables (e.g., hours of deprivation of primary needs) can be well defined and controlled. However, when the term reinforcement is extended to social situations, many variables are not well defined. Terms such as approval and attention carry meaning at the common-sense level, but actually they refer to very complicated processes which depend on much of a person's life history. At the present state of research we cannot state with any generality the social cues which can serve as reinforcers or the way in which social reinforcers mediate behavior change.

Imitative Learning. Imitation at one time in the history of social psychology was accorded a prominent role as an explanatory principle. Social development was presumed to occur by imitation: children had an innate tendency to mimic those around them and thus gradually acquired social responses characteristic of members of their social group. This simple, sovereign principle did not survive the onslaught against instinct theories, and little was heard about imitation until Miller and Dollard (1941) placed the problem within the context of learning theory. They were able to demonstrate experimentally that imitative responses, far from being instinctual, were learned and generalized across situations and leaders. More recently Bandura and Walters (1963) have presented an analysis of the social learning process which leans heavily on imitation and vicarious rewards as explanatory concepts.

Bandura and Walters distinguish three possible effects of an exposure to a model:

> . . . (1) a modeling effect, involving the transmission of precisely imitative response patterns not previously present in the observer's repertory; (2) an inhibitory or disinhibitory effect, reflected in an increase or decrease in the frequency, latency, or intensity of previously acquired observer responses that are more or less similar to those exhibited by the model; and (3) a possible eliciting effect, in which the observation of a model's responses serves as a cue for 'releasing' similar observer responses that are neither entirely novel nor inhibited as a result of prior learning (Bandura and Walters, 1963, p. 106).

The modeling effect has been demonstrated in a number of experiments by Bandura and his associates (Bandura, 1962). In one study, one group of nursery school children were exposed to an aggressive adult model and a second group to an adult model who displayed inhibited and nonaggressive behavior. In a later test situation with the model absent, the children who had observed the aggressive models displayed a greater number of precisely imitative aggressive responses than children exposed to nonaggressive models. A further study obtained similar results when film models were substituted for real life models. Such results suggest that many of the findings discussed earlier showing a high degree of relationship between parental behavior patterns and children's responses may be reflecting the operation of a modeling principle.

The inhibitory and disinhibitory effect of models is demonstrated in several experiments in which precise matching of a model's behavior is precluded by the nature of the test situation but where responses of the same class as the model's can be exhibited. Lovaas (1961), for example, showed one group of children aggressive cartoons and another group nonaggressive cartoons. In a later play situation the group exposed to the aggressive cartoon gave a greater proportion of aggressive responses (but not aggressive as exhibited in the cartoon). Siegel (1956) and Mussen and Rutherford (1961) have obtained similar results with free-play aggression and verbal aggressive responses as the dependent variable. The model, in all such studies, appears to have had a disinhibitory effect—aggressive responses already well-learned had a higher probability of being emitted after exposure to the model.

As Bandura and Walters have noted, other forms of deviant behavior also appear to be disinhibited after exposure to a model who displays the behavior. Kimbrell and Blake (1958) and Lefkowitz, Blake, and Mouton (1955) have shown, for example, that observers are more likely to violate prohibitions when they see these violated by others, and Bandura and Walters (1959) have reported that adolescent boys are more likely to engage in sexual intercourse during multiple dating than when alone with a girl.

The role of the model as an eliciting stimulus is difficult to distinguish from disinhibition. The distinction must rest on a judgment of the extent of inhibition of the response due to prior learning. A model can be said to disinhibit responses only when the response is one that is normally inhibited, whereas an elicitation can be said to occur whenever the observer exhibits behavior similar to the model's after observation of the behavior. As several experiments have demonstrated, one can readily elicit volunteering behavior or the size of a monetary contribution if appropriate models are provided (Shachter and Hall, 1952; Rosenbaum, 1956) and in animal groups releasing phenomena are common (Thorpe, 1956).

Such modeling effects are enhanced if the model is rewarded and inhibited if the model is punished. In addition, the magnitude of the effects are dependent on both characteristics of the observer and the model. Persons lacking in self-esteem (deCharms and Rosenbaum, 1960), who are incompetent (Kanareff and Lanzetta, 1960), or have been previously rewarded for imitating (Lanzetta and Kanareff, 1959) are more likely to imitate a successful model. In addition, when observers believe themselves to be more similar to a model they show more matching behavior (Stotland and Dunn, 1963). Imitation is enhanced, also, when the model is of higher status (Lefkowitz, Blake, and Mouton, 1955), has previously been successful (Lanzetta and Kanareff, 1958), or when he socially reinforces imitation of his responses (Lanzetta and Kanareff, 1961).

The above studies, in addition to demonstrating that imitation plays an important role in the learning and performance of social patterns of behavior, show that such response patterns are acquired in large segments rather than through a slow gradual process. Following exposure to a real or symbolic model, the learner reproduces more or less the complete response pattern; thus, learning is rapid and efficient.

One can more readily conceive of the complex repertory of behavior patterns characteristic of an adult being acquired, if imitation plays a prominent role than if conditioning and differential reinforcement alone were the mechanism responsible for social learning.

COGNITIVE STRUCTURE AND CHANGE: COGNITIVE CONSISTENCY THEORIES

Through social learning an individual develops characteristic ways of perceiving and reacting to his social world. Though relatively general, such modes of reaction are not insensitive to the momentary social situations in which the individual finds himself and are not so pervasive that they provide a ready basis for action in all social contexts. To cope adequately with the social world one must be continuously sensitive to the requirements of specific social situations. In addition, as noted earlier, socialization involves learning to discriminate between conditions under which one or another form of behavior is appropriate and habits of attending to and judging one's environment are as strongly reinforced as the appropriate behavior itself.

Whatever the reasons, there is little doubt that we are continuously engaged in perceiving, judging, and interpreting both social and nonsocial events. Some of these perceptions and judgments are transient, serving the purpose of guiding short-term actions in a specific situation, e.g., maneuvering our way through a crowded hotel lobby to a waiting friend. Others, either because of their emotional salience, their recurrence, or their implications for long-term programs of behavior, become organized and synthesized to varying extents, forming the content of what we call beliefs, attitudes, and values. Such organized and consistent patterns of perceiving and judging and the feelings (or emotions) and reaction tendencies with which they become associated affect our social behavior to an important extent.

The obvious importance for an understanding of social behavior, of systems of perceiving, thinking, and feeling, early directed the attention of social psychologists to an understanding of the structure, function, inter-relatedness and laws of change of such systems. The search took place along many fronts—the study of beliefs, attitudes, social judgments, perception of people, social communication, interpersonal feelings—each of which came to take on a shape and focus of its own. Many facts were accumulated, but they remained poorly organized and articulated, both within and across areas. Recently, however, some attempts at order have emerged—hesitantly, tentatively, but growing in power and scope. The order has emerged from what is generally termed "cognitive consistency theories"—in reality a set of theories which have a family resemblance. Though such theories have in no measure as yet integrated or attempted to integrate the mass of specific findings, they have pointed to communalities between previously unrelated phenomena and have been of great heuristic value in suggesting hypotheses and directing the search for new data.

The theories encompassing as they do a wide range of perceptual and cognitive phenomena hold promise of providing the basis for the unifying theory so sorely needed in social psychology. For this reason they are one of the most important developments in social psychology in decades and warrant special attention. In what follows we review briefly the theories as well as some samples of the data the theories are designed to explain.

PERCEPTION OF COGNITIVELY BALANCED UNITS

The earliest systematic statement of a consistency theory is by Heider (1944, 1946, 1958). Drawing from Gestalt principles of organization of the perceptual field of nonsocial objects, Heider (1944) postulated that proximity, similarity, common fate, good continuation, set and past experience may serve as organizing factors in social perception. Groups of elements which are organized according to these principles are referred to by Heider as perceptual units. Having formed a perceptual unit of a social phenomenon, persons tend to attribute a change in the social field to a causal relationship within the perceptual unit.

Through experience, the child learns to recognize causal relationships between doer and deed. It is possibly because such relationships are frequently characterized by physical similarity and proximity of cause and effect that one tends to attribute other changes in his environment to the will of similar or nearby inanimate objects. To the extent that these cues are the only ones available for organizing one's perceptual field, his attributions of causality are necessarily spurious in many instances. In any event, the habit of making such false attributions is not easily broken. Heider and Simmel (1944) experimentally demonstrated the persistence of this habit of perceiving that causal relationships exist within perceptual units. When a motion picture of moving geometrical forms was shown, certain patterns of movement were perceived as a chaotic or random motion. However, patterns of movement which suggested personal characteristics caused subjects to perceive the actions of the figures in terms of motives and sentiments. When this was the case, a unified structure appeared:

> . . . the movements are organized in terms of acts of persons. It is obvious that this organization has many advantages from the point of view of . . . the adaptation of the organism to the environment. The changes, when identified with a constant figural unit, no longer follow each other in an arbitrary and unconnected way. They are connected with invariable characteristics of the environment, they are meaningfully embedded in our picture of reality . . . the interpretation of movements is intimately connected with the interpretation of invariances . . . (Heider, 1958, p. 32).

In Heider's terminology, a "causal unit" is a perceived causal connection between an object and an effect. The salience of Gestalt principles is especially obvious in the formation of causal units which affect inferences people make about one another. Similarity and proximity, which are factors in figural unit formation (Wertheimer, 1939), appear to be major factors in causal unit formation. Inferences about others are made on the basis of events occurring to them or surrounding them, as though their will were the sole cause of the events; there is a disregard for the other less tangible and less proximal factors which could have brought about the change. If an act and a person are similar to one another, one is likely to be perceived as being the cause of the other (e.g., poor performance attributed to unpopular people). The strength of the similarity between the perceived actor and the act may vary, however, and the amount of discrepancy is one of the factors which determines the strength of the belief that the causal relationship exists.

The nature of the causal attribution which one infers from a stimulus configuration affects the meaning or cognitive structuring of the environment. The cognitive structure, in turn, determines the type of affect which accompanies perception of this stimulus configuration, e.g., feelings of success or failure are experienced only when the source of the achievement or failure to achieve is perceived to be located in one's own person. A morale lifting device is the attribution of failure to a source other than one's self.

Certain enduring tendencies or needs which exist within individuals can also affect origin attribution, e.g., the need to keep the ego level high may bring about an appropriate reconstruction of the environment with consequent rationalization (e.g., scapegoating behavior). More generally, causal attribution may be consistent with attempts to maintain a stable self-concept. While causal attribution is not entirely determined by the stimulus configuration, certain stimulus objects and effects form such strong perceptual units that they suggest a stronger causal connection than that which is motivated by attempts to maintain a well-integrated self. Thus, conflict may arise between cognition of a strong causal unit and one's own personal needs.

Heider enlarges upon his theory of perceived causality by demonstrating the way in which causal units affect attitude formation and change (1946). A causal unit is considered to be unbalanced when doer and deed are sufficiently dissimilar that one would not usually be expected to follow from the other. An example of an unbalanced causal unit would be: hearing an admired friend state that he favors a proposed law which you violently oppose. Such states of cognitive imbalance are postulated to be psychologically uncomfortable and give rise to pressures toward resolution of the imbalance. The imbalance resulting from a dissimilarity may bring about the formation of a more congruent causal unit (e.g., the friend was joking; the law is really quite good, etc.). However, in the event that the given unstable unit is the most satisfactory one available, its degree of balance may be increased somewhat by changing one's evaluations of the similarity between the agent or the action (e.g., the friend is not such a good friend; the law is not as bad as it first seemed).

Kerrick has obtained empirical support for the prediction that attitude change follows recognition of an unbalanced causal unit and that such a change results in greater perceived similarity between agent and action. Osgood (1960) pretested Ss' attitudes toward a newspaper and toward an issue which was subsequently discussed in an editorial in that paper. Following the appearance of the editorial, there was no net change in attitude toward the issue in the entire population of Ss, but those who favored the newspaper became more favorable toward the issue supported by the paper, and those disliking the paper became more opposed to the issue.

An extension of Heider's theory of balanced units includes configurations of stimuli in which inferred relationships are not necessarily causal (e.g., a balanced unit may consist of two persons joined by a relationship of mutual liking). A balanced state is said to exist when attitude (e.g., the positive or negative relationship of a person to another person or object) is made consistent within a unit. Balanced states exist, for example, when one's cognitions about a given object are all of a positive or all of a negative nature (X likes, trusts, admires, respects Y) and when a unit consists of several objects among which all positive relationships and negative relationships are of a mutual nature and no objects which are similarly regarded by another have a different regard for one another (e.g., X and Y like one another, but both dislike Z, who dislikes X and Y).

Balanced states are hypothesized to be psychologically more comfortable than unbalanced ones. Jordan (1953) obtained experimental evidence in support of this hypothesis. Sixty-four Ss were given hypothetical situations of varying balance to be rated for unpleasantness. Unbalanced and negative situations were most frequently rated unpleasant.

A knowledge of the nature of elements in one part of a perceived configuration, and knowledge of the relationship between some of these elements tend to cause one to infer the type of relationship and the nature of the components in other parts of the configuration, such that the entire configuration is perceived to be balanced, e.g., if A likes B and A also likes C, A tends to assume that B likes C. If a balanced state does not exist, forces toward that state will arise. Either the affective characteristics of the relationships will be changed or the unit relations will be changed through action or through cognitive reorganization. If a change is not possible, the state of imbalance will continue to produce tension (Heider, 1958).

Logically, of course, one positive relationship between X and Y does not imply another. In fact, X's relationship with Y may be positive in some respects and negative in others. But, when one is made aware of his dual relationship, a tendency exists to make the different relationships similar, in an evaluative sense, by means of cognitive restructuring. Experimental confirmation of this tendency has been obtained by Asch (1946) and Ichheiser (1949) in their finding that people tend to overestimate the consistency of personality. Many people find it difficult to conceive of good and bad personality attributes coexisting in the same person. In descriptions of persons' salient personality characteristics, people generally tend to use adjectives all of which are of a similar evaluative polarity.

DESIRE FOR BALANCE AS A MOTIVATOR OF COMMUNICATION Newcomb (1953) uses Heider's principle of balanced states to conceive of communicative acts as aroused by imbalance, and resulting in changes somewhere within the system of relations between two or more communicators. From Heider's concept of "tendency toward balance," Newcomb developed the notion of "strain toward symmetry." Strain toward symmetry is manifested in the social phenomenon of A and B tending to have attitudes of the same sign toward a common C. In a situation such as the one illustrated in Fig. 14-1, A can (1) try to influence B to dislike C, (2) change his own evaluations of C, (3) change his own evaluations of B, or (4) tolerate the tension. In general, nonsymmetrical relations A-B-C elicit a tendency to communicate about C and to increase or decrease cohesiveness of the group relationship.

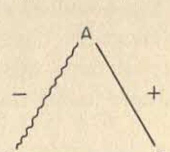


Fig. 14-1. Example of Unbalanced Structure

Newcomb uses the "strain toward symmetry" model to evaluate such social phenomena as tendencies to communicate, interpersonal attraction, tendencies to reject deviates, and pressures toward uniformity of opinion among members of a group.

Newcomb's use of this formulation differs sharply from that of Heider in one respect; he has stepped outside of the individual perceiving organism and refers instead to the relationship between persons in a social situation. This approach to the study of interpersonal behavior requires that the experimenter assume the existence of the specified cognitive organizations within each of the persons in the triad.

Newcomb's theoretical approach was used in a study by Horowitz, Lyons, and Perlmutter (1959) which supported the hypothesis that Ss tend to resolve cognitive imbalances in groups. Ss evaluated one another sociometrically before a group discussion. After the discussion, Ss were asked to evaluate an event which was connected with one other S. The experiment indicated that relationships between two persons and a concept tend toward a balanced state, e.g., if X and Y like one another, they will tend to similarly evaluate an event which they have discussed. Similarly, Tagiuri, Blake, and Bruner (1953) found that an increase in congruency in dyads usually consisted of more than a change in perception; there was usually a concurrent change in feeling.

PRINCIPLE OF CONGRUITY The various models of cognitive balance stress that awareness of cognitive inconsistency produces psychological tension, which may then be alleviated by cognitive change. Furthermore, there seems to be general agreement that the magnitude of tension increases with the degree of inconsistency. Osgood and Tannenbaum (1955) have stated these assumptions in more rigorous terms within the framework of their principle of congruity, and they have made possible the testing of these assumptions through the use of a technique called the semantic differential.

The principle of congruity holds that when cognitive change occurs, it occurs in the direction of increased congruity with the prevailing frame of references. In Heiderian terms, this means that attitude changes are always made in the direction of greater cognitive balance. The principle of congruity predicts changes in attitude given any degree of difference between the interacting cognitions, whereas cognitive balance theorists have, heretofore, dealt only with cognitive changes occurring as a result of interactions of grossly different cognitions. The previous crudity of tools of measurement would have precluded testing of the principle of congruity; however, the semantic differential has made possible a series of more refined experiments in cognitive change.

Assuming that specific cognitive states of human language users are antecedent conditions for selective encoding into language, Osgood, Suci, and Tannenbaum (1957) empirically derived three major dimensions of connotative meaning: evaluation, potency, and activity. In order to evaluate the connotation of a word, meaning is differentiated on a set of seven-point bipolar adjectival scales for each of the three dimensions. For example, the word "father" may thus be rated on three connotative dimensions:

Father

	−3	−2	−1	0	+1	+2	+3	
good	—	—	—	—	—	—	—	bad
hard	—	—	—	—	—	—	—	soft
slow	—	—	—	—	—	—	—	fast

By analysis of such data, the meaning of words can be located in three-dimensional space. A concept's distance from zero on any connotative scale represents its degree of polarization. For purposes of studying cognitive change usually only the evaluative scale is used.

Let us look now at Osgood and Tannenbaum's theory and research as it applies to their principle of congruity.

Osgood (1960) states that the production of cognitive change requires more than the presence of incongruous elements: One must be made aware of inconsistency, and must, in addition, dislike cognitively imbalanced situations before cognitive change will result from cognitive incongruity. Osgood postulates that tolerance of inconsistency probably increases with education and intelligence, and decreases with heightened emotion. In any event, given the incidence of cognitive inconsistency and the desire to resolve it, one may change the sign or the existence of the relationship or change the connotation of the cognition in such a way that it becomes more consistent with other cognitions in the relationship. The latter may be accomplished by adding new supporting elements which reduce the total imbalance or by eliminating the old incongruous element.

Osgood and Tannenbaum's research on cognitive behavior has dealt primarily with the production of change in the connotation of cognitive elements rather than changes in relations. This represents a drastic departure from the work of Heider (1958), Newcomb (1953) and Cartwright and Harary (1956) who were concerned only with the negative or positive nature of relations between elements. Osgood and Tannenbaum recognized that the extent to which a cluster of related cognitions was balanced depended, in addition to the other factors previously studied, upon the degree of congruence between related cognitions, i.e., upon the degree of positive or negative polarity of cognitions. One positively regarded element may be associatively related to another positively regarded element and still incur incongruity and cognitive change if the elements are not equally polarized. An outcome of their research has been methodology and theory which accounts for changes in degree of polarity. Hence, this model can predict more subtle changes in attitude than its predecessors.

Osgood and Tannenbaum assume that the drive for cognitive modification increases with degree of cognitive inconsistency. Using the semantic differential, degree of strain toward symmetry is equal to the difference between the initial scale position of a cognitive element on the semantic differential (prior to interaction) and its location of perfect congruence. Perfect congruence between two elements occurs when there is equal polarization (intensity), either in the same evaluative direction (for associative assertions) or in the opposite evaluative direction (for dissociative assertions). Thus, if the perceived polarity of Kennedy is +3, and of Nehru, +1, and Kennedy backs Nehru without reservation, the pressure toward congruity is 2 units. If Mao = −3, and Nehru strongly and unreservedly opposes him, there is a similar incongruity of 2 units.

Assuming that cognitive inconsistency is handled by change in the meaning of cognitive elements (as opposed to, say, change in perceived relationship between the elements, or repression of the inconsistency), Osgood and Tannenbaum (1955) offer the following formula for prediction of relative degree of change occurring in interacting cognitive elements in attempts to produce congruity:

$$mc_1 = \frac{p_2}{(p_1) + (p_2)} P$$

where mc_1 = meaning change of a cognitive element
P = total pressure toward congruity
p_1 = polarization of cognition$_1$
p_2 = polarization of cognition$_2$

Thus, in the statement, "Kennedy (+3) gives wholehearted support to Nehru (+1)," one would predict ¾ of the attitude change to be with regard to Nehru and ¼ of the attitude change to be with regard to Kennedy.

The predictive value of this formula is reduced, however, insofar as it does not handle changes in relations. When tremendous incongruity exists between elements, congruity is often gained by disbelieving the stated relationship. To account for this, a "correction for incredulity" is made; as the magnitude of P (total pressure toward congruity) increases from 3 to 6, the correction factor increases in size.

Empirically, these formulas have been shown to predict with accuracy the direction and degree of attitude change following induction into a situation of cognitive incongruity. As predicted, Tannenbaum (1953) found that when positive sources make positively valued statements or negative sources make negatively valued statements, little attitude change takes place. However, when positive sources make negatively valued statements, the source becomes less valued and the idea more valued; when negatively valued sources make positively valued statements, the source becomes more valued and the idea less valued.

Other experiments by Osgood similarly illustrate the nature of changes predictable by the principle of congruity, and they are of special interest because they are not typical attitude change studies. Two such studies are described for illustrative purposes:

In a "word mixture" experiment, Ss rated the meaning of eight adjectives and eight nouns. The sixty-four possible combinations of the adjectives and nouns were then rated for meaning, using eight groups of Ss to complete the design. The results indicated that the meanings of the combinations can be quite accurately predicted from the measured meanings of the components: correlations between predicted and obtained meanings were .86, .88, and .90 for evaluation, potency, and activity factors respectively (Osgood, Suci, and Tannenbaum, 1957). In another experiment, five faces were rated on pleasantness and activation. When the Ss then viewed all possible combinations in a stereoscope, the meanings of the focused expressions were shown to be highly predictable by the congruity formula, and, as predicted, the meaning profiles of the fused pictures regularly correlated more highly with those of the more polarized component (Osgood, 1960).

Osgood and Tannenbaum (1955) mention another characteristic of cognitive activity which seems related to the tendency to seek congruity. It seems to be cognitively easier to assume that related concepts are identical in some respects than to maintain fine distinctions between them, so that differences between concepts located in the same direction of the evaluative framework tend to be eliminated. An example of the tendency of similar cognitions to increase in perceived similarity has been demonstrated by Hovland, Harvey and Sherif (1957) in their study of "assimilation and contrast" effects in reaction to communication of information contrary to one's opinion. When the distance between one's own stand and the position advocated in communication is small, the communicator is judged to be taking a stand closer to the listener's than is actually the case. However, when one's own stand diverges widely from the position advocated, the communication is perceived as further removed from one's own state than it is.

In addition, Osgood and Tannenbaum have initiated some work based upon the hypothesis that there is continuing pressure toward cognitive simplicity. There is some evidence to indicate that over time there is continuing pressure toward cognitive simplicity; groups of similar cognitions not only tend to merge together in evaluative connotation, but also move, as a group, toward a more polar position over time. As cognitions move to more highly polarized positions, the latency of judgments based upon them tends to become shorter. Presumably, the meanings involved in such judgments become extreme and unambiguous and lose much of their complexity in the process of polarization, with the results that decisions based upon these meanings involve little conflict, few alternative choices, and little ambiguity. (Osgood and Tannenbaum, 1955.)

BALANCE OF COGNITIVE AND AFFECTIVE ATTITUDINAL COMPONENTS Concepts and attitudes are composed ot more than semantic components. Taking this into account, Rosenberg approached the study of cognitive balance by making a distinction between cognitive and affective components of attitudes. His major premise was that people seek congruence between their beliefs and feelings toward an object. Rosenberg (1960*b*) produced inconsistencies between the affective and cognitive components of attitude through the use of posthypnotic suggestion of affect reversal. Hypnotized subjects showed extensive changes in their cognitive structure consistent with newly held affect. These changes persisted for the duration of the reversed affect. Cognitive reorganization generally occurred in such a way as to assure maximum balance among all the relevant cognitions, involving integration and reorganization of a very wide area of cognitive activity. Thus, the stability of an attitude is, in part, determined by the degree of consonance between its cognitive and affective components.

Other relevant factors in assessment of stability of attitude are the degree to which the cognitive components are believed, the expected satisfaction from the values involved, and the perceived importance of the attitude object in leading to or blocking the attainment of valued outcomes. While a one-question poll assessing only one of these factors may indicate that a large majority favors a given alternative, a depth interview which assesses a number of these components may reveal a serious discrepancy between affect and cognition. If this is the case, the attitude is not stable and may be changed by dissemination of information which will relieve the unstable state (Rosenberg, 1960*a*).

Various instruments have been developed for measuring cognitive structure and attitude change (Rosenberg, 1960*a*, 1956; Carlson, 1956). A measure of cognitive structure which is quite simple and surprisingly sensitive is a Q sort of value statements which may be sorted on the basis of various criteria, e.g., instrumentality in attaining one's goals, degree of belief, degree of affect with which the value is held, etc. In studies by Carlson (1956) and others, comparison of premanipulation and postmanipulation Q sorts indicated actual changes in the ordering of values as predicted by cognitive balance theory.

COGNITIVE DISSONANCE Strivings for self-consistency affect the way in which people structure their world; acts, opinions and attitudes tend to form in internally consistent clusters (Heider, 1958). The pressure of an imbalance causes psychological discomfort and brings about efforts to reconstruct the cognition to yield better balance. Thus people tend to act in ways consistent with their cognitions. However, Festinger (1957) has shown that the converse is also true: people try to make their cognitions consistent with the way in which they have acted. The knowledge of having acted in a way which would not be expected to follow from one's cognitions causes a cognitive imbalance and an accompanying uncomfortable state which Festinger calls cognitive dissonance. Dissonance then serves as a drive state which motivates the acquisition of dissonance reducing cognitions.

Dissonance occurs following a decision in which one or more of the unchosen alternatives contain desirable elements not contained in the chosen alternative. All those elements which, considered alone, would lead to action other than that taken, are dissonant with the cognition corresponding to the action taken. For example, if one desires to have a car with a radio, convertible top, automatic transmission, and power steering, but there is only a choice of either a convertible with radio or a car with automatic transmission and power steering, then having chosen the convertible, the choice of this car would be dissonant with the desire for an automatic transmission and heated. This dissonance or incongruity between cognitions and actions may be reduced by increasing one's belief in the worth of heaters and convertible tops, and lowering one's opinion of the nonchosen objects.

Festinger states that the magnitude of cognitive dissonance between two clusters of cognitions is a function of the proportion of all relevant relations between two clusters that are dissonant, and of the importance of the elements involved in that relation. The strength of pressure to reduce dissonance is a function of the magnitude of the dissonance. The magnitude of dissonance cannot exceed the total resistance to change of the least resistant element.

Festinger makes a careful distinction between conflict and dissonance: one is in conflict before making a decision; after making the decision he is no longer simultaneously drawn toward two or more choices, as he is committed to the chosen alternative and must now reduce the dissonance resulting from favorable aspects of the nonchosen response.

Precisely what kinds of behavior cause cognitive dissonance? Festinger's general hypothesis is that persons induced to engage in behavior which is discrepant with some or all of their cognitions will undergo cognitive change such that their behavior seems more justified. This hypothesis has received support from studies by Kelman (1953), Janis and King (1954, 1956), Brehm (1956), King and Janis (1956), and others. In general, having made a decision which causes dissonance to occur, persons tend to display an increase in their valuation of the chosen alternative and may engage in selective information gathering in order to justify this change. A classic example is provided by Brehm's study (1956) in which he asked women to rate eight appliances for desirability. Ss were then allowed to choose between two of the appliances and, after an interpolated activity, were asked to again rate the appliances for desirability. Half of the Ss were given a choice between appliances which they rated in a similar manner, and half between products on which ratings differed widely. As predicted, in postdecisional ratings, the chosen alternatives showed an increase in desirability and the rejected alternatives showed a decrease. In cases where the two alternatives had received similar ratings, there was a greater increase in perceived desirability of the chosen alternative (pressure to reduce dissonance varies directly with the extent of the dissonance).

Knowledge of the kinds of situations which can cause dissonance and bring about subsequent pressure for cognitive change has been expanded and refined by subsequent research. Festinger originally stated that one must act and have no intention of revoking the action before dissonance can occur. Rabbie, Brehm, and Cohen (1959), however, found action to be unnecessary for the production of dissonance and cognitive change: the production of dissonance and subsequent attitude change depends not on actual engagement in discrepant behavior, but simply upon the decision to act. Brehm and Cohen (1959*a*) and Brehm (1959) found that chance events can bring about dissonance and cognitive change, provided the person had a prior choice in commitment to the event. Thus, a person who chooses an alternative which is subsequently changed or affected by fate will still show increasingly more favorable postdecisional evaluations of his chosen alternative, provided the other conditions required for the production of dissonance are present.

The magnitude of the dissonance which is induced has been found to vary systematically as a function of the ratio of the number of cognitions from which the decision would not be expected to follow to the total number of relevant cognitions. It follows from this formula that the greater the amount of effort one expends in carrying out a chosen alternative, the less reason there is to have chosen it, hence the higher will be the dissonance ratio; as dissonance increases, there will result a similar increase in degree to which the chosen alternative and the accompanying work will be valued. Cohen (1959) and Cohen, Terry, and Jones (1959) demonstrated the effects of effort upon production of cognitive dissonance. Ss were told to exert little or much effort in comprehending some material which opposed their views. Ss who had exerted little effort reacted to the material in much the same manner found in other studies of attempted influence; there was a contrast and assimilation effect such that attitudes changed to agree with the new information only when there was a small discrepancy between the information and the initial opinion. However, the opposite effect was observed for Ss who exerted much effort; as dissonance theory would predict, attitude change *increased* as the discrepancy increased between the information and the opinions initially held.

Another way to increase the ratio of dissonant to consonant elements is to increase the value and/or number of unchosen alternatives. Brehm and Cohen (1959*b*) found that the magnitude of dissonance is a function of the number of rejected choice alternatives and their qualitative dissimilarity.

Another type of behavior which has been explained by dissonance theory is that which follows forced compliance. It follows from dissonance theory that the more reason one has to comply against his will, the lower will be his dissonance to consonance ratio, hence the less his subsequent cognitive change. Studies such as Cohen's (1960) have demonstrated that, in general, things done for large reward have less persuasive power than things done for small reward. Brehm (1959) found that children who were rewarded for eating disliked vegetables showed greater increased liking after eating them when their reward had been small. Festinger and Carlsmith (1959) paid individuals $1 or $20 for telling others that a boring task was interesting; in a subsequent measure of their actual enjoyment, the Ss receiving less pay rated the task significantly more enjoyable. The same principle holds true for situations of negative incentive; the more unpleasant the initiation ceremony required by an organization, the more one is attracted to the group (Aronson and Mills, 1959).

The increased liking for an act, as is manifested under conditions of forced compliance with little reward, may be sufficient cause to continue performing that act. This inference has led Festinger (1961) to examine further the behavior which is emitted under conditions of insufficient reward. Psychologists have, at times, found it difficult to discover the reinforcing agents which, presumably, must be present in order for a behavior to persist. Contrary to reinforcement theory, Festinger postulates that conditions of no reward or insufficient reward may actually cause a form of behavior to persist. In an experiment with Yaryan (Festinger, 1961), Festinger found that persons required to do much studying in preparation for a test estimated that the likelihood of their taking the test was greater than did persons who were not required to make such preparation, although all of the Ss knew that it had been predetermined who was to receive the test. Festinger cites similar instances of the use of this backward logic, in which persons seem to attempt to justify amount of effort invested in a task for which there is little reward by increasing their liking for the task.

If dissonance acts as a drive, as Festinger postulated, then, following a decision, persons should tend to seek information that favors the chosen alternative and avoid information that favors the rejected alternative. Furthermore, the more important the decision, the greater should be the subsequent selectivity in exposure to information. These hypotheses have been verified in a number of studies (Mills, Aronson and Robinson, 1959; Festinger, 1957). This tendency has also been found to affect patterns of interpersonal relationships. In order to selectively expose oneself to information, certain patterns of social interaction must be developed and others avoided; e.g., persons experiencing cognitive dissonance tend to seek out others who are experiencing the same dissonance in an attempt to obtain support for new cognitions consonant with their belief system (Festinger, 1957).

Various factors may hinder the types of dissonance reduction behaviors described above. The behavior which would be consonant with one's cognitions may not be within one's behavioral repertory. Behavior which is consonant with one group of cognitions may be dissonant with other cognitions such that cognitive balance cannot be achieved by any manipulation of available behaviors and cognitions. In such cases, information may be gathered and/or interpreted to support some rather strange and unrealistic cognitions. Though social reality may be at odds with one's actions, this is one of the less formidable blocks to cognitive balance; one's own needs may motivate drastic restructuring of the social field to achieve balance (Festinger, 1957; Heider, 1944, 1958). When one is unable to select information which justifies his behavior, dissonance may still be reduced by overemphasizing those factors which favor one's decision and by establishing cognitive overlap among the alternatives involved in the choice. Presumably, such dissonance reducing processes can account for the frequent incidence of increased confidence in one's choice following decision and for the increase in perceived discrepancy in attractiveness between the alternatives involved. Through the use of these mechanisms, decisions which should be revoked in the light of new evidence may be retained and continue to appear justified in the eyes of the decision maker. Thus, the theory of cognitive dissonance would predict some extreme and nonobvious events, which have in fact been found to occur. For example, Festinger, Riecken, and Schachter (1956), studied a fanatical group who were planning their escape from earth in order to avoid a predicted major flood. They claimed that their proposed migration was directed by someone from outer space, and invited others to join their group and be saved. When the flood did not materialize at the predicted hour, the members of this group became still more vociferous about their beliefs. They continued to interact with one another, proselytizing about the event among themselves and others. Similarly, Brehm (1960) obtained empirical support for his hypothesis that persons forced to engage in an unpleasant activity would increase their liking for the activity as a function of the amount of information received from others which berated the activity; berating increases the ratio of dissonant to consonant elements.

Dissonance reduction activities appear to be carried out according to a principle of "cognitive economy." In an experiment by Rosenberg and Abelson (1960) in which *Ss* role-played the position of a person involved in a dissonant situation, it was found that they not only achieved cognitive balance with minimum effort, but also achieved a balance whose content implied a maximization of expected gain.

If dissonance reduction activities usually follow the onset of dissonance, as research seems to indicate, such activities must play a major role in our everyday lives. Dissonance is often impossible to avoid, as people do not have complete control over the information which reaches them. Furthermore, information is rarely black and white; most complex cognitions carry with them some contradictory and imbalanced implications. Thus, in any complex cluster of cognitions, some degree of dissonance exists. Man's ability to act as a rational assimilator of impinging stimuli is surely diminished to the extent that he is motivated to reduce dissonance. While dissonance is a postdecisional phenomenon, it is obvious that it affects subsequent choice behavior in situations related to that in which the dissonance exists, and it may, in this sense, motivate irrational decisions. Perhaps dissonance reduction activities may be considered adaptive in the sense that they make persons more satisfied with their choices. However, must favorable unchosen alternatives always give rise to psychological discomfort? Festinger makes some concluding comments upon these questions, which are discussed later in this paper.

MODES OF RESOLUTION OF BELIEF DILEMMAS

Balance theory, as elaborated so far, has stated some of the circumstances under which imbalance may motivate cognitive change. However, since there are several modes of imbalance resolution, accurate prediction of overt behavior is difficult. Following the line of thinking of most cognitive balance theorists, Abelson (1959) describes ways of resolving cognitive imbalance, then proceeds to postulate a preferred hierarchy of solutions.

Abelson presents a theoretical analysis of the ways in which one may restructure his cognitive representation of external objects when the objects simultaneously incur favorable and unfavorable cognitions. He postulates that two beliefs can arouse similar or opposite feelings, and that the bond that joins them can be either associative or dissociative. Thus, cognitive elements may assume various configurations of balance and imbalance:

	balanced	unbalanced
dissociation - - - -	+ —— +	+ - - - - +
association ——	+ - - - - −	+ —— −
	− —— −	− - - - - −

An unbalanced dyad constitutes a belief dilemma when it is often present in thought. Preserving the identity of the cognitive elements, Abelson describes four modes of resolution of the belief dilemma:

1. Denial—denying the value of one of the elements or of the connecting bond.

2. Bolstering—relating one or the other of the two cognitive elements in a balanced way to other valued objects.

3. Differentiation—splitting an element into two opposed parts; and identifying with the part which is consonant with one part of the dyad.

4. Transcendence—considering the opposing parts of the dyad as essential components of some higher purpose.

Abelson postulates a specific hierarchy of resolution attempts based on ease of achieving success:

1. Deny the relevance of one of the conflicting components. If unsuccessful:

2. When two elements have unequal strength, bolster the more intensely affected one. This is especially useful if one of the elements has strong social sanction.

3. If the two elements are nearly equal and bolstering doesn't work, reverse the denial process, using the new information gleaned from step 2 to deny the other conflicting component.

4. If search for information with which to bolster an alternative offers a variety of information, it may lead to differentiation. Once differentiation has taken place, old and new parts must be kept far apart by some rationale.

5. Transcendent resolutions are used only for chronically insolvable problems, but once invoked, they may solve many problems.

The relevance of Abelson's work to the theory of cognitive dissonance has not, to date, been fully realized. Any of the four resolution methods may be used to decrease dissonance; however, only the methods of denial and bolstering would lead to the type of selective exposure to information which Festinger predicts. The maintenance of cognitions which involve differentiation and transcendence probably also requires some selective exposure to information, but this exposure would not be limited to information favoring one's choice in a simple manner. Rather, a far more complex perception of the information would be taking place, e.g., one may choose to see others happily enjoying the fruits of an opposite choice to one's own, but this perception would be accompanied by cognitions such as "That's all right for him, but I am different because . . ." There may, in addition, be emphasis on the perception of situations which prove the ways in which these differences manifest themselves.

Perhaps Abelson's hierarchy represents a developmental sequence of cognitive activity as well as a sequence of attempts at imbalance reduction. Differentiation and transcendence, once used, may serve as enduring techniques of living with imbalance and may preclude some of the usual preliminary efforts at denial and bolstering. These somewhat more sophisticated methods of handling imbalance may partially account for the large individual differences in the extent to which persons are motivated to reduce cognitive imbalance. Some explorations of the various facets of cognitive balancing behavior, in terms of Abelson's proposed hierarchy, may provide considerably greater insight into the processes involved.

Large individual differences have been observed in the extent to which individuals utilize various strategies to attain cognitive balance. Some theories of personality and cognitive style have been of heuristic value in examining these differences.

Festinger (1957) for example, has suggested that the concept of degree of tolerance of ambiguity is relevant to the analysis of individual differences in dissonance reduction behavior: Persons with low tolerance for ambiguity probably tend to experience relatively great discomfort in the presence of dissonance since they prefer to perceive issues in terms of "black and white" and consequently exhibit more vigorous dissonance reduction behavior and are most prone to devise means of avoiding its occurrence. Such individuals would avoid making decisions. Paradoxically, they would be very definite and dogmatic about their attitudes. Other mechanisms which they may use to avoid dissonance include: making decisions without a feeling of commitment (i.e., assuming a passive role relative to one's environment), not considering oneself responsible for decision, and subjective revocation of decisions as soon as they are made although they may, in actuality, be irrevocable.

The literature on cognitive style (e.g., Pettigrew, 1958; Gardner, Holzman, Klein, Linton and Spence, 1959, Harvey, Hunt and Schroder, 1961) strongly supports the notion that large individual differences exist in degree of integration and differentiation of cognitive structures. It has been demonstrated, for example, that some persons are habitually able to consider a concept and its source separately (e.g., can reject an idea without disliking the person who suggested it) while others, as balance theory predicts, hold concepts which are strongly colored by their opinions of the proponents of those concepts. Differences also exist in amount of information which is incorporated in concepts, and the complexity of the intercorrelations perceived to exist among related concepts (e.g., concepts may be perceived as different from one another in some respects and similar in others, or as perfectly intercorrelated on all perceived criteria). Persons who form simple and highly polarized concepts based upon information received from their environment would, when required to integrate these concepts, need to perform some extraordinary feats of cognitive change before they are able to act in terms of their integration of information with any degree of psychological comfort. On the other hand, persons whose concepts tend to be quite complex and more congruent with reality in the first place are probably faced with fewer imbalances and may, in addition, be less bothered by imbalance when it does occur.

Polarized concepts, as determined by use of the semantic differential, are verbalized with shorter latencies (Osgood, Suci and Tannenbaum, 1957). This seems consistent with Festinger's conjecture that quick and confident decisions are given by people who cannot tolerate ambiguity and therefore maintain "black or white", i.e., highly polarized, cognitive clusters. However, the correlation between degree of polarization and intolerance of ambiguity remains to be demonstrated. Such relationships, when existent, are often far more complex than anticipated. For example, Suci (1952) compared the semantic differential factor structures of high and low ethnocentrics; his hypothesis that high ethnocentrics rely more heavily on the evaluative factors was not supported. There was, however, a fusion of the potency and activity factors into a single dynamism factor for high ethnocentrics.

Osgood, Suci and Tannenbaum (1957) have some evidence to indicate that extreme polarization and simplification on concepts is characteristic of emotional persons and persons with low intelligence and little education. This finding, if valid, has serious implications for prediction of differences in behavior, as it implies great differences among persons in the types of cognitive material with which they have to work and to attempt to balance.

Kerrick (1954) has found some support for the hypothesis that persons of low intelligence are more susceptible to pressures toward congruity, cognitive simplification and attitude change. Tannenbaum (1958), however, found that susceptibility to attitude change varies inversely with the intensity with which original attitudes are held. There appears to be a contradiction in the three statements: extreme attitudes mitigate against attitude change; low intelligence persons have more extreme judgments and simpler cognitions; and low intelligence persons show a greater degree of attitude change. Other factors must be considered, however. The formula $MC = p_2/(p_1 + p_2)$ would predict that an intense attitude tends not to change. However, in the event that all attitudes (e.g., p_1 and p_2) are extreme, as with low intelligence persons, the formula predicts facilitation, not inhibition of attitude change. Furthermore, the characteristics of simple cognitions must be considered. Complex cognitions are usually replete with knowledge of the conditions under which they are true and of the relationships between them and other cognitions; with such knowledge, seeming incongruity can often be handled by differentiation or transcendence when conditionality is an aspect of one's concepts. A simple cognition, however, lacks such qualifying conditions, and incongruity between two such cognitions would be viewed as a case of one cognition completely refuting the other, a situation which would call for some re-evaluation of the cognitions involved (i.e., attitude change).

Are specific modes of resolution of cognitive imbalance a result of selective reinforcement and subject to the laws of learning? If so, can they be modified by similar means? How broadly does one generalize from this learning?

There is evidence to indicate that enduring ways of handling cognitive conflict are related to early conflict experiences. The research of Abelson and Lesser (1959) indicates that positively reinforced instances of agreement or negatively reinforced instances of disagreement tend to increase the ease with which cognitive change can be induced; firmness of parental control correlates positively with persuasibility. The greater the frequency of control attempts by parents, the higher will be the relationship between firmness of control and persuasibility. Parental acceptance operates to increase persuasibility and correlates positively with child persuasibility. In young children, persuasibility tendencies toward parents generalize widely to other communicators.

Degree of persuasability has been found to correlate with various measures of personality. Persuasability and self-esteem are negatively correlated (Janis and Field, 1959). Field dependence correlates positively with persuasibility; persons whose opinions are changed by a communicator are more affected by the interfering aspects of the stimulus field than are nonchangers (Linton and Graham, 1959). The same investigators found a significant correlation between authoritarianism and persuasibility on all the subscales of the Authoritarian Personality questionnaire which Adorno et al. (1950) consider to reflect personality patterns predisposing the individual toward authoritarianism. However, the two subscales reflecting social attitudes (politico-economic, conservative, and ethnocentric) are not correlated with persuasibility. Persons tend to select as friends those who are similar to themselves in degree of persuasibility, another demonstration of the ubiquity of cognitive balance requirements in dictating the nature of social interaction.

Drawing from the theories and data of the authors reviewed, one could conjecture that reinforcement of agreement has a polarizing effect upon cognitive clusters. If, as Abelson theorizes, reinforcement increases the probability that an individual will concur cognitively with another communicator, it is implied that the tendency to entertain alternate cognitions and to perceive the equivocal implications of some cognitions is diminished. The resulting highly polarized cognitions would tend to contrast rather than assimilate with other partially incongruent information, yielding attitudinal instability. The absence of fine, graduated distinctions between related cognitions and lack of recognition of certain initial inherent imbalances makes it impossible to assimilate new, partially contradictory cognitions into the fabric of attitude—hence polarized attitudes produce more polarized attitudes.

Longitudinal studies of cognitive change may offer more coherent information about the relationship between individual and situational variables. Developmental studies of cognitive organization and change may be profitably directed by the question, "What developmental sequences of cognitive activity are general to all children and which are related to specific modes of child rearing?" Cognitive complexity has been shown to be a function of age (Wolf, 1961). Increase in age is not, however, a sufficient cause for increase in cognitive complexity. The developmental and environmental factors upon which cognitive styles are dependent, and the resulting effects upon cognitive balance and change behaviors remain relatively unexplored.

SEEKING OF COGNITIVE BALANCE OR IMBALANCE

The frequent seeking of cognitive imbalance presents a serious limitation to the generality of cognitive balance theory. Is it not strange that a man whose information handling behavior is ostensibly governed by a desire for cognitive balance should choose to attend a magic show? In literature, the good novel often relates the experiences of one whose major characteristic is the maintenance of an imbalance: e.g., one who hates something he has created. Statements of unbalanced relationships often have a paradoxical and profound ring to them, as when Oscar Wilde writes, "Yet each man kills the thing he loves . . ." The tragedy is, in fact, a play which ends in psychological imbalance. In literature, as in many other facets of life, balance often seems boring and self-evident; imbalance stimulates further thinking.

Does there exist a clear-cut dichotomy of circumstances under which cognitive balance is and is not sought? There seems to be a qualitative difference between the interestingly imbalanced situations just described and the circumstances under which cognitive balance seeking behavior was elicited in studies cited earlier. The circumstances under which Ss were seen to seek cognitive balance concerned for the most part cognitions of great personal relevance, e.g., attitudes, cognitions of actions previously taken by one's self. Are cognitions of less personal relevance subject to similar cognitive processes?

A study by Bruner and Potter (1962) provides evidence to indicate that cognitive dissonance can be aroused in rather objective impersonal activities. A picture identification task was used in which an out-of-focus slide was projected on a screen. Over a two-minute period of time, the slide was progressively brought into better focus. Ss were to emit hypotheses as to the subject of the picture, throughout the viewing. Those who gave hypotheses earliest took longer to correctly identify the picture if their first hypothesis was wrong than did Ss who waited some time before emitting their first hypothesis. Presumably, having committed themselves, they tried to fit the subsequently gained information to suit their hypothesis and to disconfirm other conflicting (nonmentioned) hypotheses.

Osgood and Tannenbaum's demonstration that phrase connotation represents a compromise between the location in semantic space of the component words, and that fusion of separate pictures viewed stereoscopically obeys the principle of congruity is a further demonstration of motivation to create cognitive balance, given an opportunity to personally create order in one's own environment.

On the basis of existing evidence, a plausible conjecture may be that situations involving cognitive imbalance are interesting and pleasurable when they involve someone else, or an aspect of one's own life in which action need not be taken on the basis of the unbalanced cognition, or in cases in which one's self-esteem is not affected by the action taken. Thus, idle curiosity may lead one to seek imbalanced situations which would not be sought in the course of performing a serious job.

If one's responses to the cognitive imbalance are not made publicly or do not require one to take action, there may be no drive to reduce perceived conflict. But, it may be that when one has been sufficiently threatened in past conflict-handling situations, his defensive activities will carry over to his covert perception of cognitive conflict. Much research remains to be done in an attempt to test these speculative hypotheses.

SOCIAL INTERACTION

Thus far, we have dealt with two determinants of social behavior; the early learning experiences which result in the acquisition of a basic repertory of responses to social stimuli, and the short-term environmental forces which shape attitudes and responses in transient social situations according to principles of cognitive balance. We turn now to a discussion of some selected topics on social interaction, a class of social environments which require behavioral adjustments of reasonably long-term nature, yet have not necessarily the molding influence of socialization. We shall see a high degree of overlap between these three topics. Because of differential socialization, persons bring differing response tendencies to groups—e.g., persons may vary in terms of aggressiveness, affiliativeness, need for achievement, cognitive complexity, etc., and the nature of the social interaction will vary as a function of such personality characteristics of group members. In addition to these enduring individual differences, there exist in groups many examples of cognitive balance phenomena; individuals find themselves in conflict situations and are forced to make judgments about others, to choose to affiliate with one group and not with another, to determine social causality, or yield to the persuasive efforts of others.

An analysis of social interaction, however, requires more than a knowledge of the socialization history of the group members and an understanding of transient adjustment processes. Social groups evolve specific structures and serve special functions, in which the members play specific roles and in turn shape the behaviors of other members. This section cannot handle a full discussion of all aspects of group structure and function that have been examined by social psychologists, but an attempt will be made to describe some of the major areas of empirical research and their relevance for a theory of social interaction.

GROUP NORMS Early concepts of the group process reveal a great deal of confusion concerning the mechanisms through which the individuals in a group interact with and influence one another, creating in the process a group which appears to have characteristics which are more than a summation of the component parts. Between 1850 and 1930, social scientists proposed various solutions to the problem of analysis of this "group mind;" solutions which are largely of academic interest today were the source of heated controversy in the nineteenth century. Some of the concepts generated to explicate the way groups come to feel and think included the following: (1) Collective consciousness is the notion that the subconscious mental life of all members is pooled among all members. This concept is exemplified in the "racial unconscious" proposed by Jung (1922). (2) The objective mind theory holds that one universal mind evolves and spreads through all men's minds in the course of history. Hegel (1807) envisioned such a mind as growing and developing through individuals but not to be reducible to the sum of their mental lives. Similarly, but without Hegel's mysticism, Espinas

(1879) theorized that a collective consciousness existed because of rapid verbal and nonverbal communication among group members, such that each individual helps create a social consciousness that is capable of knowing itself. (3) Analogical doctrines liken society to an organism. The fact that groups perform differentiated classes of functions with some degree of specialization among its members led some theorists to "explain" these mechanisms by analogy, comparing the human group to the human body. Plato rendered such a model in which the ruling class was analogous to the head, the warriors analogous to the breast, and the slave class analogous to the abdomen. Similarly, Hobbes (1651) conceived of the great Leviathan or commonwealth which was an artificial man, greater in stature and strength than any individual, but somehow composed of individuals.

Though differing in important respects these early theories were agreed in viewing the unified interrelationship among members of a group as a basic entity in and of itself, emanating from a group mind and being impervious to further analysis. Since the early 1900s, in contrast to the "group mind" formulations, social scientists have favored more analytic conceptual schemes: the apparent unity of activities and attitudes of groups are seen as stemming from the existence of agreed upon rules of behavior or norms. Norms specify which behaviors are proper and which are improper in specific situations and further specify the rewards for adherence and punishment for nonadherence. The well-coordinated differentiation of function within groups (which originally led to the Leviathan analogy of human groups) was reinterpreted in terms of role differentiation; while some norms specify the behavior of all members of the group, other norms specify the behavior of specific members of the group who occupy specific positions within the group, i.e., they specify role behavior.

Redefining the sociologists' concept of norms by adding more traditionally psychological variables, Sherif formulated the problem of social norms in terms of perceptual processes: norms represent shared frames of reference for perceiving things (Newcomb, 1963). Developing the psychological formulation still further, Newcomb (1963) suggested that those shared mental processes are the result of mutual social reinforcement for shared ideas. Norm building and communication are circularly related to one another; norms arise through communication, and communication is possible through norms. Norms and communication skills develop together, since children must learn society's shared ways of perceiving things in order to communicate with others, and thereby they gain the rewards of communication.

Similarly, Newcomb conceived of roles as the agreed-upon ways of discriminating between the functions of group members. Each role is defined in terms of other roles and can only be understood in that network. Roles facilitate communication, making it possible to talk about and with others. A broader class of cognitions which are found to be shared among group members are attitudes, persistent readinesses to perceive, think and feel about a thing in ways which lead to certain shared conclusions. The distinctive quality of a group is its shared attitudes, norms, and roles. A group defines the individuals in it in these terms and gives to these individuals frames of reference about things including themselves. Newcomb argues that it is this group sharing of meanings which, in part, has been mistaken for the "group mind."

Thibaut and Kelly (1959) have theorized about norms at a still more psychological level; they have examined the way in which individuals provide for their own needs in a social environment. Thus, going beyond the analytic level which describes merely the effects of norms (e.g., shared frames of reference, unified group action), Thibaut and Kelly have attempted to delineate the processes involved in the development and functioning of norms at an individual level. They begin with certain basic principles from learning theory, namely, that certain behavioral tendencies are elicited in individual members of a group by drives and incentives, that individuals will engage in various behaviors until reinforcement is obtained, and that reinforced behavior persists. In addition, they point out that certain additional factors must be taken into consideration when analyzing the learning of behavior in a social environment: persons often obtain reinforcement from some behaviors of other persons and are negatively reinforced by certain other behaviors. Thus it may be that interacting individuals cannot achieve reinforcement simultaneously, as when one member wishes to converse and the other would prefer to go sailing. If the individuals in such an interactions situation are to receive reinforcement, each must additionally engage in a behavior which is not reinforcing for him. Such "trading" may be worked out through the exercise of power, e.g., one may promise to go sailing with the other after they have spent a while conversing. However, the moment-to-moment use of power is not necessary if the group members can agree on some rule for trading. Such a rule would include an agreed-upon signal for trading, and after shifts have been effected smoothly and equitably from one activity to the other, this arrangement is likely to become sufficiently rewarding for each that the trading pattern will persist.

When such an agreement exists between group members and is accepted to some degree by each, and when social processes exist which produce adherence to these agreements, a norm is said to exist.

This would manifest itself to an outside observer in several ways:

> 1. There would be regularity in behavior . . .
> 2. In the event of disruption of this regularity, the "injured" person would attempt to restore it by appealing, at least initially, to the rule and he would exercise his personal power as an enforcer of the rule.
> 3. The person disrupting the regularity would be likely to feel some obligation to adhere to the agreement and might even exhibit some conflict or guilt about deviating from it, as if he were punishing himself for his nonconformity (Thibaut and Kelly, 1959, p. 128).

The norm exercises power over each group member in the sense of making it more desirable for him to behave in certain ways at specified times rather than advance his personal interests. To the extent that the norm is accepted by individuals to whom it applies, conformity is more rewarding than nonconformity. In large groups, the trading arrangement may not be equally acceptable to all members, but deviants are requested to comply for the sake of the group.

Enforcement of conformity to norms becomes more important as the size and complexity of the social group increases (Hare, 1952), since with increase in group size, there is usually also (1) an increase in probability of interferences with reinforcement seeking of individuals, (2) an increased difficulty of reaching agreement as a means of resolving conflicts, and (3) a need for division of labor which further necessitates smooth coordination of behavior. Thus, one important function of a viable group is the effective enforcement of norms. Norm enforcement, or "norm sending" requires that the norm sender have fate control over the norm receiver. That is, the norm sender must be in a position to (1) state the rule as to the desired behavior and enumerate the consequences of adherence and nonadherence, (2) maintain surveillance over the norm receiver to see whether his behavior meets the normative criterion, and (3) apply sanctions to produce the consequences stated with the rule. Various means of norm sending may be used. In order to maintain group cohesiveness, the application of negative sanctions is sometimes delivered by persons who are not members of the group. These outside agents may even be imaginary ones (e.g., bogeymen, witches, "laws" of nature). The effectiveness of such imaginary agents depends on the strength of the beliefs perpetuated by the group regarding the punishing power of these agents.

Ultimate development away from control by other in-group members (an ultimate socialization of the individual group members) occurs when norms are internalized—i.e., when individuals cue themselves to appropriate behavioral rules and apply self-surveillance and self-delivery of sanctions.

CONFORMITY Some of the same norm sending and receiving variables which account for the way in which individuals interact to create the unitary nature of groups can, conversely, be used to describe how groups create conformity within individual members.

A considerable body of research attests to the importance of primary and reference group membership variables for an adequate understanding of conforming or nonconforming behavior (Cartwright and Zander, 1953; Hovland, Janis, and Kelly, 1953). A typical statement of several basic hypotheses is made by Kelly: Conformity of group members to some behavioral standard results from

> . . . a complex social process which includes teaching them about the norms, motivating them to conform, and, in various situations, providing reminders of the norm and the associated sanctions. The adequacy with which these various aspects of the process are performed within a group will determine the degree of conformity exhibited by its members. It will also determine the degree to which members resist persuasive communications

> which propose non-conformity, especially those from outside the group (Hovland, Janis, and Kelly, 1953, p. 136).

In terms of the individual, conformity will presumably depend on his awareness of the expected behavior or attitude, and his motivation to conform to the standard.

> If the individual knows the norms to which conformity is expected, he will exhibit conforming behavior in any given situation to the extent that:
>
> First, he is dependent upon the group for the satisfaction of various motives, and this forms the basis for *motivation to conform.*
>
> Second, there are social stimuli or *situational cues* which operate to arouse the motive related to conformity (Hovland, Janis, and Kelly, 1953).

The term group in this formulation is used in a generic sense and refers to any aggregate of individuals with certain distinguishable modes of behaving and certain shared attitudes and beliefs. The individuals need not be physically proximal nor in continuous communication. The concept of reference group has come into use to denote both the membership and nonmembership groups which have an effect on a person's opinions.

Although there is evidence to support the position that the pressures to conform will exist when the group is not physically present (Sherif, 1948), it appears probable that the degree of pressure to conform varies as a function of the extent to which cues, reminding one of membership, are present (Merton and Lazarsfeld, 1950). Such cues presumably are important because they elicit the motives supporting conformity. Evidence based on attitude responses of combat infantrymen indicates that factors associated with primary group membership (members physically present) provided the major source of motivation for behavioral and attitudinal conformity in combat, rather than identification with more remote reference groups.

Invariably, an individual will be found to identify with more than one group. The influence of multiple-group membership on conformity has not been extensively explored, but it seems likely that influence attempts would be more successful if potential conflicts between the norms of various reference groups were exploited. For example, typical indoctrination, interrogation, and other attempts at counter-norm influence can be viewed as attempts to capitalize on conflicts engendered by multiple-group membership, and to exploit confusion over the apparent conflicts in the goals of the groups. In recent prisoner-of-war indoctrination, attempts have been made to increase the POW's identification with groups who might more commonly be thought to share certain goals and values with them (labor groups), and to decrease identification with groups whose values could be made to seem antithetical to their own (capitalistic groups).

From this interpretive framework two factors appear critical in determining the degree of conformity to be expected (or conversely, the degree of resistance to counter-norm influence attempts): the characteristics of the norm and the motivation to conform.

The term "norm" refers to standards shared among members of a group and represents the behavior and attitudes they expect of one another. A standard presumably incorporates both stimulus and response aspects: under certain conditions particular modes of response are appropriate; under other conditions different responses are stipulated.

Standards at one extreme may be unambiguous and cover all possible contingencies; at the other extreme, they may be vague principles providing general guides to action in limited situations. Some norms may require discrimination of subtle cues in terms of which finely differentiated responses are made, e.g., norms guiding the behavior of a headwaiter in a restaurant; others may require gross categorization of situations with relatively simple response choices, e.g., standards in some groups regarding behavior toward Negroes in public places. Presumably the degree of ambiguity of norms is one determinant of their effectiveness in guiding behavior, although research evidence bearing on this problem is scanty.

There is slightly more evidence bearing on the relevance of the importance of the norm to the group as a factor influencing conformity. "The perceived importance of any given norm will probably depend upon the extent to which it is emphasized and discussed within the group, the degree to which surveillance is maintained over the relevant behavior, and the quantity and quality of direct incentives used to produce conformity . . . One of the most usual cues as to the importance of conformity on a given issue is the degree of unanimity among the members" (Hovland, Janis, and Kelly, 1952, p. 168).

Several experiments indicate that the conditions under which the norm is acquired are also important. There have been a number of studies which have shown that if an entire group makes a decision about some change in behavior, this agreed-upon behavior will occur even when the members are away from the group (Cartwright and Zander, 1953). Group decisions appear to be more effective in changing behavior than lectures or group discussions without any decisions, but it is not clear as to why. One study would suggest that the important factors are the arrival at a decision and the degree of group consensus (*ibid.*, p. 145), factors which probably enhance the perceived importance of the norm.

When somewhat new situations arise, there may be an attempt to abstract certain principles of behavior from old norms and behave in terms of these principles in the new situation. Since, however, susceptibility to change is greatest when attitudes are based on abstraction rather than concrete past experience, group conformity may be difficult to maintain in new situations.

Again taking prisoner-of-war behavior as an example, clear, unambiguous norms regarding reactions to various indoctrination attempts, behavior in interrogation sessions, responses to compliance on the part of fellow POWs, etc., are often lacking. The POW is faced with a number of questions: Should peace petitions be signed? Should one write autobiographies? Was it permissible to talk with captors? Should special favors be denied, or accepted and shared with other POWs? Should a "too-cooperative" ranking officer be ousted? Should a collaborator be immediately and harshly dealt with? Should there be attempts at organization of compound groups? What was permissible behavior under a variety of other imposed conditions? Standards have been observed to develop in the face of pressures, but they were divergent from group to group and within various groups, and the general lack of unanimity detracted from their acceptance and contributed to the formation of splinter groups.

Presumably a variety of derived motives are involved in attitudinal and behavioral conformity.

> Some of them have to do with maintaining social approval and avoiding disapproval, the importance of which the individual learns during the early stages of his socialization. Others have to do with the person's desire to understand his fellows, to be understood by them and to share their solutions to recurrent problems. A related basis of conformity motives is suggested by the feelings of security, the sense of 'rightness,' which seems to come from holding opinions similar to other persons (Hovland, Janis, and Kelly, 1952, p. 137).

In general, we may assume that the degree of conformity of an individual will depend upon his expectations of being rewarded for conformity and of being punished for nonconformity. The punishments and rewards under the control of the group will primarily be their social reactions to the individual's behavior, although as Thrasher points out, more severe forms of punishment sometimes are used: "Opinion in the gang manifests its pressure in the variety of methods through which group control is exerted, such as applause, preferment, and hero-worshipping as well as ridicule, scorn, and ostracism . . . the member who has broken the code may be subjected to a beating or in extreme cases may be marked for death" (Cartwright and Zander, 1953, p. 138).

Whether the primary basis for the control is positive attraction to the group or negative reinforcement may have an important bearing on the degree to which a norm will become internalized. There are some findings consistent with the hypothesis that there is a direct relationship between positive attachment to a group and internalization of its norms (Hovland, Janis, and Kelly, 1953, p. 147). On the other hand, psychoanalytic literature would suggest that internalization may be very effective when punishment is employed because of the phenomenon of identification with the aggressor. In general, there is little evidence on which to make a choice since relatively little is known about the processes underlying internalization in adolescent and adult groups.

If, as suggested, the primary form of control is the social reaction of the group members, the degree to which they can exert influence over a member would depend on the attractiveness of the group for him. If a member is not highly motivated to remain in the group, their negative or positive reactions to his behavior would presumably lose some of their reinforcement value.

The term "cohesiveness" is often used to describe the degree of attraction a group has for its members. The cohesiveness of a group depends both on properties of the group—its goals, program, size, type of organization—and the needs of the individuals—needs for affiliation, recognition, security, and other things which can be mediated by groups. The attractiveness of a group for an individual depends upon the nature and strength of his needs and upon the perceived suitability of the group for satisfying these needs. Following from this, we would expect the attractiveness of a group to increase if it were possible to heighten the awareness of members that their needs could be fulfilled by belonging to the group.

Conversely, decreasing the perceived degree of satisfaction to be obtained through membership should decrease the attractiveness of the group. A decrease in valence of the group may occur if members disagree over ways to solve common problems (French, 1941), if a member feels others are likely to disagree with his opinions (Festinger et al., 1952), if the group makes demands on the individual that he feels incapable of fulfilling (Horowitz, 1953), if the group fails to achieve its goals (Festinger et al., 1952), or if certain members are unduly dominant or self-oriented (Fouriezos, Hutt, and Guetzkow, 1950).

Obviously, if norm sending and receiving mechanisms provide for creation of the unified processes which comprise group behavior, many other aspects of group processes may be examined in terms of their viability as mechanisms for norm sending. Of course, norms are not the only important focus in the study of group processes, however, since they are such an essential aspect, they do provide a link between many areas in social psychology, as can be seen in the following discussion of social perception and group composition.

SOCIAL PERCEPTION In the course of ongoing group activities in which there is a degree of coordination of activities, it would appear desirable for group members to be able to predict the behaviors and intentions of their co-workers on the basis of minimal cues, hence facilitating the surveillance, communication and "trading" processes essential to smooth group functioning. Jones and Thibaut (1958) hypothesize that the form of social perception which occurs in a given situation is determined by the existing social structure and the interaction goals; persons perceive what is relevant to the situation in which they find themselves.

Situations may vary in terms of the degree to which person perception is instrumental in determining one's own behavior, and in terms of the types of information one needs about others, i.e., the qualitative and quantitative interpersonal needs differ with the situation. Across the quantitative dimensions, Jones and Thibaut identify three types of interaction situations: (1) Noncontingent situations are ones in which standard procedures for behavior (norms) are utilized to the exclusion of feedback about other persons, e.g., formalized role systems or ceremonies. (2) Asymmetrical contingent situations are ones in which the behavior of only one of the persons is dependently determined by the behavior of the other; the other has a "standard" behavior and need not monitor his partner's behavior in order to plan his own, e.g., interviewer and respondent. (3) Reciprocal contingency situations refer to those fluid social situations in which each person must respond in terms of the other's behavior and therefore needs to monitor the other simultaneously to his own responding.

The following are classes of interaction goals and their characteristic requirements for interpersonal information:

1. Facilitation of personal goal attainment. Persons need to know what the other person is doing to them and how it will affect their success. This arouses a need to maintain one's own well-being which is fulfilled by seeking: (*a*) information which makes one conscious of the other's openness, expertise, and objectivity, (*b*) motivational or value support, which makes one conscious of the other's agreeableness, (*c*) beneficent social responses, making one conscious of the other's cues of liking, (*d*) help in accomplishment of a goal, making one conscious of the other's compliance, competence, and dependability.

2. Analysis of determinants of personality. With this goal the aim is primarily to understand the other's motives. The perceiver attempts to see the other person in terms of that person's own need system, whereas the aim of social perception in facilitation of personal goal attainment is to see the other person in terms of one's own need system. When analyzing the determinants of personality, the perceiver seeks any information which helps him to understand relationships between types of behavior emitted by the perceived person.

3. Application of social sanctions. The observer

is interested in changing the other's behavior in order to make it appropriate to the norms. This arouses a set to determine whether each of the emitted behaviors meets the criteria set by the norms.

Jones and Thibaut illustrate the relevance of this conceptualization by describing the differing perceptions that are entertained by persons who are attending a court trial: The judge tries to determine whether the defendant has violated norms. The psychiatrist tries to understand the defendant's motives. A person in the court audience may be trying to decide whether he would like to be acquainted with the defendant for the facilitation of personal goals.

In addition, there are varying levels of perception and the course of social interaction depends upon both the degree of accuracy of perception at a given level and the number of levels at which perception occurs. Goffman (1959) argues that there are at least two levels at which emotion may be judged: the level of emotion which the person consciously projects for others to perceive, and the level of emotion which he is really experiencing. Goffman points out that judgment of the expressed emotion is most useful in maintenance of social interaction since it helps the observed to continue the social facade he is trying to maintain. On the other hand, the evaluation of persons is aided by an ability to see beneath this facade and to assess their real emotions.

Certain facades of emotion or personality are projected by individuals in the course of interaction either in an engineered attempt to give an impression that will evoke desired responses from others, or because group norms require this kind of expression. An individual's initial projection of his personality tends to be accepted and to lay the lines for the attribution of meaning by others. Additions onto one's projected image are socially effective only if they are congruent with the original projection—incongruent information halts interaction and is embarrassing. The actor responds to such breakdowns by attempting to resurrect his old image. Observers respond to the breakdown either by becoming hostile or by tactfully expressing confidence in the old image. In addition to disrupting the social interaction, such breakdowns can bring about changes in others' perceptions of the person and of the institution he represents. These disruptions can even bring about changes in the person's self-concept. However, while the perceiver may try to help the actor maintain his projected self-image, the perceiver simultaneously covertly seeks to discover subtle cues (such as response latency) which negate the projected facade.

Implicit in Goffman's formulation is the hint that the projected facade is probably that which is most congruent with the prevailing norms, and any incongruity between the projected facade and the person's real emotions is probably a function of the unsatisfactoriness of the norms to the individual. This, then, raises some question as to whether accurate perception of emotions and motives at both levels would really facilitate interaction. Steiner (1955) theorizes that a high degree of accuracy leads to smoother interpersonal relations only under conditions in which persons are able to react freely to the new information by developing a closer dyadic relationship as perception becomes more accurate. This condition is often not met: role expectations may require ignoring of individual qualities, reaction to which would possibly disrupt the whole system. Where roles are clearly defined, they permit efficient behavior in the absence of accurate perception of real emotions. Thus, accuracy of perception perhaps enhances only the performance of groups which have no prescribed roles.

Fiedler (1958) has obtained some data which bear indirectly on Steiner's hypothesis. The efficiency of social groups was studied as a function of the extent of perceived differentiation among co-workers by leaders. It was originally hypothesized that the degree to which leaders assumed small differences in dissimilarity of attitudes between their most and least-liked co-workers connoted acceptance and would therefore conduce to high efficiency. He found, however, that good natural leaders typically perceived high dissimilarity among members on basketball teams, surveying teams, and factory crews. These data suggested that leaders who typically perceive low dissimilarity among members of their group may develop much psychological involvement with even their poorest workers and hence permit subordination of work relations to personal relations. However, there are limitations to the generalization that the best leaders are those who perceive large differences between the attitudes of their most- and least-liked co-workers. In a study of corporations, Fiedler found that successful companies were those having general managers who perceived great differences between their best and worst co-workers (a critical, differentiated attitude), but also having board directors who perceived small differences between their best and worst co-workers (i.e., who were warm and accepting of their fellow workers). Presumably, these directors were best at eliciting ideas from the board. This combination of general manager and board chairman was profitable for the company, however, only when the manager was endorsed by the board chairman. Since the board's attitudes and ideas had to be channeled to the manager before he could administer them, it was necessary to have rapport between the board and the manager. Thus, in this situation, it was not only necessary for leaders to maintain the amount of psychological distance and differentiation appropriate to their roles, but, in addition, good communication had to exist between these efficient units so that they could coordinate their various roles.

Thus accuracy, or at least degree of differentiation, is related to group efficiency in a complex way. What, then, determines whether one perceives others accurately? A great deal of research has been directed at this question, originally in the hope that it would yield a large payoff for fields such as personnel assessment, group psychology, and teaching. Much of this research up to 1954 has been summarized by Taft (1955). Although correlations between personal qualities and accurate judgment are very low and inconsistent, some trends emerged from these studies: good judging ability increased with age, IQ, specialization in the physical sciences, aesthetic interests, insight into one's own status, social detachment and independence. There seemed to be a syndrome of social detachment which characterized good judges. Such judges did not err by being too lenient. The leniency error is salient in our culture, but it is not found in oriental studies. It tends to increase with degree of acquaintance with the observed person, and it is part of a general class of errors which stem from set or expectancy. Better judges have usually been found to be harder to judge. This may be due to their greater complexity; their implicit theory of personality may be comprehensive and flexible enough to account for a wide variety of other personalities, but if this "theory" also characterizes their own personality, they will be too difficult to be judged by persons with less complex "theories."

Gage and Cronbach (1955), and Cronbach (1958) have pointed out a number of ways in which faulty conceptualization of types of interperson perception situations have necessarily resulted in the contradictory findings reported in the literature. For example, the literature does not differentiate between situations in which persons are tested for ability to draw on past knowledge of others, to observe present behaviors of others, or to make inferences on the basis of observations. In addition, a variety of judgmental skills may be required, depending upon the object of perception. Persons may be required to judge, for example, how persons in general act, how specific groups act, how individuals deviate from typical behavior, and how an individual will act on a specific occasion. Cronbach and Gage argue that one's ability to predict within one category may not generalize to other categories.

The study of social perception has moved more and more away from an emphasis on accuracy per se to attempts at understanding the underlying processes. A very fruitful line of research has been directed at the question of what determines the type and number of cues which are perceived in social interaction.

Jones (1954) has studied the effect of some learned needs on perception; he gave carefully controlled information to high and low authoritarians and found many differences in responses: low authoritarians reponded to personal qualities while high authoritarians responded to status cues, differentiated less among leaders, and perceived nonauthoritarian persons as undependable and hard to understand. Presumably, high authoritarians have found status cues more important that personal qualities.

Taguiri, Bruner, and Blake (1953) have studied the relationship between affect and perception of affect among members of small groups. It was found that the affect of one member for another strongly influenced his ability to perceive the affect of the other. In addition some negative affective relationships were found to be harder to perceive than positive ones. Specifically, Taguiri, Bruner, and Blake assessed the extent to which persons sociometrically chose others as a function of whether they thought the other chose them, and whether the other actually chose them. The degree to which sociometric choice patterns deviated from chance was assessed by comparing the data to a model derived through a technique known as relation analysis, devised by Taguiri, Bruner, and Kogan (1955). An examination of instances in which person X rejected Y indicated that Y was only 27 per cent accurate in guessing that he had been rejected, whereas persons who were chosen guessed that they had been chosen with 43 per cent accuracy. There are three possible reasons for this: (1) cues of rejection are usually hidden by politeness; (2) persons generally have inadequate training in the recognitions of rejection cues since rejectors usually fail to interact over a long period of time; and (3) rejection is often tantamount to degradation, hence the perception of rejection may be inhibited. There are data to support the third reason and to which is contrasted the perception of acceptance: persons who gain in sociometric status are sometimes aware of gain, but persons who decline in sociometric status are not aware of the increased rejection (Taguiri, 1958).

Persons were found to have a very high tendency to perceive others as having the same affective tendencies toward them as they had toward the others (congruency), when in actuality, mutual choice or rejection did not exceed chance level in most of the groups studies. This tendency is probably largely responsible for the relatively great accuracy (76 per cent) with which people can guess the affect of others when the relationship is mutual. When mutuality does not really exist, a person's ability to guess the nature of the other's affect is correct only 23 per cent of the time, significantly below chance level guessing. In cases of accurate guessing of mutual liking and disliking, it is difficult to learn whether this is due to true accuracy of perception of others or to congruency, since mutuality and congruency automatically lead to accuracy with no skill on the perceiver's part. These aspects of perception are logically interrelated, i.e., congruency and accuracy are always accompanied by mutuality, and mutuality and accuracy are always accompanied by congruency. Therefore, it is impossible to completely assess the relative contribution of congruency and perceptual skill in the correct judgment of mutual relationships. The relative contribution of congruency and skill may be inferred from the finding that congruency occurs far more often (70 per cent) than accuracy (46 per cent), and as reported above, that perceptual accuracy with nonmutuality is below chance accuracy. In cases of real nonmutuality, the affect which A thinks B has for A is the same as A's affect for B significantly more often than chance, and it is the same as B's real affect significantly less often than chance. However, when mutuality exists, the determiner of perceptual accuracy is difficult to pinpoint. When the mutuality is one of liking, there are very many cues which make the liking apparent, so that even outsiders can easily identify such relationships (Taguiri, 1958). The high visibility of mutual liking behavior is attributed to the fact that persons who feel reciprocated will manifest their choices more openly. The actual process of reciprocation involves not only choice of each for the other, but the formation of a perceptual unit in the sense that the persons tend to move about together, spend time together, agree with one another, and give things to one another.

As can be inferred by the topics discussed thus far, social perception is a most complex phenomenon which cuts across such diverse areas of psychology as learning, motivation, cognition, and systems research. Research to date in this area has barely scratched the surface in relating accuracy and type of social perception to viability of interacting groups of people. It is apparent, however, that the way in which group members perceive one another significantly affects the nature of the group's subsequent interaction and, conversely, that the nature of the interaction will affect perception—another example of the complexity of internal relationships which characterize group processes.

We turn now to a closely related area of group interaction, the study of the relationship of group composition to group behavior.

GROUP COMPOSITION[1] A number of reasons have been advanced favoring rational composition of groups. First, it has been observed that the inability of group members to get along may prevent the group from carrying out its required function. Presumably, group composition on the basis of certain sets of personality characteristics would reduce the incidence of disruptive conflicts. Relatedly, group composition may be expected to affect communication: to the extent that members are selected who share the same meanings, norms can be developed rapidly and interaction can proceed smoothly. Shared values constitute a third hypothetical source of composition effects; it is assumed that groups perform best when objectives are agreed upon and well understood. Shared values may be expected to affect effectiveness both in terms of creating smooth progress toward a common formal objective and by facilitating rapport between group members when engaged in informal communication. A fourth consideration concerns skill complementarity at both task and social levels. When there is a division of labor in performing a complex task, individuals should be best qualified to fill their respective roles in terms of their previous training and professional interests. It is interesting to speculate that a complementarity of social skills might in a similar fashion contribute to the social and psychological adaptation of individual members of the group, e.g., an ideal group may be one which includes a social catalyst, a storyteller, a father figure, etc. Fifth, while all the members of a group may be perfectly compatible with one another, they may individually and as a group be unsuited to perform the task assigned to the group. That is, the individual needs of the group members may conduce to the development of norms which are incompatible with performance of the task at hand, as when highly authoritarian individuals are required to perform a task under changing and unstructured conditions.

---

[1] This discussion draws heavily on an unpublished paper by Haythorn (1963).

Summarizing the types of composition variables which others have deemed important, one may divide composition criteria into three classes: (1) The class for which group homogeneity is desirable includes variables such as values and vocabulary. (2) In some variables differences among members are desirable, such that each member may fill one or more of the many roles required for successful group interaction. Clearly, skill complementarity falls into this class. Whether the ability to get along well depends on complementarity or homogeneity of traits would appear to depend on the trait in question and must be determined empirically. (3) The last class includes variables in which group traits optimally should vary as a function of the group task. Just as certain characteristics of individuals affect their likelihood of performing a task well, so it may be that certain characteristics or personality traits of groups may influence their effectiveness in attaining the group's goals. The question of whether the personality of the individual members will, in fact, affect the personality of the group, and further, whether group personality in turn affects group effectiveness is an empirical one.

Let us examine first the research relevant to the relationship between homogeneity of composition and group performance.

Haythorn et al. (1956) performed a study on the effects of various combinations of authoritarian and equalitarian leaders and followers and found that followers were more secure and motivated in homogeneously composed groups and that the members of homogeneous groups experienced less conflict. In a study of homogeneously and heterogeneously composed problem-solving groups, Hoffman and Maier (1961) found that heterogeneous groups produced a higher proportion of high-quality solutions than homogeneous groups, and the two groups expressed equal satisfaction with the problem solutions. In examining these contrasting results, it becomes obvious that the effects of homogeneity depend on the dimensions along which homogeneity is measured, on the nature of the group goal, and the nature of the dependent measure. In the Haythorn et al., study, groups were homogeneous with regard to high or low authoritarianism, and the dependent measures were the amount of intragroup conflict and satisfaction. In Hoffman and Maier's study, however, homogeneity was measured by profile correlations between and among subjects on the Guilford-Zimmerman Temperament Survey, and the dependent measures were the quality and number of solutions produced and group satisfaction with these solutions. Homogeneity on one variable is obviously not equivalent to homogeneity on another; profile correlations would thus be poor predictors of compatibility, since homogeneity with regard to some personality variables in the profile (e.g., degree of authoritarianism) would predict compatibility, while homogeneity on other variables on the same test (e.g., degree of dominance) would predict incompatibility.

In a study which seemed to tap attitudes and values, Havron et al. (1952) found that infantry rifle squads performed better when squad members were similar with respect to degree of confidence in their physical prowess, amount of combat time, level of aspiration, and time in the army. However, the technical effectiveness of B-29 crews was found to be curvilinearly related to degree of similarity among crew members on various status measures (Adams, 1953). The more effective crews had an intermediate level of status congruency, suggesting that some degree of status differentiation is desirable; crew members may find it difficult to work together when each is entitled to equal power.

Assuming that one measure of group viability is the tendency for group members to continue to interact even when they are free to disperse, the following study bears upon the relationship of attitudinal similarity to group viability. Students were found to choose to keep in touch with those peers after leaving college whose rankings of the importance of thirty-nine criteria for evaluation were similar to their own. Rankings were most similar between pairs of peers who mutually chose each other (Precker, 1952).

Summarizing the research on the relationship between group homogeneity and group effectiveness, Haythorn (1963) has concluded that:

> . . . similarity of certain values relevant to group activity is characteristic of more effective groups. The nature and strength of these relationships depend intimately on the variable on which homogeneity or similarity is measured. Since crew compatibility and effectiveness is dependent on a very large number of interacting factors, predictions about single groups or crews could not be based very reliably on the degree of similarity among group members with regard to any single dimension. To the best of our knowledge, there have been no experimental attempts to vary the composition of crews on a number of dimensions simultaneously (p. 21).

To the extent that each individual cannot play every desirable role effectively, composition in terms of ability of various members to play these specialized roles would seem desirable. One relevant study by Winch (1955) on selection of marital partners presents evidence that selection is in part based on the complementary need patterns of the couple; Winch predicted the correlations between couples on personality needs such as abasement, dominance, and hostility. He predicted, for example, that the husband's need to express hostility would correlate positively with the wife's need for abasement. Such correlations were in the predicted direction significantly beyond chance level (.05 level of confidence). Though not as compelling as one might hope, these results do support the complementary needs hypothesis.

According to Schutz's theory of need complementarity (1958) there are three principal interpersonal needs: need for inclusion, affection, and control. Individuals desire certain behaviors on the part of others which satisfy their own needs, and individuals behave in ways that may satisfy these or some other needs for other persons. For example, one may seek much affectionate behavior from others and be able to give much affection as well, or one may express need to control others but wish to find little controlling behavior in others. Schutz has devised a questionnaire method for measuring compatibility according to his theory, and he demonstrated its predictive power in a number of empirical situations.

Little more has been empirically gleaned concerning the interplay of personalities in groups. Additional research is required in the area of personality interaction in order to gain adequate understanding of the processes involved. The identification of major personality characteristics in the interaction process and development of better measures of these characteristics remains to be done. Similarly, little research has been reported with regard to complementarity of social skills and expertise. The work which has been done suggests that the relationships between skill similarity and group success are very complex, such that new situations require careful scrutiny before the results of studies may be generalized to them.

For example, in a study of B-29 bomb team members, Voiers (1956) found that proficiency of one bomb team member was reflected in the team's total bomb score only to the extent that other team members were proficient. The functions of such a team were hypothesized to be in series, i.e., task functions were so specialized that team members could not compensate for one another's shortcomings. Voiers suggests that in other operations, positions are in parallel, i.e., one crew member can compensate for another. In such situations, introduction of new members who were dissimilar in proficiency would be advantageous if these members deviated by being more proficient.

Dissimilarity in type rather than proficiency of skills has been examined by Peltz (1956), who studied the productivity of scientists and engineers in research organizations. The relationships between scientific productivity and frequency of contact with similar and dissimilar colleagues was examined. Higher productivity resulted from daily contact with colleagues who had experiences, values, and attitudes dissimilar to their own, as opposed to daily contact with similar colleagues or infrequent contact with any colleagues. Daily contact among persons with different outlooks thus appeared to provide mutual stimulation resulting in higher research productivity. However, for the scientists who were not in frequent contact with their colleagues, similar colleagues enhanced productivity. The interaction between similarity and frequency of contact for colleagues having specific relationships was also examined. When the most significant colleague had an outlook similar to the scientists' own, contact frequency was positively related to scientific productivity, while a negative relationship existed between contact frequency and scientific productivity when the one most significant colleague held dissimilar outlook. Peltz concluded that one condition for high performance is daily contact with colleagues of widely different values and experiences, but that one or two like-minded colleagues are valuable in encouraging the individual researcher's own ideas.

As in the other areas of group composition, not nearly enough is known about selection in terms of role complementarity to permit careful planning of actual group composition in terms of this variable. Many other important personality dimensions remain to be defined and studied.

Finally, we will examine whether groups display traits characteristic of individual group members.

That groups exhibit, as a whole, some of those qualities found in the majority of their members is a common belief of long standing, particularly with regard to beliefs about ethnic groups. Empirical research has shown that some aspects of group behavior do, in fact, vary as a function of the personality characteristics of group members. The behavior of groups which were composed of authoritarian members was found to differ significantly from the behavior of groups composed exclusively of nonauthoritarian members (Haythorn et al., 1956*b*). Haythorn (1953) composed laboratory groups in which he required each subject to work with each other subject once, and he traced the effects of individual members on the characteristics of the group in which they worked. He found that the rated characteristics of groups were directly related to the personality characteristics of individual members.

However, each group member does not have an equal effect upon the characteristics of his group. Research on leadership indicates that leader characteristics greatly influence the nature of the group. Lewin, Lippitt, and White (1939), for example, showed that the nature of group behavior was significantly related to the nature of the leadership exerted. Similarly, Haythorn et al. (1956*a*) studied the leadership of individuals with known but differing attitudes toward authority. Four types of groups were studied: groups were composed either of authoritarian or equalitarian group members and either authoritarian or equalitarian appointed leaders. There was found to be a higher degree of equal participation, striving for equal participation, and motivation toward the group goal in groups with equalitarian leadership. There was also greater satisfaction with the appointed leader and greater difference of opinion expressed under equalitarian leadership.

Obviously, too few personality variables have been examined to warrant any strong conclusions concerning the relationship between group and individual characteristics. Also the possible interaction effect between task variables and individual personality variables as a determiner of the extent to which groups acquire the characteristics of individual members remains to be studied.

SUMMARY

As noted in the introduction social psychology is in a period of rapid growth. The scope and diversity of empirical findings, theory and method encompassed by the field precludes comprehensive and systematic coverage in a brief space. Selection of topics and emphasis is necessary; our choices were dictated primarily by personal preferences although some consideration was accorded the relative importance of the topic.

After a brief consideration of the rationale for an experimental approach in social psychology we turned to three broad areas of inquiry: social learning, cognitive consistency, and social interaction. Social learning is concerned with the content and sequencing of early social experiences and the processes which produce the behaving, thinking, feeling social animal we call man. Performance in a social situation is determined not solely by the nature of the social environment but in large part by the habits, beliefs, attitudes and affective states of the performing individuals —in short, by those aspects of personality which are the products of socialization. In our brief coverage of the problem, we attempted to summarize some of the principles of social learning and to sample the empirical findings on several important behavioral dimensions.

Through social learning an individual develops characteristic ways of perceiving and reacting to his social world. Though relatively general, such modes of reaction are not insensitive to the momentary social situation in which the individual finds himself and are not so pervasive that they provide a ready basis for action in all social contexts. To cope adequately with the social world one must be sensitive to the requirements of specific social situations and thus be continuously engaged in perceiving, judging and interpreting both social and non-social events.

The obvious importance for an understanding of social behavior of cognitive activity early directed the attention of social psychologists to an understanding of the structure, function, and laws of change of cognitive systems. Though the search took place along many fronts—the study of beliefs, attitudes, social judgments, perception—there has recently developed a set of related theories which hold some promise of bringing order out of the chaotic mass of empirical findings. These cognitive consistency theories, encompassing as they do a wide range of cognitive and behavioral phenomena, hold promise of providing the basis for the unifying theory so sorely needed in social psychology. For this reason they are one of the most important developments in decades, and considerable space was devoted to reviewing several major variants of consistency theory as well as some samples of the data the theories are designed to explain.

Finally, under the rubric social-interaction we touched on a number of problems which have been of concern to social psychologists for many years. Americans have been accused of being a conforming people. Whatever the truth of this assertion, American social scientists have had a long-standing interest in the conditions under which conformity occurs or does not occur, and the personality traits which contribute to susceptibility to conformity pressures. Conformity has many faces, and though non-functional in some contexts, in others it clearly provides the basis for effective social interaction. All social groups evolve implicit or explicit norms of behavior and become differentiated with respect to the functions or roles members perform. Social psychologists have been concerned with trying to elaborate the ways in which norms and roles become established and how they achieve their control over social behavior. This aspect of group processes is but one of many facets of small group behavior which have received attention in the past several decades. However, once again space limitations did not permit consideration of topics such as leadership, group productivity, group and individual problem solving, group goals, and structural properties of groups. From the many alternatives the problem of group composition was selected as illustrative of the nature of research on groups.

REFERENCES

ABELSON, R. P. Mode of resolution of belief dilemmas. *J. conf. Resolution.* 1959, 3, 343-353.

———, AND LESSER, G. S. The measurement of persuasibility in children. In C. I. Hovland and I. R. Janis (eds.), *Personality and Persuasibility.* New Haven: Yale University Press, 1959.

ADAMS, S. Status congruency as a variable in small group performance. *Soc. Forces,* 1953, 32, 16-22.

ADORNO, T. W., FRENKEL-BRUNSWIK, E., LEVINSON, D. J., AND SANFORD, R. N. *The Authoritarian Personality.* New York: Harper & Brothers, 1950.

ALLPORT, G. W. *Personality: A Psychological Interpretation.* New York: Henry Holt and Company, Inc., 1937.

ARONSON, E., AND MILLS, J. The effect of severity of initiation on liking for a group. *J. abnorm. soc. Psychol.,* 1959, 59, 177-181.

ASCH, S. E. Forming impressions of personality. *J. abnorm. soc. Psychol.,* 1946, 41, 258-290.

BANDURA, A. Relationship of family patterns to child behavior disorders. Progress Report, U.S.P.H. Research Grant M-1734, Stanford University, 1960.

———. Social learning through imitation. In M. R. Jones (ed.), *Nebraska Symposium on Motivation.* Lincoln: University of Nebraska Press, 1962, pp. 211-269.

———, AND WALTERS, R. H. *Adolescent Aggression.* New York: The Ronald Press Company, 1959.

———, AND ———. Aggression. In *Child Psychology: The Sixty-second Yearbook of the National Society for the Study of Education,* Part I. Chicago: The National Society for the Study of Education, 1963, pp. 364-415.

———, AND ———. *Social Learning and Personality Development.* New York: Holt, Rinehart, and Winston, Inc., 1963.

BARKER, R. G., AND WRIGHT, H. F. *Midwest and Its Children.* Evanston, Ill. Row, Peterson & Company, 1954.

BERELSON, B., AND STEINER, G. A. *Human Behavior: An Inventory of Scientific Findings.* New York: Harcourt, Brace & World, Inc., 1964.

BIJOU, S. W., AND BAER, D. M. The laboratory-experimental study of child behavior. In P. H. Mussen (ed.), *Handbook of Research Methods in Child Development.* New York: John Wiley & Sons, Inc., 1960, pp. 140-197.

BOWLBY, J. *Maternal Care and Mental Health.* World Health Organization, Monograph Series, No. 2, 1952.

BREHM, J. Post decision changes in the desirability of alternatives. *J. abnorm. soc. Psychol.,* 1956, **52**, 384-389.

BREHM, J. W. Increasing cognitive dissonance by a fait acompli. *J. abnorm. soc. Psychol.,* 1959, **58**, 379-382.

———. Attitudinal consequences of commitment to unpleasant behavior. *J. abnorm. soc. Psychol.,* 1960, **60**, 379-383.

———, AND COHEN, A. R. Choice and chance relative deprivation as determinants of cognitive dissonance. *J. abnorm. soc. Psychol.,* 1959*a*, **58**, 383-387.

———, AND ———. Reevaluation of choice alternatives as a function of their number and qualitative similarity. *J. abnorm. soc. Psychol.,* 1959*b*, **58**, 373-378.

BRUNER, J. S., AND POTTER, M. C. An exploration of visual recognition. Unpublished manuscript, Harvard University, 1962.

CAIRNS, R. B. Antecedents of social reinforcer effectiveness. Unpublished manuscript, Indiana University, 1962.

CARLSON, E. R. Attitude change through modification of attitude structure. *J. abnorm. soc. Psychol.,* 1956, **52**, 256-261.

CARTWRIGHT, D., AND HARARY, F. Structural balance: a generalization of Heider's theory. *Psychol. Rev.,* 1956, **63**, 277-293.

———, AND ZANDER, A. *Group Dynamics: Research.* Evanston, Ill., Row, Peterson, & Company, 1953.

CHOWDRY, K. AND NEWCOMB, T. The relative ability of leaders and nonleaders to estimate opinions of their own group. *J. abnorm. soc. Psychol.,* 1952, **47**, 51-57.

COHEN, A. R. Communication discrepance and attitude change: a dissonance theory approach. *J. Pers.,* 1959, **27**, 386-396.

———. Attitudinal consequences of induced discrepancy between cognition and behavior. *Pub. opin. Quart.,* 1960, **24**, 296-318.

———, TERRY, H. I. AND JONES, C. B. Attitudinal effects of choice in exposure to counterpropaganda. *J. abnorm. soc. Psychol.,* 1959, **58**, 388-391.

COWAN, P. A., AND WALTERS, R. H. Studies of reinforcement of aggression. I. Effects of scheduling. *Child Developm.,* 1963, **34**, 543-551.

COWLES, J. T. Food tokens as incentives for learning by chimpanzees. *Comp. psychol. Monogr.,* 1937, **14**, No. 5.

CRONBACH, L. Processes affecting scores in "understanding of others" and "assumed similarity." *Psychol. Bull.,* **52**, 1955, 177-193.

———. Analytic treatment of social perception scores. In R. Taguiri and L. Petrullo (eds.), *Person Perception and Interpersonal Behavior.* Stanford, Calif.: Stanford University Press, 1958.

DAVIS, A. Child training and social class. In R. G. Barker, J. S. Kounin, and H. F. Wright (eds.), *Child Behavior and Development.* New York: McGraw-Hill Book Company, Inc., 1943, pp. 605-619.

———, AND HAVIGHURST, R. J. *The Father of the Man: How Your Child Gets His Personality.* Boston: Houghton Mifflin Company, 1947.

———. *Social Class Influences upon Learning.* Cambridge: Harvard University Press, 1948.

DAVIS, KINGSLEY. *Human Society.* The Macmillan Co., 1949.

DECHARMS, R., AND ROSENBAUM, M. E. Status variables and matching behavior. *J. Pers.,* 1960, **28**, 492-502.

DOLLARD, J., AND MILLER, N. E. *Personality and Psychotherapy.* New York: McGraw-Hill Book Company, Inc., 1950.

DOOB, L., et al. *Frustration and Aggression.* New Haven: Yale University Press, 1939.

ESPINES, A. *Des Societés Animales.* Paris: J. B. Baillière et fils, 1877.

FESTINGER, L. *A Theory of Cognitive Dissonance.* Evanston, Ill.: Row Peterson & Company, 1957.

———. The psychological effect of insufficient rewards. *Amer. Psychologist,* 1961, **16**, 1-2.

———, AND CARLSMITH, J. M. Cognitive consequences of forced compliance. *J. abnorm. soc. Psychol.,* 1959, **58**, 203-210.

———, GERARD, H. B., et al. The influence process in the presence of extreme deviates. *Human Relat.,* 1952, **5**, 327-346.

———, RIECKEN, J., AND SCHACHTER, S. *When Prophecy Fails.* Minneapolis: University of Minnesota Press, 1956.

FIEDLER, F. Interpersonal perception and group effectiveness. In R. Taguiri and L. Petrullo, (eds.), *Person Perception and Interpersonal Behavior.* Stanford, Calif.: Stanford University Press, 1958.

Flugel, J. C. Some reflections on the self-sustaining tendencies. *Br. J. Psychol.*, 1948, **38**, 171-190 (Cited from Woodworth and Schlosberg, 1954, p. 686).

Fouriezos, N. T., Hutt, M. L., and Guetzkow, H. Measurement of self-oriented needs in discussion groups. *J. abnorm. soc. Psychol.*, 1950, **45**, 682-690.

French, J. R. P., Jr. The disruption and cohesion of groups. *J. abnorm. soc. Psychol.*, 1941, **36**, 361-377.

Freud, S. *Beyond the Pleasure Principle.* In Standard edition. Vol. XVIII. London, Hogarth Press, 1955 (First German edition, 1920).

Gage, N., and Cronbach, L. Conceptual methodological problems in interpersonal perception. *Psychol. Rev.*, 1955, **62**, 411-423.

Gardner, R. W., Holzman, P. S., Klein, G. S., Linton, H. B., and Spence, D. P. Cognitive control: A study of individual consistencies in cognitive behavior. *Psychol. Issues,* 1959, **1**, No. 4.

Gerwirtz, J. L., and Baer, D. M. The effect of brief social deprivation on behaviors for a social reinforcer. *J. abnorm. soc. Psychol.*, 1958, **56**, 49-56.

———, and ———. Deprivation and satiation of social reinforcers as drive conditions. *J. abnorm. soc. Psychol.*, 1958, **57**, 165-172.

Goffman, E. *The Presentation of Self in Everyday Life.* New York: Doubleday & Company, Inc., 1959.

Goldfarb, W. Infant rearing and problem behavior. *Amer. J. Orthopsychiat.*, 1943, **13**, 249-265.

Gray, P. H. Theory and evidence of imprinting in human infants. *J. Psychol.*, 1958, **46**, 155-166.

Hare, A. P. A study of interaction and concensus in different sized groups. *Amer. Soc. Rev.*, 1952, **17**, 261-267.

Harlow, H. F., and Harlow, Margaret K. The effect of rearing conditions on behavior. *Bull. Menninger Clin.*, 1962, **26**, 213-224.

Hartup, W. W. Nurturance and nurturance-withdrawal in relation to the dependency behavior of pre-school children. *Child Developm.*, 1958, **29**, 190-201.

Harvey, O. J., Hunt, E. E., and Schroder, H. M. *Conceptual Systems and Personality Organization.* New York: John Wiley & Sons, Inc., 1961.

Havron, M. D., Fay, R. J., and McGrath, J. E. The effectiveness of small military units. Washington, D.C.: Institute for Research in Human Relations, Sept. 30, 1952 (Personnel Research Selection report No. 980).

Haythorn, W. The influence of individual members on the characteristics of small groups. *J. abnorm. soc. Psychol.*, 1953, **48**, 276-284.

———. The composition of groups: a review of the literature. Unpublished paper, 1963.

———, Couch, A., Haefner, D., Langham, P., and Carter, L. F. The effects of varying combinations of authoritarian and egalitarian leaders and followers. *J. abnorm. soc. Psychol.*, 1956*a*, **53**, 210-219.

———, ———, ———, ———, and ———. The behavior of authoritarian and egalitarian personalities in groups. *Hum. Rela.*, 1956*b*, **9**, 57-74.

Hegel, G. W. F. *Phänomenologie des Geistes* (first published 1807), transl.: *The Phenomenology of Mind*, London: George Allen & Unwin, Ltd., 1910.

Heider, F. Social perception and phenomenal causality. *Psychol. Rev.*, 1944, **51**, 358-374.

———. Attitudes and cognitive organization. *J. Psychol.*, 1946, **21**, 107-112.

———. *The Psychology of Interpersonal Relations.* New York: John Wiley & Sons, Inc., 1958.

———, and Simmel, M. An experimental study of apparent behavior. *Amer. J. Psychol.*, 1944, **57**, 243-259.

Hobbes, T. *Leviathan.* Reprint of (1651) first edition. Cambridge, England: University Press, 1904.

Hoffman, R., and Maier, N. Quality and acceptance of problem solutions by members of homogeneous and heterogeneous groups. *J. abnorm. soc. Psychol.*, 1961, **62**, 401-407.

Horowitz, M., Lyons, J., and Perlmutter, H. V. Induction of forces in discussion groups. *Hum. Rela.*, 1951, **4**, 57-76.

Horwitz, M. The recall of interrupted group tasks: an experimental study of individual motivation in relation to group goals. *Hum. Rela.*, 1954, **7**, 3-38.

Hovland, C. I., Harvey, O. J., and Sherif, M. Assimilation and contrast effects in reactions to communication and attitude change. *J. abnorm. soc. Psychol.*, 1957, **55**, 244-252.

———, Janis, I. L., and Kelly, H. H. *Communication and Persuasion.* New Haven: Yale University Press, 1952.

Hull, C. L. *Principles of Behavior.* New York: D. Appleton-Century Company, Inc., 1943.

Ichheiser, G. Misunderstandings in human relations. *Amer. J. Social.* 1949, SS, **2**, Part 2.

Janis, I. R., and Field, P. B. Sex differences and personality factors related to persuasibility. In C. I. Hovland and I. R. Janis (eds.), *Personality and Persuasibility*, New Haven: Yale University Press, 1959.

———, and King, B. The influence of role-playing on opinion change. *J. abnorm. soc. Psychol.*, 1954, **49**, 211-218.

JONES, E. E. Authoritarianism as a determinant of first impression formation. *J. Pers.*, 1954, **23**, 107-127.

———, AND THIBAUT, J. W. Interaction goals as basis of inference in interpersonal perception. In B. Taguiri and L. Petrullo (eds.), *Person Perception and Interpersonal Behavior.* Stanford, Calif.: Stanford University Press, 1958.

JORDAN, N. Behavioral forces that are a function of attitudes and of cognitive organization. *Hum. Rela.*, 1953, **6**, 273-287.

JUKES, C. L. Selection of diet in chicks as influenced by vitamins and other factors. *J. Comp. Psychol.*, 1938, **26**, 135-156.

JUNG, C. G. *Collected Papers on Analytical Psychology.* (2d ed.), transl. London: Baillière, Tindall & Cox, 1922.

KANAREFF, V. T., AND LANZETTA, J. T. The acquisition of imitative and oppositions responses under two conditions of instruction induced set. *J. exp. Psychol.*, 1958, **56**, 516-528.

———, AND ———. Effects of success-failure experiences and probability of reinforcement upon the acquisition and extinction of an imitative response. *Psychol. Reports,* 1960, **7**, 151-166.

———, AND ———. Effects of congruent social and task reinforcement upon acquisition of imitative responses. *Psychol. Rep.*, 1961, **8**, 47-57.

———, AND ———. An exploratory study of the factors determining the efficacy of a social reinforcement. Unpublished manuscript, 1962.

———, AND ———. Effectiveness of a social reinforcement in isolation and in conjunction with a conflicting task reinforcement. *Psychol. Reports,* 1963, **12**, 883-897.

KAUFMANN, FELIX. *Methodology of the Social Sciences,* New York: The Humanities Press, 1958.

KELMAN, H. Attitude change as a function of response restriction. *Hum. Rela.*, 1953, **6**, 185-214.

KIMBRELL, D., AND BLAKE, R. R. Motivational factors in the violation of a prohibition. *J. abnorm. soc. Psychol.*, 1958, **56**, 132-133.

KING, B., AND JANIS, I. Comparison of the effectiveness of improvising versus non-improvised role-playing in producing opinion changes. *Hum. Rela.*, 1956, **9**, 177-186.

KLUCKHOHN, CLYDE. Anthropological studies of human relations. Unpublished paper presented to the Conference on Research in Human Relations, Rockefeller Foundation, February-March, 1953.

KRASNER, L. Studies of the conditioning of verbal behavior. *Psychol. Bull.*, **56**, No. 3, 1958, 148-170.

KRECH, D., CRUTCHFIELD, R. S., AND BALLACHEY, E. *Individual in Society.* New York: McGraw-Hill Book Company, Inc., 1962.

LAMBERT, W. W., AND LAMBERT, W. E. *Social Psychology.* Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1964.

LANZETTA, J. T., AND KANAREFF, V. T. The effects of a monetary reward on the acquisition of an imitative response. *J. abnorm. soc. Psychol.*, 1959, **59**, 120-127.

———, AND ———. The effects of congruent and conflicting social and task feedback on the acquisition of an imitative response. *J. abnorm. soc. Psychol.*, 1961, **62**, 322-328.

LEFKOWITZ, M. M., BLAKE, R. R., AND MOUTON, JANES S. Status factors in pedestrian violation of traffic signals. *J. abnorm. soc. Psychol.*, 1955, **51**, 704-706.

LEVY, D. M. *Maternal Overprotection.* New York: Columbia University Press, 1943.

LEWIN, K., LIPPITT, R., AND WHITE, R. Patterns of aggressive behavior in experimentally created "social climates." *J. soc. abnorm. Psychol.*, 1939, **10**, 271-299.

LINTON, H., AND GRAHAM, E. Personality correlates of persuasibility. In C. I. Hovland and I. L. Janis, *Personality and Persuasibility,* New Haven: Yale University Press, 1959.

LOVAAS, O. I. Effect of exposure to symbolic aggression on aggressive behavior. *Child Developm.*, 1961, **32**, 329-336.

MCCLELLAND, C. *The Achieving Society.* Princeton, N.J.: D. Van Nostrand Company, Inc., 1961.

MCCLELLAND, D. C., ATKINSON, J. W., CLARK, R. A., AND LOWELL, E. L. *The Achievement Motive.* Appleton-Century-Crofts, Inc., 1953.

MARCH, J. G., AND SIMON, H. A. *Organizations.* New York: John Wiley & Sons, Inc., 1958.

MELZACK, R., AND SCOTT, T. H. The effects of early experience on the response to pain. *J. comp. physiol. Psychol.*, 1957, **50**, 155-161.

MERTON, R. K., AND LAZARSFELD, P. F. (eds.) *Continuities in Social Research.* Glencoe, Ill.: Free Press, 1950.

MILLER, N. E. Learnable drives and rewards, Chap. 13, In S. S. Stevens (ed.), *Handbook of Experimental Psychology.* New York: John Wiley & Sons, Inc., 1951.

———. Liberalization of basic S-R concepts: extensions to conflict behavior, motivation, and social learning. In Koch, (ed.), *Psychology: A Study of a Science,* Vol. 2. New York: McGraw-Hill Book Company, Inc., 1959.

———, AND DOLLARD, J. *Social Learning and*

Imitation. New Haven: Yale University Press, 1941.

MILLS, J., ARONSON, E., AND ROBINSON, H. Selectivity in exposure to information. *J. abnorm. soc. Psychol.*, 1959, **59**, 250-253.

MORGAN, C. T. The hoarding instinct. *Psychol. Rev.*, 1947, **54**, 335-341.

MOWRER, O. H. A stimulus-response analysis of anxiety and its role as a reinforcing agent. *Psychol. Rev.*, 1939, **46**, 553-566.

MUSSEN, P. H., AND RUTHERFORD, E. Effects of aggressive cartoons on children's aggressive play. *J. abnorm. soc. Psychol.*, 1961, **62**, 461-464.

NELSON, E. A. The effects of reward and punishment of dependency on subsequent dependency. Unpublished manuscript, Stanford University, 1960.

NEWCOMB, T. M. An approach to the study of communicative acts. *Psychol. Rev.*, 1953, **60**, 393-404.

———. Social psychological theory: integrating individual and social approaches. In E. P. Hunt and R. G. Hollander (eds.), *Current Perspective in Social Psychology*. New York: Oxford University Press, 1963.

OSGOOD, C. E. Cognitive dynamics in the conduct of human affairs. *Public Opin. Quart.*, 1960, **24**, 341-365.

———, SUCI, G. J., AND TANNENBAUM, P. H. *The Measurement of Meaning*. Urbana, Ill.: University of Illinois Press, 1957.

———, AND TANNENBAUM, P. H. The principle of congruity in the prediction of attitude change. *Psychol. Rev.*, 1955, **62**, 42-56.

PATTERSON, G. R., LUDWIG, M., AND SONODA, B. Reinforcement of aggression in children. Unpublished manuscript, University of Oregon, 1961.

PELTZ, D. C. Some social factors related to performance in a research organization. *Admin. Sci. Quart.*, 1956, **1**, 310-325.

PETTIGREW, T. F. The measurement and correlates of category width as a cognitive variable. *J. Pers.*, 1958, **26**, 532-544.

PRECKER, J. A. Similarity of valuings as a factor in selection of peers and near-authority figures. *J. abnorm. soc. Psychol.*, 1952, **47**, 406-414.

RABBIE, J. M., BREHM, J. W., AND COHEN, A. R. Verbalization and reactions to cognitive dissonance. *J. Pers.*, 1959, **27**, 407-417.

ROSEN, B., AND D'ANDRADE, R. The psychosocial origins of achievement motivation. *Sociometry*, 1959, **22**, 185-218.

ROSENBAUM, J. E. The effect of stimulus and background factors on the volunteering response. *J. abnorm. soc. Psychol.*, 1956, **53**, 118-121.

ROSENBERG, M. Cognitive structure and attitudinal affect. *J. abnorm. soc. Psychol.*, 1956, **53**, 367-372.

———. Cognitive reorganization in response to the hypnotic reversal of attitudinal affect. *J. Pers.*, 1960*a*, **28**, 39-63.

———. A structural theory of attitude dynamics. *Pub. Opin. Quart.*, 1960*b*, **24**, 319-340.

———, AND ABELSON, R. An analysis of cognitive balancing. In C. I. Hovland and M. J. Rosenberg (eds.), *Attitude Organization and Change*. New Haven: Yale University Press, 1960.

SALTZMAN, I. J. Maze learning in the absence of primary reinforcement: a study of secondary reinforcement. *J. comp. physiol. Psychol.*, 1949, **42**, 161-173.

SCHACHTER, S. Deviation, rejection, and communication. *J. abnorm. soc. Psychol.*, 1951, **46**, 190-207.

———, AND HALL, R. Group-derived restraints and audience persuasion. *Hum. Rel.*, 1952, **5**, 397-406.

SCHUTZ, W. C. FIRO: a three-dimensional theory of interpersonal behavior. New York: Holt, Rinehart, and Winston, Inc., 1958.

SCOTT, W. A., AND WERTHEIMER, M. *Introduction to Psychological Research*. New York: John Wiley & Sons, Inc., 1962.

SEARS, R. R., MACCOBY, E. E., AND LEVIN, H. *Patterns of Child Rearing*. Evanston, Ill.: Row, Peterson & Company, 1957.

SHERIF, M. *An Outline of Social Psychology*. New York: Harper & Brothers, 1948.

SHIRER, W. L. *The Rise and Fall of the Third Reich*, New York: Simon and Schuster, Inc., 1960.

SIEGEL, A. E. Film-mediated fantasy aggression and strength of aggressive drive. *Child Developm.*, 1956, **27**, 365-378.

SKINNER, B. F. *Verbal Behavior*. New York: Appleton-Century-Crofts, Inc., 1957.

SMITH, M. F. The establishment and extinction of the token-reward habit in the cat. *J. gen. Psychol.*, 1939, **20**, 475-486.

STEINER, I. D. Interpersonal behavior as influenced by accuracy of social perception. *Psych. Rev.*, 1955, **62**, 268-274.

STELLAR, E. The effects of experimental alternations of metabolism on the hoarding behavior of the rat. *J. comp. physiol. Psychol.*, 1951, **44**, 290-299.

STOTLAND, E., AND DUNN, R. Empathy, self-esteem and birth order. *J. abnorm. soc. Psychol.*, 1963, **66**, 532-540.

SUCI, G. J. A multidimensional analysis of social attitudes with special reference to ethnocentrism. Unpublished doctoral dissertation, University of Illinois, 1952.

Taft, R. The ability to judge people. *Psychol. Bull.*, 1955, **52**, 1-23.

TAGUIRI, R. Relational analysis an extension of sociometric method with emphasis on social perception. In, Hau, E. Borgatta, R. Bales. *Small Groups: Studies in Social Interaction.* New York: Alfred A. Knopf, Inc., 1962.

———, BLAKE, R. P., AND BRUNER, J. S. Some determinants of the perception of positive and negative feelings in others. *J. abnorm. soc. Psychol.*, 1953, **48**, 585-592.

———, BRUNER, J. S., AND KOGAN, N. Estimating the chance expectancies of dyadic relationship within a group. *Psychol. Bull.* 1955, **52**, 122-131.

TANNENBAUM, P. H. Attitudes toward source and concept as factors in attitude change through communication. Unpublished doctoral dissertation, University of Illinois, 1953.

THIBAUT, J. W., AND KELLY, H. H. *The Social Psychology of Groups.* New York: John Wiley & Sons, Inc., 1959.

THORNDIKE, E. L. *The Fundamentals of Learning.* New York: Teachers College, Columbia University, 1932.

———. *An Experimental Study of Rewards.* New York: Teachers College, Columbia University, 1933.

THORPE, W. H. *Learning and Instinct in Animals.* London: Methuen & Co., Ltd., 1956.

TRIANDIS, L. M., AND LAMBERT, W. W. Sources of frustration and targets of aggression: a cross-cultural study. *J. abn. soc. Psychol.*, 1961, **62**, 640-648.

VOIERS, W. D. Bombing accuracy as a function of the ground-school proficiency structure of the B-29 bomb team. Lackland Air Force Base, Texas: Air Force Personnel and Training Research Center, January, 1956. (Research Report AFPTRC-TN-56-4.)

WALTERS, R. H., AND BROWN, M. Studies of reinforcement of aggression: II. Transfer of responses to an interpersonal situation. *Child Developm.*, 1963, **34**, 563-571.

WERTHEIMER, M. Laws of organization in perceptual forms. In *A Source Book of Gestalt Psychology.* (W. D. Ellis, ed.) New York: Brace & Co., 1939, 71-88.

WHITING, B. *Six Cultures, Studies of Child Rearing.* New York: John Wiley & Sons, Inc., 1963.

WINCH, R. F. The theory of complementary needs in mate selection: a test of one kind of complementariness. *Amer. sociol. Rev.*, 1955, **20**, 52-56.

WOLF, R. The role of conceptual systems in cognitive functioning at varying levels of age and intelligence. Unpublished doctoral dissertation, Syracuse University, 1962.

WOODWORTH, R. S. *Dynamic Psychology.* New York: Columbia University Press, 1918.

———, AND SCHLOSBERG, H. *Experimental Psychology.* New York: Henry Holt and Company, Inc., 1954.

YOUNG, P. T. The experimental analysis of appetite. *Psychol. Bull.*, 1941, **38**, 129-164.

———. Studies of food preferences, appetite and dietary habit. VIII. Food seeking drives, patability and the law of effect. *J. comp. physiol. Psychol.*,, 1948, **41**, 269-300.

ZELDITCH, M., AND HOPKINS, T. K. Laboratory experiments with organizations. In *Complex Organizations: A Sociological Reader,* Amitai Etzioni (ed.) New York: Holt, Rinehart, and Winston, Inc., 1961, 464-478.

Index of Names

Abbott, E., 144, 179
Abe, A., 331, 340
Abelson, R. P., 6, 32, 551 ff., 567, 571
Abraham, K., 474, 497
Abt, L. E., 526
Adams, D. K., 386, 412
Adams, H. E., 59, 82
Adams, J. A., 208, 217, 356, 358 f., 363 ff., 383, 405 f., 408 f., 412 f.
Adams, J. K., 213, 217
Adams, O. S., 398, 400 f., 406, 412
Adams, S., 210, 564, 567
Adamson, R., 216 f., 256, 263, 409, 412
Adamson, R. E., 443 f., 455
Adelson, M., 187, 217
Ades, H. W., 45, 82
Adey, W. R., 46, 53, 56, 82, 88
Adler, A., 468 f., 475 ff., 497, 514
Adler J., 135
Adlerstein, A., 248, 262
Adorno, T. W., 554, 567
Adrian, E. D., 273
Aftanas, M., 325, 340, 418
Agnew, M., 427, 452, 455
Albright, G. A., 400, 416
Alexander, F., 469, 497
Alexander, I. E., 243, 269
Allen, G., 127, 133
Allfrey, V. G., 132, 134
Allport, G. W., 227, 262, 468, 485 ff., 497, 503, 525, 539, 567
Alluisi, E. A., 297 ff., 306, 398, 412
Amassian, V. E., 56, 87
American Psychiatric Assoc., 502, 505, 525
Ames, A., 389, 396, 415
Amsel, A., 244 f., 250, 259, 262
Anand, B. K., 54, 82, 226, 263
Anastasi, A., 106, 134
Anderson, E. E., 228, 263
Anderson, H. H., 456
Andersson, B., 227, 229, 263
Andreas, B. G., 363, 379
Andrews, E. G., 432, 455
Ansbacher, H. L., 469, 497, 514, 525
Ansbacher, Rowena R., 514, 525
Antonitis, J. J., 239, 269
Arieti, S., 499, 525 f.
Aristotle, 273
Armus, H. L., 237, 263
Arnheim, R., 316, 340, 438, 455
Arnhoff, F. N., 399, 412
Arnold, J. E., 416, 442 f., 453, 455
Arnold, Magda, 35, 72, 74, 76 ff., 82
Arnoult, M. D., 323, 340, 396, 412
Aronson, E., 515, 525, 550, 567, 571
Aronson, S. M., 116, 134
Asch, S. E., 192, 217, 221, 545, 567
Ashby, W. R., 69, 82
Atkinson, J. W., 262 f., 267, 342, 515, 522, 525 f., 570
Attneave, F., 20, 32, 277, 295, 298, 306, 323, 340
Auden, W. H., 455
Augerson, W. S., 415
Augsburg, T., 418
Avis, V., 131, 137

Bachrach, A. J., 527
Backer, R., 231 f., 269
Badia, P., 256 f., 265
Baer, D. M., 535, 541, 568 f.
Bagshaw, M., 48, 87
Bahrick, H. P., 358 f., 363 f., 379
Bailey, A. W., 362, 366, 379, 383
Bailey, C., 503, 527
Baillinger, E. R., 415
Bain, A., 312
Bakan, P., 407, 412
Baker, C. A., 347 ff., 379
Baker, C. H., 372 f., 379, 382, 406 f., 409, 411 f.
Baker, Grace, 515, 525
Baker, R. A., 249, 263, 373, 375, 379, 383, 405 ff., 412, 416 f.
Baker, R. W., 246, 267
Bakker, C., 510, 527
Ballachey, E., 529, 570
Bandura, A., 517 f., 521, 525, 534 f., 537 f., 542, 567
Barber, B., 447, 455
Barker, A. N., 232, 266
Barker, R. G., 388, 412, 536, 568
Barlow, H. B., 4, 32, 284, 306
Barnes, G. W., 231, 263
Barnes, J. M., 2, 11, 209, 217
Baroff, G. S., 127, 133
Baron, A., 230, 263
Barron, F., 430, 436, 455
Barry, H., III, 251, 254, 263
Barry, J., 47, 87
Bartlett, F., 452, 455
Bartlett, F. C., 358, 367, 379
Bash, K. W., 225, 263
Basilio, C., 136
Bastock, M., 106, 123, 134
Batchelder, W. H., 187, 217
Bates, J. A. V., 47, 82
Bates, M. W., 225, 267
Battersby, W. S., 44, 60, 86, 89
Battig, A. W., 299, 306
Battig, K., 50, 82
Battig, W. F., 193, 195, 198, 217
Baxter, J. C., 520, 525
Beach, F. A., 37, 82, 507, 525
Beadle, G. W., 100, 129, 134
Bearn, A. G., 127, 134
Becker, J., 515, 520, 525
Beebe-Center, J. G., 297, 306
Behan, R. A., 229, 265
Behar, I., 333 ff., 340
Behrend, E. R., 171, 176
Beittel, K. R., 430, 455
Békésy, G. von, 282, 306
Bell, C., 273
Belleville, R. E., 415
Bellows, R. T., 227, 263
Benda, C. E., 127, 134
Bender, M. B., 44, 60, 86, 89
Benjamin, F. B., 400, 416
Bennett, E. L., 58, 87, 135
Bennett, W. F., 359, 364, 379
Bentley, A. F., 389, 413

Berelson, B., 536, 568
Berger, L., 454, 457
Berger, R. M., 423, 455
Bergmann, G., 160, 178, 387, 391, 412
Bergum, B. O., 405 ff., 412
Berkeley, B., 311 f.
Berkson, J., 280, 306
Berkun, M. M., 231, 233, 263, 267
Berlin, L., 60, 89
Berlyne, D. E., 240, 243, 259, 263, 400, 412
Berman, A. J., 57, 87
Bernard, C., 431
Bernheim, H., 471
Bernstein, J., 273
Bernstein, L. M., 226, 263
Berry, P. C., 458
Bersh, P. J., 235, 237, 239, 263, 269
Bertocci, P. A., 228, 263
Bessman, M. J., 135
Bevan, W., 216 f., 256, 263, 331, 333 ff., 343, 392 ff., 396, 407, 409, 412, 414
Bexton, W. H., 241, 263, 397 ff., 412, 417
Bicknell, E. A., 228, 264
Biderman, A. D., 397, 413
Bijou, S. W., 535, 568
Bilodeau, I. M., 209, 217
Bindra, D., 522, 525
Birch, H. G., 440, 443 f., 455
Birdsall, T. G, 186, 221, 301, 303, 306, 308
Birmingham, H. P., 358, 360 ff., 369, 379
Bishop, G. H., 284, 306
Bitterman, M. E., 139, 157 f., 163, 167, 171 f., 175 f., 178 f.
Black, A. H., 230, 264
Black, R., 216 f., 256, 263
Black, R. W., 336, 340
Blackwell, H. R., 282, 306
Blair, W. C., 354 f., 379
Blake, L., 46, 82
Blake, R. R., 285, 308, 542 f., 546, 562, 570, 572
Bliss, E. L., 405, 416
Block, C. H., 458
Blodgett, H. C., 158, 176
Blum, J. S., 47, 82
Boardman, W. K., 336, 340
Bodian, D., 37, 82
Bogartz, W., 454, 457
Bois-Reymond, E., du, 273
Boisen, A., 503 f., 525
Bolles, R. C., 206, 217
Bond, N. A., 444, 455
Bonner, D. M., 100, 134
Borek, E., 129, 134
Boren, J. J., 54, 82
Boring, E. G., 182, 217, 280, 306, 312, 316, 330, 340
Boudreau, J. C., 113, 135
Boulter, L. R., 372 ff., 376 ff., 406, 409, 412 f.
Bouman, M. A., 289, 306
Bousfield, W. A., 249, 263, 441 f., 455
Bowen, H. M., 360 ff., 380
Bower, G. H., 187, 197, 199 ff., 215 ff., 234, 245, 263
Bower, J. L., 368, 379
Bowlby, J., 533, 535, 568
Boyd, W. C., 117, 134
Braden, I., 242 f., 267
Brady, J. V., 54, 82
Braithwaite, R. B., 133 f.
Brazier, M. A. B., 43, 82
Breen, R. A., 59, 82
Breese, E. L., 120, 134
Breg, R. W., 126
Brehm, J. W., 549 ff., 568, 571
Breuer, J., 471
Briggs, G. E., 209, 211, 218, 358, 363, 365 f., 379
Bright, M. G., 239, 264
Brittain, W. L., 430, 455
Broadbent, D. E., 298, 306, 373, 376, 379, 408, 413
Broadhurst, P. L., 120, 134, 252 f., 263
Brobeck, J. R., 54 f., 82, 226, 263
Brodie, D. A., 54, 82
Brogden, W. J., 163, 176, 250, 263
Brooks, L. O., 444, 457
Brosgole, L., 321, 343
Brown, G. S., 368, 379
Brown, G. W., 234, 263 f.
Brown, J., 183, 218
Brown, J. F., 395, 413
Brown, J. L., 396, 403, 413
Brown, J. S., 228 ff., 244, 259, 261, 263
Brown, L. T., 334, 342
Brown, M., 537, 572
Brown, M. L., 234, 264
Brown, R. H., 394 f., 413
Brown, T., 49, 82
Brown, U., 238, 269
Brown, W., 161, 176
Brownfield, C. A., 399, 412
Bruner, J. S., 389, 401, 413, 546, 554, 562 f., 568, 572
Brunswik, E., 385, 388, 392, 413, 417, 467, 490, 497
Brush, E., 50, 82
Buchanan, G., 234, 270
Buchwald, A. M., 213, 216, 218
Buchwald, N. A., 57, 82
Buckner, D. N., 405 f., 409, 413 f., 416
Bugelski, B. R., 203 f., 218
Bühler, K., 318 f., 340
Bulatao, E., 225, 264
Bullock, D. H., 171 f., 176
Bureš, J., 58, 82
Burešová, O., 58, 82
Burke, C. J., 196, 216, 219, 283, 307
Burkhart, R. C., 436, 456
Burns, N. M., 397, 413
Burt, C., 474, 497
Buschke, H., 183, 218
Bush, R. R., 80, 82, 196, 199, 218
Butler, R. A., 168, 176, 231, 241, 264
Butter, C. M., 50, 61, 83, 235, 264

Cairns, R. B., 538, 568
Calfee, R. C., 287, 308
Calhoun, J. B., 109
Calvin, A. D., 157 f., 176, 179
Calvin, J. S., 228, 264
Camberari, J. D., 400, 413
Cameron, N., 509, 518, 525
Campbell, A. A., 250, 254, 264
Campbell, B. A., 231 f., 249 f., 264, 269 f.
Campbell, D. P., 368, 379
Campbell, D. T., 299, 306
Candland, D. K., 171, 176, 398 f., 417
Cannon, W. B., 223, 225 f., 264, 387, 413, 431, 456
Cantor, G. N., 510, 525
Cantril, H., 487 f., 497, 527
Carleton, J. L., 226, 269
Carlsmith, J. M., 515, 525, 550, 568
Carlson, A. J., 225, 264, 270
Carlson, E. R., 548, 568
Carlson, N. J., 230, 264
Carnap, R., 275, 306, 386, 413
Carpenter, J. A., 251, 265
Carr, H. A., 150, 176, 182, 224, 264
Carroll, J. B., 423, 456
Carter, L. F., 569
Carterette, E. C., 303, 306
Cartwright, D., 547, 557, 559, 568
Cason, H., 215
Caspari, E., 123, 134
Cattell, J. M., 280, 306
Cattell, R. B., 124, 134, 468, 497
Ceraso, J., 192, 209, 217
Chambers, R. M., 233, 264, 394, 397, 413
Chambliss, D., 239, 270
Champion, R. A., 258, 264
Chapanis, A., 297, 307, 347, 352, 379
Chapman, L. F., 60, 89
Charcot, J. M., 471
Chargaff, E., 100, 134
Charipper, B. A., 366, 379
Chassell, L. M., 432, 456
Chein, I., 385, 413
Chernikoff, R., 358, 361, 379
Chiles, W. D., 398, 400 f., 406, 412
Chodorkoff, B., 483 f., 497

Chorness, M. H., 456, 459
Chow, K. L., 41, 43 ff., 47, 53, 83, 85 f.
Chowdry, K., 568
Christensen, J. M., 348 f., 355, 379
Christensen, P. R., 423, 441 f., 445, 455 ff., 459
Churchill, W., 441
Churukian, G. A., 43, 87
Cicala, G. S., 252, 264
Clark, G., 41, 83, 85
Clark, L. L., 198, 218
Clark, R. A., 262, 267, 314 f., 342, 515, 526, 570
Clarke, A. R., 507, 520, 526
Clausen, J. A., 12, 32
Clayton, F. L., 231, 264
Coate, W. B., 158, 176
Cofer, C. N., 223, 250, 264
Coghill, G. E., 506, 525
Cohen, A. R., 549 f., 568, 571
Cohen, B. D., 234, 263 f., 398, 417
Cohen, B. H., 455
Cohen, J., 298, 308, 331, 340
Cohen, Mable B., 515, 525
Cohen, R. A., 515, 525
Cohen, W., 394 f., 413
Coke, E. U., 187, 221
Cole, J., 47, 57, 83 f.
Cole, L. W., 144, 176
Cole, M., 216, 219, 303, 306
Coleman, E. B., 188, 212, 218
Coleman, P. D., 59, 84
Collier, G., 231 f., 264, 269, 291, 306
Collins, W., 400, 416
Combs, A. W., 316, 343
Condillac, E. B. de, 273
Conklin, J. E., 361, 379
Conners, C. K., 314, 341
Connors, J., 52, 87
Conrad, D., 54, 82
Conrad, R., 182, 218
Coombs, C. H., 5, 12 ff., 17, 26, 32
Coons, E. E., 59, 83
Cooper, A., 456
Copeland, Nola K., 403, 416
Coppock, H. W., 233, 264
Cornsweet, J., 320, 341
Corso, J. F., 283, 298, 306
Cotton, J. W., 54, 83, 196, 251 f., 254 f., 260, 264, 267
Couch, A., 569
Coutu, W., 389, 413
Cowan, P. A., 537, 568
Cowell, H., 438, 456
Cowen, E. L., 456
Cowey, A., 43, 83, 89
Cowles, J. T., 234, 264, 539, 568
Cox, A. B., 456
Craig, D. R., 369, 379 f.
Crandall, V. J., 497
Crary, D. D., 116, 136
Crawford, R. P., 453, 456
Creamer, L. R., 358, 364, 367, 378
Creelman, C. D., 303, 306
Crespi, L. P., 165, 170, 176, 216, 218, 256, 264
Crick, F. H. C., 100, 137
Crockett, F., 239, 267
Cromwell, R. L., 510, 525
Cronbach, L., 562, 568 f.
Crothers, E. J., 198, 201, 218 f.
Crowder, W. F., 239, 264
Crowne, D. P., 496 f.
Crutchfield, R. S., 437, 456, 529, 570
Cruzen, M., 358, 382
Culler, E., 162, 176
Curtin, M. E., 255, 265

Dallenbach, K. M., 198, 218
D'Amato, M. F., 189, 220
D'Amato, M. R., 235 ff., 264
D'Andrade, R., 538, 571
Daniel, W. J., 253, 267
Darby, C. L., 331, 340
Darwin, C., 140, 176
Dashiell, J. F., 223 ff., 228, 240, 248, 265
Davenport, R. K., Jr., 240, 267
Davidson, L. P., 287, 308
Davis, A., 537, 539, 568
Davis, G. D., 57, 83
Davis, H. B., 145, 176
Davis, J. D., 231, 265
Davis, K., 533, 568
Davis, R., 186, 218
Davis, R. C., 36, 83, 225, 265
Davis, R. H., 251, 265
De, S. C., 231, 266
Dean, W. H., 57, 83
Dearborn, G. V., 432, 456
DeBeer, G., 103, 134
deCharms, R., 542, 568
Deese, J., 187, 218, 251, 265, 372 f., 377, 379 f., 393, 405 ff., 413
Deevey, E. S., 109, 134
DeGeorge, F. V., 125 f., 136
Deininger, R. L., 298, 306
Delboeuf, J. R. L., 274
Delgado, J. M. R., 50, 54, 83, 87, 234, 265
Delhagen, J. E., 231, 267
Dement, W. C., 53, 83
Dennis, W., 451, 456 f.
Denny, M. R., 229, 265
Denny-Brown, D., 48, 57, 83
De Soto, C. B., 28, 32
Deutsch, J. A., 36, 83, 321, 328, 341
DeVeau, L., 456
Dewey, J., 388 f., 413, 435, 456
Dhunjibhoy, J. E., 117
Diamond, I. T., 41, 83
Diamond, L., 167 ff., 173, 176
Dinsmoor, J. A., 239, 265
Dittman, P. E., 382
Doane, B. K., 299, 307, 398 ff., 413, 415, 417
Dobie, Shirley I., 398, 417
Dobzhansky, T., 93, 106, 108, 110, 113 f., 134, 136
Dodson, J. D., 253, 265, 270
Dollard, J., 244, 265, 516, 525, 539, 542, 568, 570
Doob, L. W., 244, 265, 537, 568
Doser, B. T., 26, 29, 31 f., 286, 307
Doty, R. W., 44, 83
Douglas, R. J., 51, 84
Douglas, W. K., 415
Dowd, J. F., 362, 366, 383
Down, J., 127
Downer, J. L. deC., 46, 83
Drevdahl, J. E., 430, 456
Ducoff, H. S., 104, 134
Duffy, M., 226, 270, 291, 308
Dufort, R. H., 249, 265
Dumont, S., 52, 84
Duncan, C. P., 59, 83, 208, 221
Duncker, K., 322, 438, 456
Dunn, L. C., 93, 136
Dunn, R., 542, 571
Dusek, E. R., 320, 341
Dworkin, R. S., 286, 307

Ebbinghaus, H., 181, 218
Ebenholtz, S. M., 204, 218, 321, 343
Ebner, F. F., 47, 83
Eccles, J. C., 78, 83
Eckel, H. E., 226, 268
Eckman, D. P., 345, 380
Eckstrand, G. A., 363, 382
Edelberg, R., 314, 341
Edison, T. A., 441
Edwards, W., 297, 306
Egan, J. P., 186 f., 218, 303, 306
Egyházi, E., 58, 84, 131, 135
Ehrenfels, C. von, 316, 341
Ehrenfreund, D., 256 f., 265
Ehret, C. F., 104, 134
Eindohoven, J. E., 438, 456
Einstein, A., 428, 437, 443
Eisenson, J., 215, 218
Ekman, G., 279, 306
Elam, C. B., 157, 176, 353, 380
Elkin, E. H., 349, 380
Eliseo, T. S., 445, 459
Elliott, J. M., 430, 456
Elliott, M. H., 151, 154, 165, 176, 249, 263
Ellison, R., 458
Ellson, D. G., 368 f., 380
Ely, J. H., 360 ff., 380
Emery, J., 353, 380
English, A. C., 35, 83
English, H. B., 35, 83
Epstein, A. N., 54, 88, 226, 270
Epstein, W., 192, 218

Ericksen, C. W., 8, 32, 189, 219, 297, 306
Erickson, R. L., 192, 218
Erlenmeyer-Kimling, L., 113 f., 122, 134 f.
Espines, A., 555 f., 568
Estes, B. W., 255, 268
Estes, W. K., 169 ff., 176, 185, 196 ff., 207, 210, 216, 218, 244, 251, 259, 265
Ettlinger, G., 47, 51, 82 f.
Evans, R. M., 317, 341
Eysenck, H. J., 468, 497, 508, 525, 527
Eysenck, H. W., 205, 219

Falek, A., 123, 135
Fantz, R. L., 45, 86
Farber, I. E., 244, 259, 263 f.
Farina, A., 520, 525
Fay, R. J., 569
Feather, N. T., 224, 265
Fechner, G. T., 17, 274, 279, 286, 306, 312
Fehrer, E., 248, 262, 265, 316 ff., 322 f., 341
Feigenbaum, E. A., 189, 205, 219
Felsinger, J. M., 254, 265
Fenichel, O., 512, 516, 525
Ferster, C. B., 520, 525
Festinger, L., 549 ff., 560, 568
Ficks, L., 298, 308
Fiedler, F., 561, 568
Field, P. B., 554, 569
Finch, G., 162, 176
Finger, F. W., 248, 250, 265
Finklestein, B., 399 f., 416
Fiske, D. W., 376, 380, 397, 413
Fitts, P. M., 298, 306, 347, 349, 355, 358 f., 361, 363 ff., 370, 380
Fitzgerald, B. J., 494, 498, 522, 525
FitzHugh, R., 4, 32
Flagle, C. D., 345, 380
Flaherty, B. E., 397, 399 f., 413
Flavell, J. H., 444, 456
Flechsig, P. E., 42, 83
Fleming, W. C., 239, 264
Flickinger, D., 402, 413
Flinn, D. E., 397, 399 f., 413
Flugel, J. C., 539, 569
Flynn, B., 280, 283, 306
Ford, C. S., 507, 525
Ford, E. B., 130, 134
Forgays, D. G., 232, 265
Forlano, G., 215, 221
Foshee, D. P., 287, 307
Fouriezos, N. T., 560, 569
Fowler, H., 257, 265
Fox, G. A., 349, 353, 380, 383
Fox, R. E., 238, 265
Fox, W. C., 301, 303, 308
Frankmann, Judith P., 365, 372 f., 376, 380, 405, 408, 413
Franks, P. E., 363, 382
Fredericson, E., 109, 136
Freedman, S. J., 398, 401, 414
Freeman, F. N., 506, 526
Freeman, G. L., 387, 414
Freire-Maia, A., 111, 134
Freire-Maia, N., 111, 134
French, G. M., 43, 84
French, J. R. P., Jr., 560, 569
French, T. M., 469, 497
Frenkel-Brunswik, E., 567
Freud, Anna, 513, 525
Freud, S., 468 ff., 479, 497, 499, 512 ff., 525, 569
Frick, F. C., 293 f., 297 ff., 306
Frick, J. W., 423, 442 f., 456 f.
Frolov, Y. P., 149, 176
Fromm, E., 514, 526
Fromm-Reichmann, Frieda, 515, 525
Fuller, J. L., 93, 107, 109, 113 f., 116, 119 f., 123, 126, 134, 136
Fullerton, G. S., 280, 306

Gaffron, M., 287, 309
Gage, N., 562, 569
Gagné, R. M., 345, 380
Gaito, J., 323, 341
Galambos, R., 52, 86
Galanter, E. H., 23, 25, 32, 66, 69, 85, 186, 389, 416, 457
Galton, F., 419, 448, 456
Garafolo, L., 225, 265
Gardner, R. W., 552, 569
Gardner, W. J., 333, 341
Garfinkel, H., 389, 391, 414
Garlich, M. M., 237, 263
Garmezy, N., 507, 520, 526
Garner, W. R., 8, 14, 20, 22, 25, 30 ff., 187, 219, 282, 286, 295, 297, 306 f., 322 f., 341, 347, 352, 379
Garrod, A. E., 129
Gastaut, H., 53, 84
Gault, F. P., 225, 265
Gautier, M., 135
Gay, B. R., 239, 264
Gazenko, O. G., 402, 414
Gebhard, J. W., 346, 382
Gebhard, P. H., 507, 526
Geldard, F. A., 386, 414
Gentry, G. V., 157, 176
Gerall, A. A., 363, 379
Gerard, H. B., 568
Gerathewohl, S. J., 402 ff., 414
German, J. L., 127, 134
Gerry, R., 452, 456, 459
Gerstman, L. S., 297, 307
Gerwirtz, J. L., 540 f., 569
Getzels, J. W., 427, 432, 449, 456
Ghent, L., 60, 88, 250, 265
Ghiselin, B., 458
Gibbs, C. B., 363, 380
Gibson, Eleanor J., 189, 203, 219, 320, 343
Gibson, J. J., 189, 219, 276, 307, 313, 318 ff., 326 f., 341, 387, 390, 394 f., 414
Gill, K., Jr., 239, 264
Gilman, A., 227, 265
Ginsberg, R., 199, 221
Ginsburg, B. E., 128, 134
Glad, D. D., 502, 526
Gladstone, A. I., 254 f., 265
Glees, P., 47, 57, 83 f.
Gleitman, H., 49, 89, 193, 220
Glenn, J. R., Jr., 396, 402, 414
Glickman, S. E., 240, 265
Glorig, A., 279, 307
Glover, E., 474, 497
Goddard, H. H., 506, 526
Goffman, E., 561, 569
Goldberg, J. M., 48, 84
Goldberger, L., 398, 400 f., 414 f.
Goldfarb, Joyce L., 333, 341
Goldfarb, W., 535, 569
Goldfried, M. R., 401, 414
Goldman-Eisler, Frieda, 473 ff., 497
Goldring, S., 81, 86
Goldstein, M. J., 518, 526
Goldstone, S., 333, 336, 341
Gonzalez, R. C., 157, 167 ff., 171, 173, 176
Goode, H. H., 345, 368, 380
Goodnow, J. J., 171, 176
Goodrich, H. B., 449, 457
Goodrich, K. P., 232, 265
Gordon, W. J. J., 442, 445, 448, 456
Gorham, D. R., 504, 527
Goss, A. E., 189, 219
Gougerot, L., 404, 414
Graf, V., 171 f., 176
Graff, H., 55, 84
Graham, C. H., 93, 134, 288 f., 307, 313, 341
Graham, E., 554, 570
Graham, Elaine H., 328, 341
Graham, Norah E., 349, 380
Granich, L., 223, 265
Grant, D. A., 252, 269
Grastyán, E., 53, 84
Graveline, D. E., 403, 414
Gray, P. H., 536, 569
Graybiel, A., 403, 414, 417
Green, B. F., 8, 32
Green, R. F., 363, 379
Greenberg, I., 229, 265
Greenblatt, M., 61, 84, 398, 414
Greenspoon, J., 209, 219
Gregersen, M. I., 226, 266
Grether, W. F., 347 ff., 352, 379 f.
Grice, G. R., 159 f., 177, 257, 265
Grindley, G. C., 150, 177
Grissom, R. J., 314, 343
Gross, C. G., 50 f., 84, 89

Grossberg, J. M., 230, 265
Grosser, G. S., 44, 84
Grossman, M. I., 226, 263, 266
Grotjahn, M., 431, 436, 456
Grunebaum, H. U., 398, 414
Grunzke, M. K., 415
Guetzkow, H., 367, 369, 380, 560, 569
Guilford, J. P., 20, 32, 274, 276, 278, 307, 315, 341, 422 f., 425, 432, 434 f., 437 f., 442, 447, 453, 455 ff., 459
Gulamerian, N. B., 316, 341
Gulick, W. L., 398 f., 417
Gulliksen, H., 26, 32 f.
Gunderson, E. K. E., 401, 414
Guthrie, E. R., 143, 147 ff., 153 f., 169 f., 173, 177
Guthrie, T. C., 60, 89
Guttman, L., 11 ff., 26, 32

Haber, H., 404, 414
Haber, R. N., 183, 219, 314, 341
Hadamard, J. S., 428, 431, 436, 438, 456
Haefner, D., 569
Haggerty, M. E., 144, 177
Hake, H. W., 8, 20, 32, 189, 219, 295, 297 f., 306 f.
Hall, J. F., 223, f., 235, 238, 240, 246, 248, 250, 252, 266, 270
Hall, R., 542, 571
Hall, R. J., 395, 416
Hall, T. J., 398, 412
Hallaran, W. R., 225, 269
Halsey, R. H., 297, 307
Halstead, W. C., 61, 84
Hammer, Lois R., 402, 414, 416
Hammes, J. A., 252 f., 266
Hancock, W., 262
Hand, V., 185, 220
Hanford, P., 240, 248, 266
Hansel, C. E. M., 331, 340
Harabedian, A., 405 ff., 409, 412, 414, 416
Harary, F., 547, 568
Hardesty, D., 407, 409, 414
Harding, H. F., 455, 458
Hardt, H. D., 353, 380 f.
Hare, A. P., 557, 569
Hargreaves, H. L., 423, 456
Harlow, H. F., 43, 49, 84, 88 f., 164, 168, 176 f., 240 f., 247, 264, 266, 493, 497, 506, 526, 535, 569
Harlow, Margaret K., 240, 266, 535, 569
Harmon, J. A., 459
Harris, C. S., 183, 219
Harris, H., 109, 129, 134
Harrison, J. M., 44, 84
Hartman, B. O., 358, 363, 380, 403, 414
Hartman, E. B., 298, 307
Hartman, H., 471, 497
Hartup, W. W., 540, 569
Harvey, O. J., 520, 526, 548, 552, 569
Hasek, J., 418
Haslerud, G. M., 49, 84
Hassler, R., 56 f., 84
Hauty, G. T., 397, 399 f., 413
Havighurst, R. J., 537, 568
Havron, M. D., 564, 569
Hawkes, G. R., 405, 416
Hayes, K. J., 199, 219
Hayes, W. H., 334, 342
Hayman, B. I., 120, 134
Haythorn, W., 563 ff., 569
Head, H., 314, 405
Hebb, D. O., 35 f., 62 ff., 70 f., 78 ff., 84, 223, 241, 261, 266, 277, 307, 377, 380, 385, 389, 392 f., 397, 401, 414, 506, 526
Hecht, Elizabeth, 409, 415
Hegel, G. W. F., 555, 569
Heider, F., 543 ff., 548, 551, 569
Heimer, W., 192, 198, 217, 221
Held, R., 398 f., 401, 414 f.
Hellyer, S., 184, 219
Helmholtz, H. von, 273, 289
Helson, H., 29, 31 f., 71, 84, 216, 219, 256, 261, 266, 284 ff., 307, 315 f., 321 f., 324, 326, 328 ff., 334 ff., 358, 361, 363 f., 367, 380, 409, 415, 492 f., 497
Helvey, W. M., 400, 416
Henderson, R. L., 168, 177, 231, 267
Hendler, E., 397, 413
Hendrix, C. E., 53, 82
Hendry, D. P., 231, 266
Henneman, R. H., 346, 380
Henry, J. P., 402, 404, 415
Hensen, Jean B., 362, 381
Herbart, J. F., 274
Heriot, J. T., 59, 84
Hermans, T. G., 332, 342
Hernàndez-Peòn, R., 52, 84, 313 ff., 342
Heron, W., 241, 263, 299, 307, 397 ff., 412 f., 415, 417
Herrmann, T., 316, 342
Herron, W. G., 519, 526
Herter, K., 119
Hess, W. R., 53, 84
Hetherington, A. W., 226, 266
Heuser, G., 57, 82
Hickson, R. H., 171, 177
High, W. S., 279, 307
Hilgard, E. R., 164, 177, 250, 264, 386, 415, 431, 456, 473, 497
Hill, H., 369, 380
Hill, J. H., 249, 270
Hill, W. F., 243 f., 266
Hillman, B., 251 f., 254, 266
Hillner, K., 185, 220
Hinde, R. A., 91 f., 134
Hirsch, J., 113 f., 121 f., 128, 133 f.
Hitchcock, F. A., 248, 266
Hobbes, T., 556, 569
Hobhouse, L. T., 144 ff., 177
Hochberg, J. E., 394, 415
Hodge, C. C., 239, 264
Hoebel, B. G., 55, 84
Hoffa, H. E., 456
Hoffman, A. C., 373, 380
Hoffman, J., 397 f., 417
Hoffman, R., 564, 569
Holland, J. G., 362, 381, 407, 415
Holmes, J. H., 226, 266
Holmes, S. J., 146, 177
Holt, E. B., 387, 415
Holt, R. R., 398, 400 f., 414 f.
Holway, A. N., 376, 381
Holzinger, K. J., 506, 526
Holzman, P. S., 552, 569
Honzik, C. H., 151 f., 158 ff., 165, 177, 179
Hopkins, B. L., 198, 219
Hopkins, C. O., 235, 266
Hopkins, T. K., 530, 572
Horenstein, B. R., 249, 266
Horney, Karen, 469, 514, 526
Horowitz, L., 231, 267
Horowitz, L. M., 192, 219
Horowitz, M., 546, 560, 569
Horwitt, M. K., 511, 526
Houston, J. P., 436, 457
Houston, R. C., 349, 381 f.
Hovland, C. I., 205, 219, 548, 557 ff., 569
Howard, T. C., 229, 266
Howell, W. C., 365 f., 379
Howland, D., 364, 373, 381
Hubbert, H. B., 150, 177
Hubel, D. H., 44, 84
Huff, F. W., 287, 307
Hugelin, A., 52, 84
Huggins, W. H., 345, 380
Hull, C., 57, 82
Hull, C. L., 80, 84, 133, 135, 150, 152 ff., 158 f., 162, 165 ff., 170, 174, 177, 193, 213, 219, 228, 231 ff., 253 ff., 258, 265 f., 375, 381, 387, 415, 539, 569
Hulse, S. H., 232, 270
Hume, D., 273, 312
Humes, J. M., 373 ff., 377 f., 406, 412
Humphreys, L. G., 509, 526
Hunt, D. E., 520, 526
Hunt, E. E., 552, 569
Hunt, J. McV., 231, 266, 506, 526
Hunter, R. S., 320, 342
Hunter, W. S., 144 f., 162, 203, 205, 220, 251 f., 254, 266
Hurst, R. W., 239, 264
Hurvich, L. M., 376, 381
Hurwitz, H. M. B., 231, 266
Husserl, E. G., 315, 342
Hutt, M. L., 560, 569
Huxley, T. H., 312, 342

Hydén, H., 58, 84, 131 f., 135
Hyman, R., 298, 307, 441, 454, 457

Ichheiser, G., 545, 569
Irion, A. L., 182, 189, 206 ff., 213, 220, 230, 267, 440, 457
Irwin, J. M., 209, 220
Isaacson, R. L., 51, 84
Ittelson, W. H., 319, 342, 396, 415

Jackson, B., 248, 270
Jackson, C. W., 398 ff., 415 f.
Jackson, D. D., 504, 507, 511, 526
Jackson, K. F., 365, 367, 381
Jackson, P. W., 427, 432, 449, 456
Jacobs, A., 229, 263
Jacobsen, C. F., 49, 84
Jaffe, R., 328 f., 342
James, W., 274, 307
Janis, I. L., 549, 554, 557 ff., 569 f.
Janowitz, H. D., 226, 266 f.
Jarvik, L. F., 125, 127, 135
Jarvik, M. E., 59, 87
Jasper, H. H., 43, 53, 86, 88, 376, 383
Jastrow, J., 274
Jeantheau, G., 373, 381
Jenkins, H. M., 373 f., 377, 381, 405 f., 415
Jenkins, W. O., 237, 267
Jensen, A. R., 205, 207, 219
Jensen, G. D., 240, 265
Jerison, H. J., 373, 381, 404 ff., 415
Jervis, G. A., 134
Jessor, R., 36, 84, 495, 498
Jinks, J. L., 120, 134
Johannsen, W. J., 518, 526
John, E. R., 53, 83 f.
Johns, M. D., 216, 219
Johnson, B. E., 382
Johnson, D. M., 435, 444, 457
Johnson, E. E., 160, 177
Johnson, R. C., 191, 219
Johnson, W., 482, 498
Johnston, R. S., 415
Jones, A., 242 f., 267
Jones, C. A., 430, 457
Jones, C. B., 550, 568
Jones, E. E., 560, 562, 570
Jones, L. V., 31 f.
Jones, M. R., 527
Jones, R. E., 349, 380
Jordan, N., 545, 570
Jouvet, M., 52, 84
Joy, V., 326, 329, 342
Joyce, J., 494, 498
Judd, B. R., 186, 218
Jukes, C. L., 539, 570
Ju Lin, H., 226, 268
Jung, C. G., 469, 555, 570
Jung, R., 56 f., 84

Kaada, B. R., 52, 84
Kagan, J., 231, 267
Kahn, A., 362, 379
Kalish, H. I., 264
Kallmann, F. J., 125 ff., 135, 506 ff., 526
Kalow, W., 117, 135
Kalsbeck, J. E., 47, 83
Kama, W. N., 402 f., 415 f.
Kanareff, V. T., 541 ff., 570
Kandel, E. J., 398, 415
Kanner, L., 520, 526
Kantor, J. R., 467, 489, 498
Kappauf, W. E., 355, 381, 407, 415
Kardiner, A., 469
Karlin, L., 406, 415
Kas’ian, I. I., 403, 418
Ka’swan, J., 192, 219
Katz, D., 313, 315 f., 321, 342
Kaufman, R. A., 187, 218
Kaufmann, Felix, 530, 570
Keller, L., 216, 219
Kellogg, W. N., 250, 267
Kelly, E. L., 398 f., 415
Kelly, G. A., 502, 526
Kelly, H. H., 556 ff., 569, 572
Kelman, H., 549, 570
Kendler, H. H., 174, 177, 189, 220
Keppel, G., 198, 221
Kerrick, Jean S., 545, 553
Kessen, M. L., 233, 263, 268
Kessen, W., 251, 267
Kettlewell, H. B. D., 130, 135
Kettner, N. W., 423, 457
Kety, S. S., 511, 526
Killam, K. F., 53, 84
Kimble, G. A., 230 f., 251 f., 254, 266 f.
Kimbrell, D., 542, 570
Kimura, D., 397, 413
Kincaid, C. E., 436, 457
King, B., 549, 569 f.
King, B. G., 403, 415
King, F. A., 51, 84
King, G., 503, 526
King, J. A., 92, 109, 135
King, R. A., 238, 265
King, S. M., 331, 342
King, W. J., 365, 381
Kinnaman, A. J., 144 f., 168, 177
Kinsey, A., 507, 526
Kirk, R. E., 409, 415
Kirman, J. H., 230, 264
Kish, G. B., 231, 263, 267
Kivy, P., 236 f., 264
Klein, G. S., 552, 569
Klein, I. C., 405, 408 f., 412
Klein, R. M., 237, 267
Klemmer, E. T., 298, 307, 406, 415
Klett, C. J., 504, 526
Klineberg, O., 111, 117, 135
Kling, J. W., 231, 267
Kluckhohn, Clyde, 536, 570
Klüver, H., 43, 85
Knaff, P. R., 373, 382, 407, 417
Knapp, R. H., 448, 457
Knarr, F. A., 232, 264
Kobrick, J. L., 250 f., 266 f., 320, 341
Koch, S., 32, 253, 267, 269, 527
Koffka, K., 315, 330, 342, 388, 394, 396, 415
Kogan, N., 563, 572
Köhler, W., 81, 85, 147, 177, 192, 220, 316, 327 f., 342, 387, 415, 436
Kohn, M., 233, 267
Konorski, J., 50, 62, 85, 164, 177
Kornberg, A., 135
Kraeling, D., 254, 264
Kraepelin, E., 502, 505, 526
Kraft, M. S., 43, 85
Krasne, F. B., 55, 85
Krasner, L., 540, 570
Krauskopf, J., 163, 176
Krech, C., 389, 413, 415
Krech, D., 58, 61, 85, 87, 128, 135, 529, 570
Krechevsky, I., 157, 177
Kretschmer, E., 508, 526
Kris, E., 431, 440, 449, 457, 471, 498
Krueckel, B. A., 135
Kruger, L., 40, 47, 57, 85, 87
Kubie, L., 401, 415
Kubie, L. S., 431, 448, 457
Kubzansky, P. E., 242, 270, 401, 415, 417
Kuffler, S. W., 4, 32
Kurtz, K. H., 189, 220
Kveim, K., 225, 265

Lachman, R., 236 f., 264
Ladd, G. T., 324, 342
Lambert, W. E., 529, 533, 536, 570
Lambert, W. W., 529, 533, 536, 570, 572
Lamoreaux, R. R., 162, 178
Landsman, H. J., 214, 220
Langdon-Down, J., 127
Langham, P., 569
Lansford, T. G., 198, 218
Lanzetta, J. T., 541 ff., 570
Larsen, R. M., 44, 83
Larsson, S., 229, 263
Lashley, K. S., 41, 43, 45, 62, 66, 78, 85, 132, 145, 150, 156 ff., 177, 273, 434
Lasky, J. J., 504, 526
Lauer, D. W., 171
Laughlin, P. C., 415
Lavin, K., 566, 570
Lawrence, D. H., 156 f., 177, 183, 220, 249, 267
Lawson, R., 235, 267
Lazarsfeld, P. F., 12, 26, 32, 558, 570
Lazarus, R. S., 246, 267
Lebow, K. E., 234, 269

Lee, M. F., 239, 264
Lefkowitz, M. M., 542, 570
Lehman, H. C., 449, 451, 457
Lehman, I. R., 100, 135
Lehr, D. J., 405 ff., 412
Lehrman, D. S., 102 f., 109, 135
Leibnitz, G. W., 274
Leiderman, P. H., 270, 399, 417
Lejeune, T., 127, 135
Lengyel, P., 136
Leon, H. V., 399, 412
Lerner, I. M., 107, 114, 135
Lesser, G. S., 522, 526, 553, 567
Levan, A., 126, 136
Levin, H., 232, 265, 571
Levine, M., 361, 381
Levine, M. V., 200
Levinson, D. J., 567
Levy, D. M., 478, 498, 537, 570
Levy, E. Z., 398, 401, 417
Lewin, K., 388, 390, 416, 467, 489 f., 498, 503, 526, 566, 570
Lewis, D., 283, 307
Lewis, D. J., 54, 59, 82 f., 251, 254 f., 267, 459
Licht, L., 457
Licklider, J. C. R., 333, 341, 368, 381
Lilly, J., 47, 56, 85
Lilly, J. C., 397, 401, 416
Lincoln, R. S., 360 f., 381
Lindsley, D. B., 313 f., 342, 374, 376, 378, 381, 401, 416
Lindzey, G., 116, 135
Linton, H. B., 552, 554, 569 f.
Linton, R., 467, 498
Lippitt, R., 159 f., 178, 566, 570
Liverant, S., 503, 506, 513, 522 f., 526 f.
Livingston, J. R., 232, 266
Livingstone, R. B., 81, 85
Locke, J., 273, 311, 342
Lodahl, T. M., 231, 268
Loeb, M., 373, 381, 405, 416
Loftus, J. P., 402, 416
Logan, F. A., 255, 267
Loiselle, R. H., 456
Long, C. N. H., 55, 82, 226, 263
Long, E. R., 346, 380
Longo, N., 171, 177
Lord, Edith, 504, 526
Lorente de Nó, R., 63, 85
Lorge, I., 135
Lorr, M., 504, 526
Lotze, R., 273
Loucks, R. B., 355, 381
Lovaas, O. I., 542, 570
Loveless, N. E., 406, 416
Low, L., 240, 248, 266
Lowell, E. L., 267, 342, 515, 526, 570
Lowenfeld, V., 428, 457
Lucas, K., 273
Luchins, A. S., 443, 457
Luchins, E. H., 443, 457
Ludvigh, E. J., 394, 416
Ludwig, M., 537, 571
Lutz, R. E., 365, 381
Lykken, D. T., 135
Lyons, J., 546, 569

Maccoby, E. E., 571
MacCorquodale, K., 154, 177, 275, 307
MacDonnell, M. F., 228, 269
Mach, E., 316, 342
MacKinnon, D. W., 432, 449, 457
MacLean, P. D., 51, 85
Machol, R. E., 345, 368, 380
Mackworth, Jane F., 183, 220, 407, 416
Mackworth, N. H., 372 f., 375, 378, 381, 405, 407 f., 416
Maddi, S. R., 397, 413
Madow, W. G., 295, 307
Madsen, K. B., 258, 267
Margaret, Ann, 509, 525
Magendie, F., 273
Magoun, H. W., 52, 85, 401
Mahatoo, W., 398, 413
Maher, P. H., 415
Mahrer, A. R., 493, 498
Mahut, H., 56, 85
Maier, N. R. F., 161, 174, 177, 439 ff., 443, 457, 564, 569
Maier, R. A., 334, 340
Malis, J. L., 54, 82
Malmo, R. B., 49, 85, 376 f., 381
Maltzman, I., 433, 444, 453 f., 457
Manley, R., 412
Manning, A., 106, 113, 134 f.
March, J. G., 531, 570
Margules, D. L., 54, 85
Mark, J. C., 507, 526
Marlowe, D., 496 f.
Marquis, D. G., 164, 177
Marsh, J. T., 52, 86
Marshall, L. M., 287, 308
Marshall, W. H., 43, 88
Marton, T., 399, 417
Marx, M. H., 168, 177, 231 f., 245, 264, 267, 269
Masling, J., 503 f., 526
Maslow, A. H., 431, 457
Mason, D. J., 236, 267
Mason, W. A., 249, 267
Masters, H. G., 338 f.
Matheny, W. G., 347 ff., 352 f., 364, 380 ff.
Mather, K., 120, 134
Matthaei, J. H., 101, 136
Mayer, J., 225 f., 267 f.
Mayr, E., 91, 130, 135
McAllister, W. R., 160, 177
McBride, D. A., 258, 264
McCann, S. M., 227, 263
McClearn, G. E., 116, 136
McCleary, R. A., 51, 85
McClelland, C., 538, 570
McClelland, D. C., 228, 246, 261 ff., 267, 314 f., 342, 515, 526
McCormack, P. D., 406, 408, 416
McCormick, E. J., 347, 381
McCrary, J. W., 203, 205, 220
McDaniel, M. H., 239, 264
McFarland, R. A., 376, 381
McFarland, W. L., 230, 270
McGaugh, J. L., 59, 82, 128, 135
McGeoch, J. A., 182, 206 ff., 213, 230, 267, 440, 457
McGill, T. E., 398 ff., 417
McGill, W. J., 295, 297, 307
McGrath, J. E., 569
McGrath, J. J., 377, 381, 405 f., 409, 413 f., 416
McGuigan, F. J. 239, 267
McGuire, W. J., 189, 220
McKenna, V., 193, 220
McKenzie, R. E., 403, 414
McLean, F. C., 431, 457
McNair, D. M., 504, 526
Mead, G. H., 387, 416, 467, 498
Mead, L. C., 373, 380
Meadow, A., 441, 444, 452 f., 457 f.
Medawar, P. B., 106, 135
Médioni, J., 123, 135
Mednick, S. A., 419, 433 f., 436, 457, 519, 526
Meehl, P. E., 154, 177, 275, 307, 494, 498
Mehlman, B., 503, 526
Meier, G. W., 287, 307
Melton, A. W., 183 f., 220, 224, 267, 356, 381
Melzack, R., 533, 570
Mendel, G., 94
Mendelson, J. H., 270, 399, 417
Mengelkoch, R. F., 349, 381 f.
Menzel, E. W., Jr., 240, 267
Merkel, J., 274
Merrifield, P. R., 425, 435, 437, 456 f.
Merton, R. K., 558, 570
Meselson, M., 100, 136
Messick, S., 32 f.
Metfessel, M., 23, 32
Metzger, R., 255, 267
Metzger, W., 394, 416
Meyer, D. R., 43, 85, 163, 178, 240, 251 f., 255, 266, 268
Michels, K. M., 322 f., 342
Michels, W. C., 26, 29, 31 f., 286, 307
Michotte, A., 390, 416
Mihailovič, L., 50, 89
Miles, R. C., 235, 238, 252, 268
Mill, J., 311
Miller, A., 184, 220
Miller, G. A., 66, 69, 85, 182, 187, 220, 282, 294 f., 297 ff., 307, 323, 342, 389, 416, 434, 457

Hydén, H., 58, 84, 131 f., 135
Hyman, R., 298, 307, 441, 454, 457

Ichheiser, G., 545, 569
Irion, A. L., 182, 189, 206 ff., 213, 220, 230, 267, 440, 457
Irwin, J. M., 209, 220
Isaacson, R. L., 51, 84
Ittelson, W. H., 319, 342, 396, 415

Jackson, B., 248, 270
Jackson, C. W., 398 ff., 415 f.
Jackson, D. D., 504, 507, 511, 526
Jackson, K. F., 365, 367, 381
Jackson, P. W., 427, 432, 449, 456
Jacobs, A., 229, 263
Jacobsen, C. F., 49, 84
Jaffe, R., 328 f., 342
James, W., 274, 307
Janis, I. L., 549, 554, 557 ff., 569 f.
Janowitz, H. D., 226, 266 f.
Jarvik, L. F., 125, 127, 135
Jarvik, M. E., 59, 87
Jasper, H. H., 43, 53, 86, 88, 376, 383
Jastrow, J., 274
Jeantheau, G., 373, 381
Jenkins, H. M., 373 f., 377, 381, 405 f., 415
Jenkins, W. O., 237, 267
Jensen, A. R., 205, 207, 219
Jensen, G. D., 240, 265
Jerison, H. J., 373, 381, 404 ff., 415
Jervis, G. A., 134
Jessor, R., 36, 84, 495, 498
Jinks, J. L., 120, 134
Johannsen, W. J., 518, 526
John, E. R., 53, 83 f.
Johns, M. D., 216, 219
Johnson, B. E., 382
Johnson, D. M., 435, 444, 457
Johnson, E. E., 160, 177
Johnson, R. C., 191, 219
Johnson, W., 482, 498
Johnston, R. S., 415
Jones, A., 242 f., 267
Jones, C. A., 430, 457
Jones, C. B., 550, 568
Jones, E. E., 560, 562, 570
Jones, L. V., 31 f.
Jones, M. R., 527
Jones, R. E., 349, 380
Jordan, N., 545, 570
Jouvet, M., 52, 84
Joy, V., 326, 329, 342
Joyce, J., 494, 498
Judd, B. R., 186, 218
Jukes, C. L., 539, 570
Ju Lin, H., 226, 268
Jung, C. G., 469, 555, 570
Jung, R., 56 f., 84

Kaada, B. R., 52, 84
Kagan, J., 231, 267
Kahn, A., 362, 379
Kalish, H. I., 264
Kallmann, F. J., 125 ff., 135, 506 ff., 526
Kalow, W., 117, 135
Kalsbeck, J. E., 47, 83
Kama, W. N., 402 f., 415 f.
Kanareff, V. T., 541 ff., 570
Kandel, E. J., 398, 415
Kanner, L., 520, 526
Kantor, J. R., 467, 489, 498
Kappauf, W. E., 355, 381, 407, 415
Kardiner, A., 469
Karlin, L., 406, 415
Kas'ian, I. I., 403, 418
Ka'swan, J., 192, 219
Katz, D., 313, 315 f., 321, 342
Kaufman, R. A., 187, 218
Kaufmann, Felix, 530, 570
Keller, L., 216, 219
Kellogg, W. N., 250, 267
Kelly, E. L., 398 f., 415
Kelly, G. A., 502, 526
Kelly, H. H., 556 ff., 569, 572
Kelman, H., 549, 570
Kendler, H. H., 174, 177, 189, 220
Keppel, G., 198, 221
Kerrick, Jean S., 545, 553
Kessen, M. L., 233, 263, 268
Kessen, W., 251, 267
Kettlewell, H. B. D., 130, 135
Kettner, N. W., 423, 457
Kety, S. S., 511, 526
Killam, K. F., 53, 84
Kimble, G. A., 230 f., 251 f., 254, 266 f.
Kimbrell, D., 542, 570
Kimura, D., 397, 413
Kincaid, C. E., 436, 457
King, B., 549, 569 f.
King, B. G., 403, 415
King, F. A., 51, 84
King, G., 503, 526
King, J. A., 92, 109, 135
King, R. A., 238, 265
King, S. M., 331, 342
King, W. J., 305, 381
Kinnaman, A. J., 144 f., 168, 177
Kinsey, A., 507, 526
Kirk, R. E., 409, 415
Kirman, J. H., 230, 264
Kish, G. B., 231, 263, 267
Kivy, P., 236 f., 264
Klein, G. S., 552, 569
Klein, I. C., 405, 408 f., 412
Klein, R. M., 237, 267
Klemmer, E. T., 298, 307, 406, 415
Klett, C. J., 504, 526
Klineberg, O., 111, 117, 135
Kling, J. W., 231, 267
Kluckhohn, Clyde, 536, 570
Klüver, H., 43, 85
Knaff, P. R., 373, 382, 407, 417
Knapp, R. H., 448, 457
Knarr, F. A., 232, 264
Kobrick, J. L., 250 f., 266 f., 320, 341
Koch, S., 32, 253, 267, 269, 527
Koffka, K., 315, 330, 342, 388, 394, 396, 415
Kogan, N., 563, 572
Köhler, W., 81, 85, 147, 177, 192, 220, 316, 327 f., 342, 387, 415, 436
Kohn, M., 233, 267
Konorski, J., 50, 62, 85, 164, 177
Kornberg, A., 135
Kraeling, D., 254, 264
Kraepelin, E., 502, 505, 526
Kraft, M. S., 43, 85
Krasne, F. B., 55, 85
Krasner, L., 540, 570
Krauskopf, J., 163, 176
Krech, C., 389, 413, 415
Krech, D., 58, 61, 85, 87, 128, 135, 529, 570
Krechevsky, I., 157, 177
Kretschmer, E., 508, 526
Kris, E., 431, 440, 449, 457, 471, 498
Krueckel, B. A., 135
Kruger, L., 40, 47, 57, 85, 87
Kubie, L., 401, 415
Kubie, L. S., 431, 448, 457
Kubzansky, P. E., 242, 270, 401, 415, 417
Kuffler, S. W., 4, 32
Kurtz, K. H., 189, 220
Kveim, K., 225, 265

Lachman, R., 236 f., 264
Ladd, G. T., 324, 342
Lambert, W. E., 529, 533, 536, 570
Lambert, W. W., 529, 533, 536, 570, 572
Lamoreaux, R. R., 162, 178
Landsman, H. J., 214, 220
Langdon-Down, J., 127
Langham, P., 569
Lansford, T. G., 198, 218
Lanzetta, J. T., 541 ff., 570
Larsen, R. M., 44, 83
Larsson, S., 229, 263
Lashley, K. S., 41, 43, 45, 62, 66, 78, 85, 132, 145, 150, 156 ff., 177, 273, 434
Lasky, J. J., 504, 526
Lauer, D. W., 171
Laughlin, P. C., 415
Lavin, K., 566, 570
Lawrence, D. H., 156 f., 177, 183, 220, 249, 267
Lawson, R., 235, 267
Lazarsfeld, P. F., 12, 26, 32, 558, 570
Lazarus, R. S., 246, 267
Lebow, K. E., 234, 269

Lee, M. F., 239, 264
Lefkowitz, M. M., 542, 570
Lehman, H. C., 449, 451, 457
Lehman, I. R., 100, 135
Lehr, D. J., 405 ff., 412
Lehrman, D. S., 102 f., 109, 135
Leibnitz, G. W., 274
Leiderman, P. H., 270, 399, 417
Lejeune, T., 127, 135
Lengyel, P., 136
Leon, H. V., 399, 412
Lerner, I. M., 107, 114, 135
Lesser, G. S., 522, 526, 553, 567
Levan, A., 126, 136
Levin, H., 232, 265, 571
Levine, M., 361, 381
Levine, M. V., 200
Levinson, D. J., 567
Levy, D. M., 478, 498, 537, 570
Levy, E. Z., 398, 401, 417
Lewin, K., 388, 390, 416, 467, 489 f., 498, 503, 526, 566, 570
Lewis, D., 283, 307
Lewis, D. J., 54, 59, 82 f., 251, 254 f., 267, 459
Licht, L., 457
Licklider, J. C. R., 333, 341, 368, 381
Lilly, J., 47, 56, 85
Lilly, J. C., 397, 401, 416
Lincoln, R. S., 360 f., 381
Lindsley, D. B., 313 f., 342, 374, 376, 378, 381, 401, 416
Lindzey, G., 116, 135
Linton, H. B., 552, 554, 569 f.
Linton, R., 467, 498
Lippitt, R., 159 f., 178, 566, 570
Liverant, S., 503, 506, 513, 522 f., 526 f.
Livingston, J. R., 232, 266
Livingstone, R. B., 81, 85
Locke, J., 273, 311, 342
Lodahl, T. M., 231, 268
Loeb, M., 373, 381, 405, 416
Loftus, J. P., 402, 416
Logan, F. A., 255, 267
Loiselle, R. H., 456
Long, C. N. H., 55, 82, 226, 263
Long, E. R., 346, 380
Longo, N., 171, 177
Lord, Edith, 504, 526
Lorente de Nó, R., 63, 85
Lorge, I., 135
Lorr, M., 504, 526
Lotze, R., 273
Loucks, R. B., 355, 381
Lovaas, O. I., 542, 570
Loveless, N. E., 406, 416
Low, L., 240, 248, 266
Lowell, E. L., 267, 342, 515, 526, 570
Lowenfeld, V., 428, 457
Lucas, K., 273
Luchins, A. S., 443, 457
Luchins, E. H., 443, 457
Ludvigh, E. J., 394, 416
Ludwig, M., 537, 571
Lutz, R. E., 365, 381
Lykken, D. T., 135
Lyons, J., 546, 569

Maccoby, E. E., 571
MacCorquodale, K., 154, 177, 275, 307
MacDonnell, M. F., 228, 269
Mach, E., 316, 342
MacKinnon, D. W., 432, 449, 457
MacLean, P. D., 51, 85
Machol, R. E., 345, 368, 380
Mackworth, Jane F., 183, 220, 407, 416
Mackworth, N. H., 372 f., 375, 378, 381, 405, 407 f., 416
Maddi, S. R., 397, 413
Madow, W. G., 295, 307
Madsen, K. B., 258, 267
Margaret, Ann, 509, 525
Magendie, F., 273
Magoun, H. W., 52, 85, 401
Mahatoo, W., 398, 413
Maher, P. H., 415
Mahrer, A. R., 493, 498
Mahut, H., 56, 85
Maier, N. R. F., 161, 174, 177, 439 ff., 443, 457, 564, 569
Maier, R. A., 334, 340
Malis, J. L., 54, 82
Malmo, R. B., 49, 85, 376 f., 381
Maltzman, I., 433, 444, 453 f., 457
Manley, R., 412
Manning, A., 106, 113, 134 f.
March, J. G., 531, 570
Margules, D. L., 54, 85
Mark, J. C., 507, 526
Marlowe, D., 496 f.
Marquis, D. G., 164, 177
Marsh, J. T., 52, 86
Marshall, L. M., 287, 308
Marshall, W. H., 43, 88
Marton, T., 399, 417
Marx, M. H., 168, 177, 231 f., 245, 264, 267, 269
Masling, J., 503 f., 526
Maslow, A. H., 431, 457
Mason, D. J., 236, 267
Mason, W. A., 249, 267
Masters, H. G., 338 f.
Matheny, W. G., 347 ff., 352 f., 364, 380 ff.
Mather, K., 120, 134
Matthaei, J. H., 101, 136
Mayer, J., 225 f., 267 f.
Mayr, E., 91, 130, 135
McAllister, W. R., 160, 177
McBride, D. A., 258, 264
McCann, S. M., 227, 263
McClearn, G. E., 116, 136
McCleary, R. A., 51, 85
McClelland, C., 538, 570
McClelland, D. C., 228, 246, 261 ff., 267, 314 f., 342, 515, 526
McCormack, P. D., 406, 408, 416
McCormick, E. J., 347, 381
McCrary, J. W., 203, 205, 220
McDaniel, M. H., 239, 264
McFarland, R. A., 376, 381
McFarland, W. L., 230, 270
McGaugh, J. L., 59, 82, 128, 135
McGeoch, J. A., 182, 206 ff., 213, 230, 267, 440, 457
McGill, T. E., 398 ff., 417
McGill, W. J., 295, 297, 307
McGrath, J. E., 569
McGrath, J. J., 377, 381, 405 f., 409, 413 f., 416
McGuigan, F. J. 239, 267
McGuire, W. J., 189, 220
McKenna, V., 193, 220
McKenzie, R. E., 403, 414
McLean, F. C., 431, 457
McNair, D. M., 504, 526
Mead, G. H., 387, 416, 467, 498
Mead, L. C., 373, 380
Meadow, A., 441, 444, 452 f., 457 f.
Medawar, P. B., 106, 135
Médioni, J., 123, 135
Mednick, S. A., 419, 433 f., 436, 457, 519, 526
Meehl, P. E., 154, 177, 275, 307, 494, 498
Mehlman, B., 503, 526
Meier, G. W., 287, 307
Melton, A. W., 183 f., 220, 224, 267, 356, 381
Melzack, R., 533, 570
Mendel, G., 94
Mendelson, J. H., 270, 399, 417
Mengelkoch, R. F., 349, 381 f.
Menzel, E. W., Jr., 240, 267
Merkel, J., 274
Merrifield, P. R., 425, 435, 437, 456 f.
Merton, R. K., 558, 570
Meselson, M., 100, 136
Messick, S., 32 f.
Metfessel, M., 23, 32
Metzger, R., 255, 267
Metzger, W., 394, 416
Meyer, D. R., 43, 85, 163, 178, 240, 251 f., 255, 266, 268
Michels, K. M., 322 f., 342
Michels, W. C., 26, 29, 31 f., 286, 307
Michotte, A., 390, 416
Mihailovič, L., 50, 89
Miles, R. C., 235, 238, 252, 268
Mill, J., 311
Miller, A., 184, 220
Miller, G. A., 66, 69, 85, 182, 187, 220, 282, 294 f., 297 ff., 307, 323, 342, 389, 416, 434, 457

Miller, J. G., 299, 307, 314, 342, 527
Miller, J. W., 394 f., 416
Miller, L. B., 255, 265, 268
Miller, N. E., 54 f., 59, 83, 85, 163, 173, 177, 227, 229 f., 233 f., 244, 263, 265, 268, 490, 498, 516, 525, 539, 542, 568, 570
Miller, O. J., 126
Mills, J., 550, 567, 571
Milner, B., 52, 60 f., 85 f
Milner, P., 232 f., 268, 490, 498
Milner, P. M., 54, 63, 85 f.
Miner, N., 43, 88
Miner, R. W., 225, 268
Mirsky, A. E., 132, 134
Mirsky, A. F., 51, 88
Mischel, W., 493, 498
Mishkin, M., 35, 44 ff., 49 f., 57, 61, 82 f., 86 ff.
Montgomery, K. C., 231, 240, 243, 268, 270
Montgomery, M. F., 226, 268
Moon, L. E., 231, 268
Moore, R. Y., 51, 84
Moreno, O. M., 54, 82
Morgan, C. L., 140 ff., 147 f., 150 f., 153, 159, 163, 166, 172 ff., 178
Morgan, C. T., 225, 268, 281, 283, 308, 347, 352, 379, 386, 416, 539, 571
Morgan, J. D., 225, 268
Morgan, R. L., 363, 382
Morikiyo, Y., 287, 307
Morrell, F., 43, 53, 58, 86
Morris, J. B., 239, 264
Morrison, S. D., 226, 268
Morse, E. B., 171, 178
Morton, N. E., 120, 136
Mosely, J. D., 415
Moskowitz, M. J., 248, 268
Moss, F. A., 224, 268
Mosteller, F., 80, 82, 196, 199, 218
Mountcastle, V. B., 46 f., 86 f.
Moushegian, G., 52, 86
Mouton, Jane, 338, 542 f., 570
Mowatt, Marian H., 318, 323, 342
Mowbray, G. H., 346, 382
Mowrer, O. H., 162 f., 169, 172 ff., 178, 244, 265, 405, 416, 516, 526, 539, 571
Moylan, J. J., 329, 342
Muckler, F. A., 187, 217, 358, 360, 364, 382, 521, 527
Muenzinger, K. K., 155, 178
Müller, G. E., 274, 279, 307
Müller, J., 273
Munn, N. L., 150, 178, 386, 416
Munroe, Ruth, 515, 526
Murdock, B. B., 184 f., 188, 192, 206, 220, 330, 342
Murphy, D. B., 398, 415
Murphy, D. P., 363, 379
Murphy, G., 386, 416
Murray, H. A., 474
Mussen, P. H., 542, 571
Myers, A. K., 229 f.
Myers, N. A., 252, 269
Myers, R. E., 43, 47, 83, 86, 88
Myers, T. I., 398, 415

Nachmias, J., 187, 193, 220, 330, 342
Naitoh, P., 58, 86
Narva, M. A., 351, 382
Nash, F. A., 239, 264
Nash, Myrtle C., 328, 342
Necheles, H., 226, 270
Neff, W. D., 48, 84, 86
Nellis, Barbara, 332, 342
Nelsen, E. A., 539, 571
Nelson, P. D., 401, 414
Newcomb, T. M., 467, 498, 545 ff., 556, 571
Newman, E. B., 297, 307
Newman, H. H., 506, 526
Newman, S. E., 189, 193, 220
Newton, J. M., 353 f., 383
Nichols, J. W., 92, 135
Nicholson, P. J., 452, 457
Nirenberg, M. W., 101, 136
Nissen, H. W., 49, 84, 240, 268, 319, 342
Nixon, J., 279, 307
Noble, C. E., 190 f., 220
Noble, M. E., 364, 381
Nolan, C. Y., 239, 270
Nottelman, D. A., 456
Novin, D., 229, 268

Obermayer, R. W., 358, 360, 382
Obrist, W., 43, 85
Ochoa, S., 136
O'Connell, D. N., 306
O'Kelly, L. I., 521, 527
Olds, J., 54 f., 58, 85 f., 88, 232 f., 268, 490, 498
Olds, M. E., 55 f., 86
O'Leary, J. L., 81, 86
Orbach, J., 43, 45, 47, 52, 86
Orlansky, J., 360 ff., 380
Ormiston, D. W., 398, 400, 416
Ormond, E., 373, 377, 380, 405 f., 413
Osborn, A. F., 441, 444 f., 452 f., 457
Osborn, W. C., 406, 416
Osborne, R. H., 125 f., 136
Oscar, M., 49 f., 89, 171, 179
Osgood, C. E., 545 ff., 553 f., 571
Overall, J. E., 504, 527
Owens, C., 452, 457

Pack, G. T., 226, 268
Paillas, N., 52, 84
Papez, J. W., 51, 86
Parducci, A., 287, 308
Parker, G. V. C., 190, 220
Parnes, S. J., 441, 452 f., 455, 457 f.
Pasik, P., 44, 47, 86
Pasik, T., 44, 86
Pastore, N., 507, 527
Paterson, D. G., 339, 342
Patrick, C., 434, 438 f., 458
Patterson, G. R., 537, 571
Patton, H. D., 56, 87
Pavlik, W. B., 256 f., 269
Pavlov, I. P., 61 f., 87, 149, 154, 156 f., 162, 178, 431
Payne, T. A., 351, 382
Peak, H., 224, 268
Pearlman, C. A., Jr., 59, 87
Peelar, D. F., 287, 307
Peltz, D. C., 565 f., 571
Penfield, W., 46, 60 f., 87
Penick, E. C., 245, 262
Pennock, L., 238, 269
Penny, R. K., 245, 268
Perin, C. T., 253, 257, 268
Perkins, C. C., Jr., 257, 269, 365, 381
Perlmutter, H. V., 546, 569
Perry, M. M., 136
Peters, J., 400, 416
Peterson, E., 399, 417
Peterson, L. R., 183 ff., 220
Peterson, M. J., 183 ff., 220
Peterson, W. W., 301, 303, 308
Petrie, A., 400, 416
Pettigrew, T. F., 552, 571
Pfaffmann, C., 48, 87, 231, 270, 291, 308, 315, 342
Phares, E. J., 503, 513, 518, 527
Philip, B. R., 285, 308
Phillips, L., 503 f., 519 f., 527
Piaget, J., 506
Pickford, R., 440, 458
Pickleman, J. R., 231, 264
Pierson, W. P., 135
Pigg, K. D., 402, 416
Pigon, A., 131, 135
Pinto-Hamuy, T., 49, 88
Pittendrigh, C. S., 130, 136
Plateau, J. A. F., 274
Plath, D. W., 354 f., 379
Poincaré, H., 439 f., 458
Poliak, S., 43, 87
Pollack, I., 297 f., 308, 373, 382, 407, 416
Pollard, J. C., 398 ff., 415 f., 510, 527
Pomeroy, W. B., 507, 526
Pores, E. B., 29, 33
Porter, J. P., 144, 178
Porter, P., 47, 85
Postman, L., 187, 192, 198, 208, 211, 213, 220, 389
Potter, M. C., 554, 568
Poulton, E. C., 29, 33, 358, 367, 382

Powe, W. E., 407, 415
Precker, J. A., 565, 571
Prell, D., 508, 525
Prentice, W. C. H., 192, 221
Pribram, K. H., 35 f., 40 f., 43 ff., 47, 57, 60, 66 ff., 71, 78 f., 81, 85 ff., 389, 416, 457
Pritchard, Joan F., 333, 336 f., 340
Prockop, E. S., 50, 86
Prokasy, W. F., Jr., 252, 269, 365, 382
Prokofiev, G., 164, 178
Prysiazniuk, A., 400, 406, 418
Pubols, B. H., Jr., 251, 269

Quastler, H., 298, 308
Quay, H. C., 231, 266
Quigley, J. P., 225, 269

Raab, D. H., 45, 82
Rabbie, J. M., 549, 571
Rabinowitz, H. S., 444, 455
Rakic, L., 57, 82
Rank, O., 480, 498
Ranson, S. W., 226, 266
Ranzoni, J. H., 440, 459
Rapaport, D., 471, 473, 498, 516, 527
Rasche, R. H., 231, 266
Rashevsky, N., 80, 87
Raskin, D., 457
Rasmussen, T., 46, 52, 60 f., 86 f.
Ratoosh, P., 93, 134, 313, 341
Rayman, N., 405, 416
Raynard, R., 209, 219
Reed, E. W., 123, 136
Reed, P., 163, 176
Reed, S. C., 123, 136
Rees, D. W., 403, 416
Rehula, R., 198, 221
Reid, L. S., 248, 250, 265
Reis, B. B., 226, 270
Reiss, B. F., 526
Rendel, J. M., 123, 136
Repucci, L. C., 430, 448, 458 f.
Restle, F., 189, 197, 200, 221
Rethlingshafer, D., 228, 269
Reynolds, G. S., 216, 221
Reynolds, R. W., 55, 87
Reynolds, W. F., 256 f., 269
Richardson, F., 145, 178
Richter, C. P., 132, 136, 223 f., 248, 269
Riecken, J., 551
Riesen, A. H., 299, 308, 314, 393, 397, 417
Riesz, R. R., 290, 308
Riley, D. A., 192, 221
Riopelle, A. J., 43, 87
Ritchie, B. F., 174, 178
Ritchie, M. L., 349, 382
Rixon, R. H., 248, 270
Roback, A. A., 388, 417
Roberts, C. L., 168, 177, 231, 234, 267, 269
Roberts, L., 43, 86
Roberts, W. A., 171, 176
Roberts, W. H., 150, 178
Roberts, W. W., 54, 83, 87, 234, 265, 269
Robertson, J. D., 37, 87
Robertson, M. H., 401, 417
Robinson, E. S., 182
Robinson, F., 57, 87
Robinson, H., 550, 571
Robinson, J. S., 231, 269, 334, 342
Roby, T. B., 231 f., 262, 267, 269
Rock, I., 192, 197 f., 215, 218, 321, 343
Rockway, M. R., 208, 221, 362 f., 382
Roderick, T. H., 128, 136
Rodgers, D. A., 116, 136
Rodnick, E. H., 520, 527
Roe, Anne, 428, 436, 443, 448, 458, 503, 527
Roelofs, C. O., 333, 343
Rogers, C. M., 240, 267
Rogers, C. R., 419 f., 422, 436, 439, 458, 467, 479 ff., 498
Rogers, M. S., 306
Rohles, F. H., Jr., 324, 326, 342, 415
Rohrer, J. H., 244, 269
Roman, J. A., 403, 417
Romanes, G. J., 140, 178
Roscoe, S. N., 350 ff., 354, 382 f.
Rose, J. E., 41, 47, 87
Rosen, B., 538, 571
Rosen, V. H., 431, 458
Rosenbaum, G., 398, 417
Rosenbaum, J. E., 542, 571
Rosenbaum, M., 285, 308
Rosenberg, M., 548, 571
Rosenzweig, M. R., 55, 58, 87, 135
Rossi, G. F., 376, 382
Rossman, J., 434, 436, 448, 453, 458
Rosvold, H. E., 41, 46, 49 ff., 57, 61, 82 f., 86 ff.
Rothenbuhler, W. C., 116, 119, 136
Rothkopf, E. Z., 187, 221
Rotter, Clara B., 478 f., 498
Rotter, J. B., 478, 489 ff., 498, 501, 503, 513, 517 f., 522 ff., 527
Rouse, R. O., 232, 266
Roussel, J., 244 f., 263
Roy, R. H., 345, 380
Rubin, E., 315, 330, 334, 343
Rubin, L. S., 511, 527
Ruesch, J., 499, 527
Ruff, G. E., 398, 401, 417
Runquist, W. N., 171, 177, 192, 221
Rupert, A., 52, 86
Rusinov, V. S., 58, 88
Rutherford, E., 542, 571
Rutledge, L. T., 44, 83
Saffir, M. A., 20, 32
Saldanha, E. L., 157, 178
Saltz, E., 192 f., 198, 220 f.
Saltzman, D., 185, 214, 220
Saltzman, I. J., 253, 269, 539, 571
Samuels, Ina, 376, 382
Sanford, R. N., 529, 567
Sansom, W., 400, 418
Santibañez, G., 49, 88
Sargent, S. S., 246, 269
Sassenrath, J., 213, 221
Sawin, P. B., 116, 136
Schachter, S., 542, 551, 571
Schafer, T. H., 407, 417
Scherrer, H., 52, 84
Schlosberg, H., 163, 178, 209, 217, 316, 343, 410, 418, 539, 572
Schludermann, E., 418
Schneider, R., 364, 379
Schneirla, T. C., 174, 177
Schock, G. J. D., 403, 417
Schoenfeld, W. N., 239, 269
Schrier, A. M., 255 f., 269
Schroder, H. M., 493, 498, 520, 526, 552, 569
Schulman, J. L., 226, 269
Schultheiss, P. M., 368, 379
Schulz, R. W., 190, 192, 204, 213, 221
Schultz, W. C., 565, 571
Schwartzbaum, J. S., 52, 88
Scott, J. P., 109, 123, 136
Scott, T. H., 241, 263, 299, 307, 376, 382, 397 ff., 409, 412 f., 415, 417, 533, 570
Scott, W. A., 500, 502, 527, 529, 532 f., 571
Seaman, G., 394, 415
Searle, L. V., 369, 382
Sears, R. R., 244, 265, 473, 498, 539, 571
Sedgwick, C. H. W., 455
Seeman, M., 513, 527
Segall, M., 231, 268
Selfridge, J. A., 187, 220, 298, 307
Sells, S. B., 386, 417
Semmes, J., 43, 60, 85, 88
Senden, M., 64, 88
Senders, J. W., 358, 382
Senders, Virginia L., 298, 308, 365, 383
Serson, E. A., 29, 33
Settlage, P. H., 43, 88
Seward, J. P., 54, 88, 161, 178
Sgonina, K., 119
Shands, H. C., 299, 308
Shannon, C. E., 187, 221, 297, 308, 322, 343
Shannon, T. E., 453, 458
Shapiro, K., 455
Sharpless, S., 376, 383
Sharpless, S. K., 53, 59, 87 f.
Shaw, M. E., 299, 308
Shaw, M. W., 128, 136

Sheffield, F. D., 173 f., 178, 231 f., 250, 269
Sheffield, V. F., 244, 269
Sheldon, R. W., 406, 416
Sheldon, W. H., 465 f., 498, 508 f., 527
Shepard, R. N., 186, 189, 221
Shepherd, W. T., 144, 146, 178
Sherif, M., 548, 558, 569, 571
Shields, J., 125, 136, 508, 527
Shirer, W. L., 571
Shirley, M., 243, 269
Shuford, F. H., 260 f., 270
Sidorsky, R. C., 298, 306, 353 f., 362, 383
Sidowski, J., 239, 270
Siegel, A. E., 542, 571
Siegel, A. I., 367, 372, 383
Siegel, P. S., 228, 243, 248 f., 269
Silver, C. A., 163, 178
Simberg, A. L., 453, 458
Simmel, M., 544, 569
Simms, E. S., 135
Simon, C. W., 238, 269, 365, 380
Simon, H. A., 189, 205, 219, 390, 417, 531, 570
Simon, S., 457
Simons, D. G., 415
Simons, J. C., 403, 417
Simpson, G. G., 92, 136
Sinnott, E. W., 93, 136
Sipowicz, R. R., 375, 379, 383, 405 ff., 412
Siskel, M., Jr., 291, 306
Skeels, H. M., 124, 136
Skinner, B. F., 36, 88, 163, 174 f., 178, 291, 490, 498, 540, 571
Skodak, M., 124, 136
Slamecka, N. J., 209, 221
Slater, E., 508, 527
Sleight, R. B., 349, 383
Smith, A., 127, 136
Smith, E. L., 101, 136
Smith, J. F., 382
Smith, K. U., 360 f., 381
Smith, M. F., 225, 270, 291, 308, 539, 571
Smith, M. P., 234, 270
Smith, O. A., Jr., 55, 88, 230, 270
Smith, Patricia C., 315, 341
Smith, S., 148, 178
Smith, W. R., 458
Snapper, A. G., 440, 458
Snyder, H. L., 232, 270
Snygg, D., 316, 343
Solomon, H. C., 61, 84
Solomon, L. N., 163, 178
Solomon, P., 242, 270, 397, 399 f., 416 f.
Sonoda, B., 537, 571
Spaet, T., 49, 88
Spalding, D., 108, 136
Spassky, B., 113 f., 134
Spearman, C., 433, 458
Spence, D. P., 552, 569
Spence, K. W., 155 ff., 166 ff., 174, 178, 257 ff., 270, 275, 308
Sperling, D. S., 228, 264
Sperling, G., 183, 221
Sperry, R. W., 43, 88
Speyer, J. F., 101, 136
Sporn, J., 226, 270
Spragg, S. D. S., 161, 179
Springer, K. J., 449, 459
Spuhler, J. N., 111, 136
Squires, P. S., 322, 343
Srb, A. M., 130, 136
Stabler, J. R., 256 f., 270
Stahl, F. W., 100, 136
Stallings, H. D., 403 f., 414
Stamm, J. S., 45, 47, 50 f., 88 f.
Star, S. A., 12, 32
Stauffer, D. A., 455
Stebbins, G. L., Jr., 107, 136
Stebbins, W. C., 235, 270
Steger, J. A., 326, 335, 343
Steggerda, F. R., 226, 270
Stein, L., 54, 88, 167, 178
Steinberg, A. G., 116, 136
Steinberg, M., 248, 269
Steiner, G. A., 536, 568
Steiner, I. D., 561, 571
Steinkamp, G. R., 397, 399 f., 413
Stellar, E., 55, 80, 84, 88, 226, 249, 270, 539, 571
Stenson, H. H., 373 ff., 377 f., 406, 412
Stern, C., 115 ff., 129, 136
Stern, W., 388, 417
Sternberg, S., 187, 220
Stevens, S. S., 23, 25, 28, 32, 278, 281, 283, 286, 308
Stevenson, J. A. F., 248, 270
Stockner, Carol, 507, 520, 526
Stotland, E., 542, 571
Stouffer, S. A., 12, 26, 32
Strange, J. R., 160, 178
Strassburger, R. C., 254, 270
Strong, P. N., Jr., 248, 270
Strughold, H., 404, 414
Stunkard, A. J., 225 f., 270
Suchman, E. A., 12, 32
Suchman, J. R., 436, 458
Suci, G. J., 546 f., 553, 571
Suedfeld, P., 314, 343
Sullivan, H. S., 469, 498, 514, 527
Sullivan, J. J., 163
Sumby, W. H., 298, 308
Summers, S. S., 457
Suppes, P., 199, 221
Survis, J., 45, 83
Sutherland, N. S., 186, 218
Sviminoff, C., 349, 383
Swanson, C. P., 103, 136
Swartz, W. F., 358, 360, 382
Sweeney, J. S., 362, 366, 383
Swets, J. A., 186, 221, 303 f., 308
Sylvester, J. D., 331, 340
Szasz, T. S., 501 f., 527
Szwarcbart, M. K., 46, 57, 88

Taft, R., 562, 572
Taguiri, R., 546, 562 f., 572
Talbot, S. A., 43, 88
Tannenbaum, P. H., 546 ff., 553 f., 571 f.
Tanner, W. P., Jr., 186, 221, 301, 303 f., 308
Taylor, C. W., 423, 430, 448, 455, 458
Taylor, D. W., 432, 443 ff., 458
Taylor, E., 230, 270
Taylor, F. V., 358, 360 ff., 369, 379, 382
Teas, D. C., 158, 178
Teel, K. S., 254, 270
Teghtsoonian, M., 186, 221
Teichner, W. H., 320, 341
Teitelbaum, P., 54 f., 84, 88, 226, 270
Ten Eyck, P. H., 287, 308 f.
Tepperman, J., 55, 82, 226, 263
Terry, H. I., 550, 568
Terzuolo, C. A., 46, 56, 88
Teuber, H. L., 42, 48, 60 f., 80, 88 f., 401, 417
Thaler, V. H., 398, 401, 417
Thetford, W. N., 60, 89
Thibaut, J. W., 556 f., 560, 570, 572
Thistlethwaite, D., 160, 179
Thomas, D. R., 235, 264
Thompson, G. H., 274
Thompson, W. R., 93, 107 ff., 113 f., 116, 119 f., 123, 126, 134, 136
Thorndike, E. L., 142 ff., 152 ff., 160, 163 f., 168, 174 f., 179, 213, 215, 221, 329 f., 343, 387, 436 f., 458, 490, 498, 540, 572
Thorpe, W. H., 542, 572
Thune, L. E., 208, 221
Thurstone, L. L., 17, 20, 32, 194, 221, 274, 278, 280, 308, 422 f., 433, 441, 458
Tighe, T., 320, 343
Tinbergen, N., 130, 136, 318, 343
Tinker, M. A., 339, 342
Titchener, E. B., 274, 312, 326, 330, 343, 434
Tjio, J. H., 126, 136
Tolman, E. C., 150 ff., 157 ff., 165 f., 177, 179, 388, 417, 434, 490, 498
Torgerson, W. S., 15 f., 23, 26, 28, 32 f., 278, 281, 308
Torrance, E. P., 425, 429, 432, 444 ff., 452, 454, 458 f.
Towbin, E. J., 227, 270
Trabasso, T., 199, 218
Trapold, M. A., 257, 265
Treichler, F. R., 248, 270

Triandis, L., 536, 572
Triebel, W., 394, 415
Trowbridge, M. H., 215
Trumbo, D. A., 407, 414
Trumbull, R., 270, 417
Tryon, R. C., 112, 136
Tsang, Y. C., 225, 270
Turbeville, J. R., 158, 179
Turchioe, R. M., 285, 308
Turkewitz, M., 214, 220
Turner, E. D., 338, 340, 343, 393, 407, 412
Turpin, R., 135

Uhr, L., 398 f., 416, 527
Ulrich, J. L., 145, 179
Underwood, B. J., 189 f., 198, 209, 211, 213, 217, 221
Urban, F. M., 274
Uyeda, A., 54, 88

Van Itallie, T. B., 226, 268, 270
Vernon, J. A., 314, 343, 397 ff., 417
Vinacke, W. E., 223, 270, 434, 438, 456, 459
Voas, R. B., 415
Voeks, V. W., 148, 169, 179
Voiers, W. D., 565, 572
Volk, B. W., 116, 134
Volkmann, J., 281, 283, 308
von Beckh, H. J., 402 f., 417
von Frisch, K., 116, 136
von Holst, E., 401

Waddell, D., 394, 414
Waddington, C. H., 123, 136
Wade, J. E., 403 f., 415, 417
Waelsch, H., 134
Wald, G., 248, 270
Walk, R., 320, 343
Walker, E. L., 250, 267
Wallace, B., 130, 136
Wallach, H., 327 f., 342
Wallas, G., 434, 444, 459
Wallis, R. A., 373, 381, 404 ff., 415
Walls, G. L., 324, 343
Walraven, P. L., 289, 306
Walter, D. O., 53, 82
Walters, P. A., 230, 264
Walters, R. H., 534 f., 537 f., 542, 567 f., 572
Wang, G. H., 225, 270
Wangenstein, O. H., 225, 270
Ware, J. R., 373, 375, 379, 383, 405 ff., 412, 417
Warren, B. H., 403, 417
Warren, J. M., 43, 89, 252, 270
Warren, R. M., 29, 33
Warren, R. P., 231, 270
Warrick, M. J., 361, 383
Washburn, A. L., 225, 264
Washburn, M. F., 144, 179
Washburn, S. L., 131, 137
Washburne, N., 527
Waters, R. H., 198, 222
Watson, J. B., 145 ff., 153, 168, 172, 179, 386, 391, 417
Watson, J. D., 100, 137
Waugh, N. C., 188, 221
Weasner, M. H., 250, 265
Webb, W. B., 239, 270, 374, 383
Webber, C. E., 358 f., 369 ff., 379, 383
Weber, E. H., 274, 290, 312
Wegener, J., 51, 83
Weidenfeller, E. W., 373, 383, 407, 417
Weigert, Edith V., 515, 525
Weinstein, S., 60, 88
Weinstock, S., 170, 179
Weisberger, P. S., 459
Weiskrantz, L., 43, 48, 50 f., 84, 87, 89
Weiss, J. M., 114, 134
Weiss, R. F., 256 f., 270
Weiss, W., 287, 308 f., 503, 527
Weisskopf-Joelson, E., 445, 459
Weisz, A. Z., 333, 341
Welch, L., 428, 430, 432, 459
Wells, F. L., 499, 527
Wertheimer, M., 315 f., 334, 343, 529, 532 f., 544, 571 f.
Westcott, M. R., 440, 459
Westheimer, G., 394 f., 417
Wever, E. G., 273
Wexler, D., 270, 399, 417
Wham, D. S., 208, 219
Wherry, R. J., Jr., 374, 383
White, B., 398 f., 415
White, R. W., 436, 459, 490, 498, 516, 518, 527, 566, 570
White, S. C., 415
Whitehorn, J. C., 226, 269
Whiteside, T. C. D., 395, 418
Whiting, B., 536, 572
Whitney, G., 226, 269
Wickens, D. D., 238, 268 f.
Wiesel, T. N., 44, 84
Wilgosh, L., 418
Wilkerson, L. E., 349, 383
Wilkinson, H. J., 242 f., 267
Wilkinson, R. T., 406, 418
Williams, A. C., Jr., 187, 217, 350, 382 f.
Williams, J. P., 195, 198, 221
Williams, R. A., 249, 270
Williams, R. J., 107, 137
Wilson, Martha, 44, 89
Wilson, R. C., 423 f., 433, 456, 459
Wilson, W. A., 47, 49 f., 52, 87, 89, 171, 179
Winch, R. F., 565, 572
Wing, J. F., 407, 415
Wing, S., 373, 381
Winocur, G., 418
Winston, H. D., 135
Wittels, F., 469, 498
Wittenborn, J., 503, 527
Wittrig, J. J., 287, 307
Witty, P. A., 449, 457
Wodinsky, J., 171, 176
Wogan, M., 198, 222
Wolf, A. V., 225, 270
Wolf, J. J., 367, 372, 383
Wolf, R., 554, 572
Wolfe, J. B., 161, 179
Wolff, H. G., 60, 89, 225, 270
Wollen, K., 195, 198, 222
Wolpe, J., 517, 527
Woodworth, R. S., 316, 324, 342, 388, 410, 418, 439, 459, 539, 572
Woolf, B., 136
Woolsey, C. N., 41, 46, 48, 87, 89
Work, M. S., 250, 263
Wright, H. F., 536, 568
Wright, J. H., 249, 265
Wright, S., 93
Wulff, J. J., 231 f., 269
Wulff, V. J., 298, 308
Wundt, W., 273, 312, 315, 324, 330, 434
Wyckoff, L. B., 239, 270, 365, 383
Wyers, E. J., 57, 82

Xhignesse, L. V., 364, 366, 379

Yadovsky, V. I., 402, 414
Yamaguchi, H. G., 253 ff., 265, 271
Yazdovskii, V. I., 403, 418
Yerkes, R. M., 147, 179, 253, 271
Yoder, R. M., 234, 269
Young, F. A., 229, 266
Young, P. T., 246, 260 f., 271, 539, 572
Young, R. K., 203, 222
Young, T., 289
Yuganov, E. M., 403, 418

Zaccaria, M. A., 430, 459
Záharóva, A., 58, 82
Zanchetti, F., 376, 382
Zander, A., 557, 559, 568
Zeaman, D., 256, 271
Zelditch, M., 530, 572
Zeliony, G., 164, 178
Zener, K., 287, 309
Zigler, E., 503 f., 519 f., 527
Zimbardo, P. G., 240, 271
Zimmer, H., 397, 413
Zimmerman, D. W., 238, 271
Zimmerman, R. R., 247, 266
Zimmerman, S. B., 135
Ziskind, E., 418
Zubek, J. P., 325, 340, 397 f., 400, 418
Zusne, L., 322 f., 342
Zwicky, F., 453, 459

Index of Subjects

Ability, 276; convergent production, 428
Ablation, 43
Abnormal, EEG, 45
Abnormalities, 115, 128 f. (*See also* Down's syndrome, Galactosemia, Klinefelter's syndrome, Manic depressive, Microcephaly, Neurological disorders, Normal, PTC taste blindness, Phenylketonuria, Schizophrenia, Tay-Sachs disease, Turner's syndrome)
Absolute scaling, 20
Abstraction, 60 f., 311; and flexibility, 442
Accentuation, 316 f.
Acceptance, 315
Accommodation, 332, 396
Acetylcholine-cholinesterase system, 38, 58 f.
Achievement, academic, creativity and, 449
Acquired drive, 162
Acquired needs, 227 ff.; appetitional states and, 228 f.; role of fear in, 229; Hullian position on, 228; implications of, 228; learning and, 228; noxious stimuli and, 229
Action circuit, 73 f., 76 ff.
Activating system, 53, 65, 68, 78
Activation principle, Morgan's, 141 f.; Hull's and Tolman's compared, 154
Activationist hypothesis, 376
Activity, 325; body temperature and, 248 f.; deprivation states and, 248; interpolated, 185; learning and, 249 f.; reinforcement and, 231
Acuity, depth, 320; tactual, 325; visual, 323
Adaptation, central, 324 f.; desensitizing effects of, 325; durative, 314, 323 ff.; peripheral, 325; sensitizing effects of, 324 f.; steady-state, 324; transient, 324
Adaptation level, 71, 216, 284 ff., 324, 329, 331, 337 f.; motivation and, 256; theory, 29 f., 284 ff., 409 ff.; vigilance and, 409 ff.
Adaptive flexibility, as factor DFT, 427
Additivity, 111
Advertising, creative potential and, 430
Aesthetics, 315
Affect, 61 f., 314 f.
Affective states, arousal and, 243; motivation and, 246 f.
Aftereffect, figural, 327, 340; negative, 326, 328
Afterimage, 330, 332
Age, creative potential and, 449; maximum abilities and, 451; quality of creative productions and, 451; quantity of creative production and, 451
Aggression, 470, 473; counter, in creative children, 447
Aiding, rate, 360 f.; time constant, 361; in tracking, 360 ff.
Air, drinking, 231
Alleles, 94
All-or-none principle, 202, 205, 281, 284; models of, 207
Alternatives, number of, 196
Altruism, as motive, 436
Alumina cream, 43, 45, 50
Amygdala, 40, 51 f., 71, 73 f.
Analyser, 62
Analysis, dimensional, 315
Anchor, 204 f., 332, 336 ff.; heteromodal, 333; intramodal, 334 f.; lighter, paradox of, 339; subliminal, 336, 339
Androgen, 54
Anecdotalism, 140
Anesthesia, 314
Anterior insula, 74
Anterior thalamus, 51
Anticipation, 364, 366 f.
Anticipatory goal response, 153, 160, 166
Anxiety, 512, 514
Aphagia, 226
Appetitional states, acquired needs and, 228
Appraisal, 72 ff., 77 f.
Approach, subject-centered, 278
Aptitude, factors of, 423
Area, estimates of, 339
Army-Navy instrument program, 352, 354
Arousal, Berlyne's theory of, 242 f.; determinants of, 243; energizing function of, 225; hypothesis, 376 f.; postulates of, 243; system, 40, 52, 65, 67; vigilance and, 410
Array, optical, 320
Art courses, creativity and, 452
Assimilation, 323, 325 ff., 340
Association, 62, 65 f., 73; areas, 41 f., 46, 60, 62 f., 65, 70, 74, 78; backward, 192; formation, 189, 192; time course of, 193; implications and, 434; interletter, 191; preestablished, 184, 187, 203, 212; principle, weakness of, 434; remote, 203 f., 424; stimulus response, 182, 189; word, 454; working time, 441

Associational fluency, as factor DMR, 427
Associationists, British, 312
Assortment, independent, 95, 104, 108
Assumptive world, 389, 396
Athetosis, 57
Attensity, 330
Attention, 52, 63, 65, 313 f., 329 f., 372, 377, 405; vigilance and, 408
Attitudes, 546 ff., 551, 553; evaluative, 444 f.; formation, 284
Attribute, 2, 275, 277, 286, 312, 315 f., 330; relation to response, 26 ff.
Audiogenic seizures, 113, 394
Audition, 289
Auditory discrimination, 60; system, 48, 50
Authors, most creative years of, 451
Autonomic function, 40, 53, 77
Availability, 192, 209
Avalanche conduction, 77
Aversive stimuli, acquired needs and, 228
Avoidance behavior, 524; conditioning, 44, 51, 139; explanation, 162; learning with brain stimulation, 234
Axon, 37 f.

Background, 209, 325
Balanced units, theory of, 544 ff.
Basal ganglia, 40, 57
Behavior, 91 f., 102, 107, 130; affective, 287; episodes, 388; genetics, 91, 111, 128, 133; motivational, 287; potential, 490 f.; social, 287; verbal, 286
Behavioral setting, 388
Behavioral stereotype, index of, 294, 297
Behaviorism, 56, 387
Belief dilemma, 551 f.; modes of resolution of, 551
Binary digit, 293
Biochemical pathways, 128 f.
Biochemistry, characteristics of research in, 509; correlated constructs and, 510; reductionism and, 510; schizophrenia and, 511
Biographical information, creativity and, 430
Biological requirements, as primary source of motivation, 224
Birth order, creativity and, 448
Bisection, method of, 14
Bit, 293, 322
Bitter, 315
Black box, 292
Blend, sensory, 324
Blindness, 80; congenital, 64
Blocking, selective, 313; sensory, 314
Blood sugar level, hunger and, 225
Body tonus, 57
Body types, 508 f.
Brain, behavior correlations, 80; human function, 59 f.; injury, role in producing aphagia and hyperphagia, 226
Brain stimulation, escape and avoidance behavior and, 234; hunger and thirst and, 227; reinforcement and, 233; rewarding and punishing effects and, 234
Brainwashing, 241
Brief Psychiatric Rating Scale, 504
Brightness, 297; discrimination, 44; series, 316
Bulb, olfactory, 315

Caloric intake, as a factor in hunger, 225
Cardinal trait, 486
Card-sorting task, 61
Caste systems, 110
Castration, 54
Category ranks, method of, 11; normalized, 20 f.
Caudate loop, 57
Caudate nucleus, 40, 50, 54
Cell assemblies, 63, 71, 78
Cell body, 37
Cerebellum, 39, 46, 57, 73, 76 f.
Cerebral hemispheres, 40
Channel capacity, 293, 297, 299
Characters, qualitative and quantitative, 125
Chemical stimulation, 43
Chemists, most creative years of, 451
Chi square, 295
Children, divergent production in, 430
Cholinesterase, 59
Chorea, 57
Chromatic adaptation, 394
Chromosome, 94, 126; aberrations, 105, 127 f.; analysis, 120; aneuploidy, 105; autosomes, 96; complement, 94; deficiency, 105; diploid, 94, 104; duplication, 105; genome, 103; heterogametic sex, 96; homogametic sex, 96; homologous, 94; human complement, 126; idiogram, 126; inversion, 105, 121; karyotype, 94, 126; linkage, 96; nondisjunction, 127; polyploids, 105; sex, 96; translocation, 105; trisomy, 127; X, 122
Cingulate cortex, 40, 51, 58, 74 f., 77
Class concept, unconscious, 440
Classes, hardening of, 443; reinforcement and, 454
Classical conditioning, Hullian's explanation, 152, 162
Clearness, 317, 330
Cleverness, originality and, 424 f.; working time and, 441
Closed-loop task, 356
Cochlear nucleus, 52
Codominance, 102
Cognition, 61
Cognitive consistency, 543 ff.; balance theory, 544 f.; cognitive and affective congruence, 548; principle of congruity, 546 f.; strain toward symmetry, 545 f.
Cognitive dissonance, 548 ff.; individual differences in, 552 ff.
Cognitive reinforcement, 65, 72; style, 554 f.
Cohesiveness, 559
Collative variables, as a determinant of arousal, 243
Color, achromatic, 316; change, 314; chromatic, 316; constancy, 396; conversion, 285; discrimination, 46
Color blindness, 323
Colorimetry, 320
Combination, 311
Commissure, 46
Common trait, 486
Communication channel, 292
Community, and creativity, 448 f.
Comparison, 311
Competing responses, role in differential responding, 252
Complexity, 323; preference for and age, 436
Complication, 318, 324
Compound response, 201

Compulsions, 514, 516
Concentric brain, 67
Concept identification, 199
Concordance, 125
Conditional reaction procedure, 44
Conditioned response, 53, 62
Conditioning, 181; background, 184, 188, 190
Conditions of worth, 481 f.
Conduction, nervous, 38; one-way, 315
Cone, 312
Conflict, 65
Confluxion, 324
Conformity, 557 ff.; originality and, 437; pressures for, 450
Confusions, 185
Congruency, principles of, 438
Connectionism, 387
Consanguinity, 111 (*See also* Inbreeding)
Consciousness, 273, 277
Constancy, 320, 323, 325 ff., 332, 340, 394 ff.; lightness, 326; phenomenal, 321, 324; shape, 332
Constant sum method, 23 ff.
Construct, 312, 461 ff.; hypothetical, 275; operationality of, 464 f.
Constructive will, 480
Consummatory response, 53; as related to K, 257 f.; learning and, 249 f.; motivation and, 248 f.; primary needs and, 249; reinforcement and, 232
Contact comfort, 247
Content, auditory figural, 427; categories in personality theories, 465; factors and, 427 f.; kinds of, 426; language as, 428
Contiguity, 213, 437
Continuation, good, 316
Continuity-noncontinuity, 157 f.
Continuum, 275; physical, 276, 279; physiological, 276, 280; sensory, 276, 281; stimulus, 279, 281, 284
Contour, 320; isohedonic, 315
Contrast, 165, 323 ff., 340; in learning, 256; reversed, 329; simultaneous, 324
Control, acceleration, 360; order, 359; position, 359; scale factors, 362 f.; sensory, 315; system dynamics, 359 ff.; velocity, 360
Control-display, ratio, 362; relationships, 348
Convergence, 332 f., 396
Converging operations, 8
Conversion, color, 326
Coordination, 57
Coping, 518, 520
Corpus callosum, 40
Correlation, 292 f.
Cortex, 41, 43
Corti, organ of, 312
Cortical reflex, 81
Cortico-cortical connections, 45
Counter, 347; conditioning, 209, 211, 517; will, 480
Creative potential, 420; as composite, 422; distribution of, 421; IQ and, 432
Creative production, 420
Creativity, definitions of, 419; development of, 448 ff.; group, 445; in music, 427; in painting, 428; social conditions in, 445 ff.; structure of intellect and, 425 ff.; tolerance of, 448; training for, 451 ff.; training methods, 453
Criterion, 186 f.; cut-off, 302 f.; quantum, 281 ff.
Critical flicker frequency, 60, 80
Criticism, effects of, 446
Crossing over, 95, 119
Cue, contextual, 183 f.; learning, 183 f., 187, 191
Curiosity, motivation and, 240 (*See also* Exploration)
Curve, backward-learning, 199, 206; learning, 188; production, 441 f.; ROC, 186; serial position, 203; spatial position, 204

Daphnia, 319
Dark adaptation, 60
Darwin, interest in animal behavior, 140
Data, color-naming, 289
Death instinct, 516
Decay, 184
Decision axis, 302
Decision process, 299
Decrement, cognitive, 314
Defecation, 314
Defense mechanisms, 513 f.
Definitions, ideal, 494; operational, 465, 494; systematic, 494
Deiters' nerve cells, 131
Delayed alternation, 49, 57
Delayed reaction, 145
Delayed response, 44, 49 f., 57
Dendrites, 37
Dendritic zone, 37
Denial, 514
Deorsumversion, 333
Dependencies, intersymbol, 296; response, 296; sequential, 187 f.
Depolarization, 38
Deprivation, activity and food, 248; activity relationship to, 225; consummatory activity and, 249; in drive stimulus theory, 259; of information, 242; parental, 448 f.; sensory, 313 ff.
Depth, 312, 322
Design, representative, 385
Desynchronization, 52
Detectability, ideal, 303; index of, 294, 303
Detection, signal, 186
Deutlichkeit, 330
Device, mnemonic, 192
Dexterity, manual, 47
Diallel crosses, 120
Diencephalon, 39 f.
Difference, noticeable, 281, 286; temporal, 333
Differentiation, 189
Differentiation procedure, 48
Dimension, 275 f., 312, 315; personality theories, 463 ff.; turbid-clear, 317
Direct current, 55, 81
Direction, 318 f.; spatial, 320
Discordance, 125, 127
Discriminal process, 280
Discrimination, 189; auditory intensity, 290; frequency, 283; loudness, 281, 283; perceptual, 277, 313; pitch, 280; sensory, 277, 279 f., 313; training, role in establishing secondary reinforcing stimuli, 239
Disease entity approach, 502 ff., 521 f.
Displacement, 513 f.

Display, circular scale, 348; contact analog, 350, 352 ff.; fixed-scale moving-pointer, 348; horizontal, 350 ff.; inside-out, 351; linear scale, 348; moving-scale fixed-pointer, 348; outside-in, 351; pictorial, 347, 350 ff.; practice effects for, 355 f.; situational, 350 ff.; symbolic, 347; vertical, 350, 352 ff.
Display integration, 366
Dispositions, 69, 71
Distance, 312; Adler's concept of, 476
Distinction, 311, 329 f.
Divergence, 333
DNA, 100, 131
Dominance, 95, 99, 115, 121
Dominant focus, 58
Double dissociation, 42, 48
Down's syndrome, 126 f.
Drawing, criterion for, 430
Drift, genetic, 98
Drinking, 54
Drive, externalization of, 228; motivational concept, 223; problems of, 244; reduction theory, 55; state, 65; stimulus theory, 259
Drives, 285
D-scale, 330
Dualism, 470, 472, 489
Duration, 334; discrimination of auditory, 48
Dynamic systems, 389

Eating, 54 f.
Ecological validity, 392
Education, correlates of, 433; creativity training and, 454
Effect, anchor, 336; empirical law of, 468, 490, 494; kappa, 331; paradoxical anchor, 338; tau, 331; transfer, 453 f.; von Restorff, 192 f., 205
Efferent control, of sensory information, 69, 71, 78
Efficiency, 303
Ego, 312, 471 f., 512 f., 516; impairment, 401
Elaboration, factor of, 425
Electrical stimulation, 43 ff., 47, 50 f., 54, 56, 58, 60
Electroconvulsive shock, 59
Electroencephalograph, 43, 45, 52, 376
Emotion, 51, 65, 72, 79, 400 f.; intuition and, 439
Empiricism, 311; British, 273, 312
Empty-field myopia, 395
Encoding, 183, 189, 192
Engineering psychology, 345 ff.
Entropy, 293
Environment, 124; behavioral, 387 f., causal texture of, 388, 391; common sense, 389 f.; complexity of, 58; enrichment of, 240; formal properties of, 390; functional definition, 388 ff.; heredity and, 386; impoverishment of, 241 f.; methodological issues, 391 f.; physical, 386 f.; preconceptions of, 386; unusual, 385, 392 ff.
Environmentalism, Watsonian, 387
Enzyme, 102, 129
EPAM model, 189
Epistasis, 102, 105
Equality, 278
Equisection, method of, 14 f.
Equivalence, sensory, 274
Equivalent configurations, 391
Equivocation, response, 294; stimulus, 294
Error peaks, 369
Escape learning, 234
Estrous cycle, motivation, and, 225
Eureka feeling, 439
Evaluation, 78, 429; age and, 445; effects of, 446; factor of, 429; role of, 444 f.; set in reading, 444 f.
Evolution, 66, 98, 100, 130 f.; of intelligence, 140 f., 175 f.
Excitation, 52, 62 f.
Expectancy, 65, 73 f., 78, 319, 408 f., 490 ff., 523 f.
Expected value, 301
Experience, early, 385, 389; past, 334
Experiment, psychophysical, 278, 280 f., 288 f.
Experimental approach, 529 ff.; comparison with other approaches, 532; definition of, 529 f.; types of, 530 f.
Exploration, activity deprivation and, 240; motivation and, 240 (*See also* Curiosity)
Exploratory behavior, varying categories of, 243
Expressional fluency, 423; as factor DMS, 429
Extensity, 330
External world, 311; control, 493
Externalization, drive, 228
Extinction, 53 f.; to differentiate learning from performance, 253 f.; persistence and, 224; secondary reinforcement and, 239; vigilance and, 408
Extrapyramidal system, 56, 77
Eye-hand coordination, 399
Eye movements, 349, 365

F_1, F_2, 118 ff.
Facilitation, 314, 325
Factor analysis, applications of, 421; experimental controls in, 422; treatment of data, 422
Factors, aptitude, 423; flexibility, 423 ff.; as functions, 422; redefinition of, 425
FAE, 328 f.; kinesthetic, 329
Familiarity, 186 f., 190; flexibility and, 443
Family correlations, 124
Fate, common, 316
Fatigue, incubation and, 439
Fear, difficulties, 230; factors involved in strength of, 230; acquired need and, 229; reduction, 229
Feedback, 313; afferent, 61
Feminine protest, 477
Fibers, motor, 273; sensory, 273
Figural aftereffects, 289
Figure, 317, 334; brain-babies, 330; detection, 395; inspection, 328; perception, 394; recognition, 395; von Bezold, 324
Figure-ground, 315, 329 f.
Filter theory of vigilance, 408
Finickiness, 55
Fixation process, 59
Fixed ratio schedule, secondary reinforcer, and, 237
Fixedness, functional, conditions of, 444; embeddedness and, 444; forgetting of, 444; overlearning and, 444; redefinition of, 443 f.
Flaccid paralysis, 57
Flexibility, adaptive, 424; with classes, 442; factors of, 423 ff.; role of, 442 ff.; spontaneous, 424, 442
Fluctuation theory, 186
Fluency, associational, 423; curves of, 442; expressional, 423; factors of, 423; ideational, 423; pathol-

ogy and, 441; role of, 441 f.; training and, 452; vocabulary, 442; word, 423
Folie à deux, 507
Forced-choice method, 304
Forebrain, 39
Forgetting, 183, 185, 211; account of, 185; long-term, 207
Form, 320, 322; discrimination, 44, 46; nonsense, 323; perception, 299, 398; space, 316; time, 316; type, 318
Formation, reticular, 333
Fornix, 76
Frame of reference, 284 f.
Freedom of movement, 491 f., 523 f.
Frequency, primary reinforcement and, 254 f.; as principle of learning, 147 ff.; secondary reinforcing stimuli and, 235
Frequency discriminations, 48
Frontal cortex, 49 f., 52, 60 f., 67, 71, 73, 76 f.
Frontotemporal cortex, 40, 51
Frustration, associative factors in, 245; as motivational determinant, 244 f.; primary consequences of, 244; reduction in reward and, 245; role in acquired needs, 229 f.
Function, critical value, 289, 291; linear, 281 ff.; logistic, 280; perceptual, 289; psycho-physical, 288; rectangular probability, 282; step, 197
Functional autonomy, 228, 485 f.
Functionalism, 182
Fusion, 332; tactual, 325

Gain, signal, 362
Galactosemia, 115 f.
Gamete, 94
Ganzfeld, 394 ff.
Gating, 313 ff.
Gene, comparison, 122; pool, 98
Generality-specificity, 475, 477, 484, 491
Generalization, 44, 52, 193
Generalizers, and scanning, 443
Genetic variance, 108, 114, 120
Genetics, homosexuality and, 507 f.; human behavior and, 120, 126; intelligence and, 506; manic-depressive psychosis, 507; neurosis and, 508; psychosis and, 506 f.; schizophrenia and, 506 f.
Genotype, 93
Geotaxis, 113
Gestalt, 316, 389, 391, 394 f.
Glial cells, 37
Globus pallidus, 40, 77
Gloss, 320
Glucagen, as related to hunger, 226
Glucoreceptors, hunger and, 225 f.
Glucose, role in hunger, 225 f.
Gnostic responses, 62
Goals, 78
Graded dichotomies, method of, 20
Gradient, density, 320; texture, 320
Ground, 334
Group, composition, 563 ff.; definition of, 556; evidence for existence of, 557; homogeneity of values and, 565; mind, 556; need complementarity and, 566; norms, 555 ff.; performance, 401
Grouping, 316; phenomenal, 321; visual, 321
Growth curves, creative potential, 449 f.
Guidance, 144
Gustation, 291 f.
Gustatory cortex, 48

Habit, breaking and transformation of, 443; Hullian theory of, 258; letter-sequence, 190, 212; linguistic, 212; and need intensity, 254; strength, 152 ff., 253 f.
Habituation, 71, 376
Hallucinations, 398 f.
Hedonic value, 261
Heredity, 93, 95 f., 106, 119 f., 130, 133; creativity and, 448; environment and, 115, 386
Heritability, 107, 113, 124
Heterozygotes, 95
Hind brain, 39
Hippocampal gyrus, 74
Hippocampal memory circuit, 73 ff.
Hippocampus, 40, 51, 53, 58, 60, 75 ff.
Hits, 301 ff.
Home life, creativity and, 449
Homeostasis, 225, 324, 387 f.
Homozygotes, 95, 105, 114
Horizon, 320
Hue, 297
Humor, transformation and, 427
Hunger, blood-sugar level and, 225; gastric contractions in, 225; glucostatic factors in, 225; nervous-system factors in, 225; physiological basis of, 225 f.
Huntington's chorea, 506
Hybrid, 117
Hyperphagia, 226
Hypothalamic rage, 234
Hypothalamus, 40 f., 55, 67, 77; drinking response and, 229; hunger and thirst and, 227
Hypothesis, constancy, 330; position, 204; quantal, 283 f.; specificity, 203; spew, 190; testing, 389
Hypothetical construct, 8
Hysteria, 514, 516

Id, 471 f., 512 f., 516
Idea, 314; complex, 311; hitch-hiking, 445
Ideal observer, 300
Idealism, subjective, 311
Ideational fluency, 423; as factor DMU, 426
Illusion, 332
Image, 71, 78
Imagination, 73 f., 78
Imitation, 144
Imitative learning, 541 ff.; inhibitory and disinhibitory effects in, 542; modeling effect in, 542 f.
Immediate memory, 49
Implication, association and, 434
Imprinting, 231
Impulse, afferent, 313; tactile-kinesthetic, 333
Inbreeding, 98, 114 ff. (*See also* Consanguinity)
Incentive, as *K* in Hull-Spence motivation theory, 259; role in temporal delay, 257
Incongruence, 481 f.
Incubation, fatigue in, 439; hypotheses, 439; research on, 439; role of, 438 f.; spaced practice and, 439
Individual differences, 92, 105 f., 116, 122, 132
Individual psychology, 468, 475 ff.
Induction, color, 330

Infantile autism, 520
Inferior colliculi, 40
Inferotemporal cortex, 42, 44 ff., 70
Infinity, 311
Inflection point, 339
Information, 184, 186 ff., 212, 215 f., 293, 319; cumulation of, 451; definition of, 438; deprivation, 242; importance of, 437; input, 292; recall of, 438; theory, 322; transmission, 293, 299; transmitted, 295, 299; types, 321
Inhibition, 47, 50, 52, 55, 57, 62 f., 77, 314, 325 f., 333; proactive, 451; vigilance and, 408
Input, kinesthetic, 333
Insight, 62; conditions of, 440 f.; previous experiences and, 440; motivation and, 440; nature of, 439 f.; role of, 439 f.
Insistence, 316
Instincts, 108, 468, 495; psycho-analytic theory, 470 ff., 475
Instrumental behavior, motivation and, 250 ff.
Integration, motor responses, 57
Intellect, structure of, 438; transfer in, 438
Intellectual, drive, 436; functions, 67, 399 ff.
Intelligence, 79, 117, 124, 287, 506; evolution of, 175
Intensity, 330; auditory, 48
Intensive variables, as determinants of arousal, 243
Intention, 65, 71, 77 f.
Interaction, 329; cross-modal, 337; heteromodal, 329, 333
Interest, in thinking, 437
Interference, 49, 184, 190, 208; hypothesis, 439; intralist, 196; Lashley's theory of, 62 f.; proactive, 208; proactive accumulation, 211; retroactive, 208 f.; sequential, 212; sources of, 212
Intermediate-size problem, 156
Intermittent reinforcement (See partial reinforcement)
Internal consistency, category ranks, 20 f.; constant sum method, 25; equisection, 15; magnitude estimation, 23; pair comparisons, 20 f.; ratio production, 22; scales, general, 8 f.
Internal control, 493
Internal core, of thalamus, 41, 67 f.
International Congress of Psychology, 316
Intertrial interval, 43, 54
Intervals, equality of, 278; method of, 15
Intervening variable, 8, 53
Intrinsic nuclei, of thalamus, 41, 67, 70 f., 74
Introspection, 273, 277; analytical, 312
Intrusion, 209
Intuition, efficiency of, 440; role of, 439 f.
Inventors, family backgrounds of, 448; early inventions of, 451
Investigating response, 243
IQ, correlations with creative potential, 432 f.; of highly creative individuals, 432
Irrelevant incentives, 159 f.
Irritative lesions, 43, 45, 50
Isogenic, 121
Isolation, 513; tolerance, 400
Isomorphism, 324

Judgment, 334; suspended, in brainstorming and originality, 444 f.

K, in Hull-Spence system, 259
Keplerian trajectory, 402
Kinesthesis, 312; figural factors in, 427; impulses, 43, 46 f.
Klarheit, 330
Klinefelter's syndrome, 128
Knowledge, 311

Lactation in affectional responses, 247
Lag, 361
Landmark, 319
Language, construct, 275; data, 275
Latent structure analysis, 26
Lateral geniculate body, 45
Lateral hypothalamus, 54
Lateroversion, 333
Law, 133; configurational, 316; of comparative judgment, 17; of effect, 142 f., 154, 169 f., 230; Emmert's, 332; empirical, 275 f., 279; exercise, 148; Gestalt, 317; Hardy-Weinberg, 98; hypothetical, 276; independent assortment, 95, 101, 108; logarithmic psychophysical, 286; photographic reciprocity, 331; Prägnanz, 316; primary reinforcement, 231; psychophysical, 276; Ricco's, 331; of simplicity, 317
Learned drives (*See* Acquired needs)
Learning, 45, 48, 53, 62, 65, 70, 79, 287 f.; animal, 139 ff.; comparative study, 175 f.; concept, 189; definition, 139 f.; delay of reinforcement and, 257 f.; discriminative, 155 ff.; distinguished from performance, 253 f.; effect of drugs upon, 59; electrical events and, 53; experiments, classification of, 139; incidental, 213; information and, 438; latent, 152, 158 ff.; need intensity and, 251 ff.; one-trial, 145, 148 f.; operant, and originality, 454; paired-associate, 189; perceptual, 144 ff.; problems in, 517 f.; psychopathology, applied to, 516 ff.; reinforcement and, 254 ff.; relational, 155 ff.; response, 182, 190, 200, 202; serial, 190, 202 f.; set formation in, 44; structural basis of, 58 f., 62; theory, 181, 517; trial and error, 140 f.
Lethal gene, 121
Level, 314, 340; of arousal, 79; clear, 330; of organization, 92; unclear, 330
Leveling, 316 f.
Life-space, 390 f.
Likelihood ratio, 301
Likeness, phenomenal, 337
Limbic areas, 40, 50 ff., 67, 69
Limen, 279
Linkage, 119
List, identification, 210; length of, 188
Lobotomy, 61
Localization, 48, 59 ff., 80; absolute, 333; egocentral, 333; optic, 333
Locomotion, exploratory behavior and, 243
Love, as motivational determinant, 247

M I, M II, 56
Magnitude estimation, method of, 22 f.
Mammillary bodies, 76
Manic-depressive psychosis, 117, 507, 515
Manipulation drive, 240
Man-machine system, 345, 378

Markov chain, 197
Masculine protest, 476 f.
Masking, 333
Mass action, 80
Maternal behavior, 51
Mathematical biophysics, 80
Mathematicians, most creative years of, 451
Mating, assortative, 98, 110, 114; random, 98; systems of, 109
Maze, 61
Meaning, 64, 190, 192, 202, 312
Measure, criterion, 274
Measurement, 276 ff.; attitude, 274; psychophysical, 274; unit of, 275, 286
Medial forebrain bundle, 51, 54
Medial geniculate, 48
Mediating process, in scaling, 27 ff.
Mediator, 192
Medulla oblongata, 39, 56
Meiosis, 94, 104, 127
Memory, 45 f., 60, 62, 65, 72, 78 f., 132; circuit, 73; factors of, 429; immediate, 182, 187, 377; loss of, 69; measure, recognition, 186; short-term, 182; trace, 183
Mendelian genetics (*See* Transmission genetics)
Mendelian populations (*See* Population genetics)
Mental processes, higher, 47, 61
Mental retardation (*See* Abnormalities)
Mesencephalon, 39 f., 66, 76
Messenger RNA, 101, 131
Metabolism, inborn errors of, 128
Metencephalon, 39
Method, of absolute judgment, 292; attribute-listing, 453; of average error, 278; beating tone, 290; brainstorming, 445; check-list, 453; of constant stimuli, 288 f.; fractionation, 286; of minimal changes, 278 f.; of morphological-analysis, 453; multivariate, 421; paired comparisons, 278; psychological scaling, 278; psychophysical, 278 f.; synectic, 445
Microcephaly, 125
Microelectrodes, 44, 58
Microscopic anatomy, 41
Midbrain (*See* Mesencephalon)
Midbrain, nonspecific systems, 40 f., 51 ff., 57
Midline thalamic nuclei, 77
Mind, 273, 311; entities, 471
Minimal goals, Rotter's concept of, 491 f., 495
Misses, 301
Mitosis, 94, 104
Mnemonics, 193
Modality specificity, 41, 60, 68, 70, 78
Mode, color appearance, 315, 317
Model, all-or-none, 198; electronic, 329; information-processing, 189; linear, 196; multistage, 202; physiological, 326 f.; search, 438, 443
Molar properties, 315
Monitoring, 372, 398
Mongolism (*See* Down's syndrome)
Motivation, 43, 51, 53 ff., 65, 67, 72, 79 f., 288, 314 f., 377; affective state theories, 260 f.; Allport's theory, 485 f., 488 f.; awareness of, 448; determinants, influence on behavior, 247 ff.; difficulty of task and, 253; drive stimulus theory, 259 f.; Hull-Spence theory, 258 f.; insight and, 440; psychoanalytic theory, 470, 475; Rogers' theory, 480, 484; Rotter's social-learning theory, 490, 494; theoretical assumptions concerning, 467 f.; unconscious, 470
Motivator, 315
Motive, 285; activating function, 223; altruistic, 436; as distinguished from motivation, 262; cue function, 224; defense function, 224; definition, 223; directive function, 224; energizing function, 223, 225; functional characteristics, 223 f.; independence in thinking, 437; interest in thinking, 437; need to be different, 436; preference for complexity, 436; preference for novel responses, 436; selective function, 223 f.; sensitizing function, 224
Motor, activity, 63; areas, 41, 47, 56, 60, 62, 76; component, 64; equivalence, 66; function during weightlessness, 403; memory, 78; recall, 74; systems, 56
Movement, apparent, 321; illusory, 315; induced, 322; pattern, 56; perceived, 321; perception, 398; stroboscopic, 321
Müller-Lyer illusion, 324
Multiple prediction, 299
Muscle sense, 312
Muscles, middle ear, 52
Musician, auditory imagery in, 452; learning creative habits by, 452; most creative years of, 451
Mutation, 98, 122 f.
Myelencephalon, 39
Myelin sheath, 37

Natural selection, 108 f., 113
Nature-nurture issue, 107
Nearness, 316
Need, 285, 313; to be different, 436; for positive regard, 481, 484; potential, 491, 494 f., 523; value, 491, 523
Negative potential, 58
Neocortex, 40 f., 58, 67
Neo-Freudians, 514
Nerve impulse, 38
Nervous system, hierarchical nature of, 78
Neural representations, 71
Neurochemistry, 276
Neurological disorders, 116
Neuron, 37
Neurophysiology, 36, 275 f., 284
Neurosis versus psychosis, 521
Neurotic paradox, 516
Neutralization, 324
New Look, 333
Noise, 294, 299
Nonspecific systems, 53, 56 f.
Norm of reaction, 93, 106 f.
Normal, abnormality of the, 107
Normal distribution, 279, 301, 303; integral of, 279 ff., 283
Noxious stimuli, acquired needs and, 228
Nucleus, cochlear, 313
Numerical rating, 22

Object, perceptual, 312
Observing response, 365
Obsessions, 514
Occipital cortex, 42 ff., 56, 63
Oculogravic illusion, 403

Oedipus complex, 470, 472
Olfactory deficits, 48
Ontogeny, 108
Open-loop task, 356
Operations, kinds of, 426
Operationality, of Adlerian theory, 478; of Allport's theory, 487; of personality theories, 464 f.; of psychoanalysis, 473; of Rogers' theory, 483 f.; of Rotter's social learning theory, 494
Optic, array, 319; chiasm, 46; texture, 319
Optimal criterion, 301
Orbital cortex, 50, 61
Order, 312; of approximation, 296, 298
Ordering, 202; errors of, 203; sequential, 202
Organism, 311; environment and, 388 f.
Organization, 192 f., 316; perceptual, 192
Orientation, 318, 334; during weightlessness, 402 f.; in space, 50
Orienting response, 243
Originality, association theory of, 433; and conformity, 437; criteria of, 424 f., 441; as factor DMT, 424 f.; as flexibility, 427; judgment and, 444; training and, 452; training for, 453 f.
Overachieving, of creative children, 449
Overestimation, error of, 339
Overloading, 299
Overtraining, 45

Pain, 312, 333, 400; acquired needs and, 229; sensitivity, 398
Pair comparisons, method of, 10; normalized, 18 ff.
Pampering, 477 f.
Parietal cortex, 44, 46
Partial reinforcement, 56; reinforcement and, 236 f.
Partitioning of sets, 70 f.
Passive avoidance, 51
Pathology, creative production and, 431 f.
Pattern, 312, 316, 320, 334; structured, 322
Patterned-string problems, 44
Payoff, 187; matrix, 300
Pedigree, 123
Perception, 64, 70, 72, 75, 77, 79, 277 f., 288 f., 292, 311 ff., 313; depth, 320, 333; space, 319, 333; tactile-kinesthetic, 320; theory of, 330
Perceptual, constancy, 394 f.; learning, 64; orientation, 396
Percussion, critical frequency, 325
Performance, creative, group sanctions and, 447; creative, MMPI and, 447; creative, sex role and, 447; distinguished from learning, 253 f.; during space flight, 404
Perikaryon, 37
Permeability, 38
Perseveration, 58 f., 61; flexibility and, 442
Persistence, motivation and, 224
Personalistic theory, 468, 485 ff.
Personality, 287
Personnel subsystem, 345
Personology, 485 ff.
Perspective, 326, 332; normal, 332; reversible, 330
Phantasy, as measure of motivation, 262
Phase sequence, 63 f.
Phenomena, heteromodal, 328
Phenomenal report, 278 f.
Phenomenology, 313, 315 f.
Phenotype, 93
Phenotypic variance, 107 f., 120, 124
Phenylketonuria, 100, 115 f., 129
Phi phenomenon, 316
Phi-gamma function, 279 f., 283 f.
Philosopher, most creative years of, 451
Phobias, 514, 516
Photic driving, 44
Phototaxis, 113, 130
Phrenology, 80
Physicalism, in psychology, 387
Physiognomic characters, 315
Physiological factors, in hunger and thirst, 225 ff.; as basis for reinforcement, 232 ff.
Physiological genetics, 100
Physiological psychology, 35, 79
Placement experiments, 143 f., 160 ff., 167 ff., 173
Plans, 51, 53, 71, 78, 389
Plasticity, 62
Pleasantness, 74, 314 f.
Pleiotropic gene effects, 102, 119
Poet, most creative years of, 451
Polygenic inheritance, 96, 98, 119 f.
Pooling, 313, 329, 331, 333, 336 ff.; cross-modal, 332; model, 331, 337
Population, genetics, 97, 106, 108, 117; isolates, 110; stereotype, 355
Position, in family, 477 f.
Position sense, 47
Post-central cortex, 46 ff., 56, 60
Posterior association areas, 67
Posterior hypothalamus, 56
Posterior insula, 74
Posterior intrinsic system, 70
Posterior-parietal cortex, 47, 56
Postremity, 148 f.
Potential, auditory, 313
Practice, distribution of, 182; incubation and spaced, 439
Precentral areas, 60
Prefamiliarization, 192, 202
Preference method, 48
Prefrontal cortex, 73
Preoccipital cortex, 45
Preparation, for creative production, 437 f.
Prepyriform cortex, 49, 74
Presetting of responses, 61, 71
Pressure, deep, 312; light, 312
Primary needs, definition of, 224; as motivational antecedents, 224 f.; physiological factors in, 225 ff.; spontaneous and consummatory activity and, 248 f.
Primary projection areas, 41, 46, 60, 67, 72
Primary reinforcement, and ease of learning, 254 f.; and secondary reinforcement, 234 f.
Prior experience, 398
Prisoner of War, 558 f.
Probability, conditional, 292; density function, 301; learning, 49, 171 f.
Problem, human transportation, 453; Maier's string, 443; printing press, 442 f.; screwdriver, 453
Problem solving, creative production and, 435; factors and, 435

Process, dimension, 465; ergodic, 296; sensory, 273 ff., 287, 289, 291 f., 312 f.; stochastic, 292, 296
Process-reactive classification, 519 f.
Product, kinds of, 419, 426; psychological, 420
Production, convergent, defined, 426; divergent, defined, 426
Production, creative, association theory, 433; homogeneous groups and, 446; intuition in, 433; in inventors, 434; masculinity and, 447; motivation for, 435 ff.; nature of, 431 ff.; preconscious in, 431; preparation for, 437 f.; problem solving and, 435; quantity vs. quality, 441; systems in, 438; theories of, 431 f.; transfer theory of, 435, 437; unconscious in, 431, 433
Projection, 513; cortical, 273
Pronounceability, 191
Pronouncedness, 316
Property, object, 319; suprasummative, 316
Proprioceptive feedback, 363 ff., 377
Proprioceptive stimulation, anticipatory, 364; regulatory, 364
Proprium, 485 ff.
Protensity, 330
Pseudoconditioning, 230
Psychiatric diagnosis, as typology, 505; homogeneity in, 503; limitations of, 502 ff.; problems of, 505 f.; reliability of, 503 f.; validity of, 504
Psychiatry, 299
Psychic, determinism, 470, 472; energy, 470, 472
Psychoacoustics, 274
Psychoanalysis, 468 ff.
Psychoanalytic theory, 431; applied to psychopathology, 512 ff.; view of neurosis, 512; view of psychosis, 515 f.
Psycholinguistics, 181
Psychological scaling, 2
Psychological theory, 367
Psychology, informational, 438
Psychometric function, 278, 280 f., 283 f.
Psychopathology, 499 ff.; definitions of, 500 ff.; energy models of, 518; values in, 502
Psychopharmacology, 274
Psychophysics, 274 f., 284, 288
Psychosexual development, 469 ff.
Psychosis (*See also* Schizophrenia *and* Manic-depression)
Psychosis, developmental theory, 518; drive reduction and, 519; social participation in, 518
Psychotherapy, 479 ff., 493
PTC taste blindness, 115, 117, 120
Public relations, creative potential and, 430
Pulvinar, 45
Putamen, 40
Puzzle box, 329
Pyramidal system, 56, 77

Quality, 330; primary, 311; secondary, 311; tertiary, 315
Quanta, 281, 289; neural, 283
Quantitative methodology, 1
Quickening, 360 ff.

Race, 116 f.
Radio-frequency current, 55
Rank ordering, method of, 9; normalized, 17 f.
Ratio production, method of, 21 f.
Rationalism, 311
Rationalization, 514
Ratios, equality of, 278
Reaction formation, 513
Reaction potential, 153 f., 166
Reaction time, 405, 408
Reasoning, in rats, 161
Recall, 65, 72, 74, 78; anchors and, 335 f.; free, 188; principles of, 441 f.; problem solving in, 438; recognition and, 186; selective, 183; serial, 188; test, modified free, 209; verbal, free, 187
Receiver characteristic ROC, 302
Recency, principle of learning, 148 f., 187
Receptor, 312; system, 62
Recessiveness, 95, 99, 115, 123
Recoding, 182
Recognition, 189; threshold, 398
Recombination, 104, 119
Recovery, spontaneous, 207, 210, 212
Red nucleus, 57
Redefinition, factor of, 425; functional fixedness and, 443 f.
Reductionism, 472, 489; theories, 36
Redundancy, 293, 322 f.
Reflex arc, 313 f.; theory, 66
Registration, 72
Regression, 470, 473; of creative potential on IQ, 432 f.
Reinforcement, 53 ff., 65, 72, 79, 212, 291, 315, 330, 387 f., 391; AL conception of magnitude of, 217; blood condition and, 233; brain stimulation and, 232 f.; contrast effects in, 216, 256; degree of, 215; differential vs. absolute, 235; ease of learning and, 255 ff.; intensity of needs and, 256 ff.; need reduction and, 231; operations, 216; in originality learning, 454; periodic, 291; primary, 152 f., 231; relativeness of, 256; response and, 231; secondary, 152 f., 235, 238; social, 540 f.; stimulus change and, 231; temporal parameters in, 213; theory of, 538 ff.; value of, 490 ff.
Rejection, 315
Relation, empirical, 278
Relationship, cognitive, 287; discriminatory, 287 f.; experiential, 287; functional, 276 f., 289; isomorphic, 276
Relative uncertainty, 294
Relevance, 337 ff.
Reliability, 279, 462 f.
Religion, creativity and, 449
Repetition, 184 f.
Repetition compulsion, 470
Replacement, 197
Report, introspective, 278, 288
Representation, 78
Representative design, 392
Repression, 470 ff., 494, 513
Reproductive isolation, 106
Repulsion, 327
Resentment, of creative persons, 447
Residual, 319
Response, bar-pressing, 291; criterion, 279; delayed, 213; deprivation, 243 f.; differentiation, 198; dis-

criminatory, 275, 279, 282; equivalence, 144 f.; false positive, 284; fractional anticipatory goal, 259; goal, 153; graded neural, 284; hypothetical, 145, 153, 164; mediating, 189; motor, 276; negative, 284; neutral, 284
Responses, correlation among, 250 f.
Result, knowledge of, 212
Retention, long-term, 207
Reticular activating system, 376 f.
Reticular nucleus, 41
Retrieval, 183
Retrograde degeneration, 41
Reverberating circuits, 63
Reversal learning, 49 f.
Reward, delayed, 214; empirical definition of, 230; motivational determinant, 230 ff.; symbolic, 215; theoretical definition of, 230 f. (*See also* Reinforcement)
Rhythm, 316
Ribonucleic acid, 58
Ribosomes, 101
Rigidity, transformation and, 443
RNA, 100 f.
Rod, 312
Role, 556
Rotary pursuit performance, 399
RTT, 199

Salivary glands, role in thirst, 226 f.
Satiation, 340
Scale, 395; equal discriminability, 20; interval, 3 ff.; latent, 7 f.; latent interval, 17 ff.; latent ratio, 25 f.; loudness, 25 f.; manifest, 7; manifest interval, 14 ff.; manifest ordinal, 9 ff.; manifest ratio, 21 ff.; nominal, 3; number, 2 f.; numerical category, 15 f.; ordinal, 3, 5; ratio, 4 f.; sensory, 286
Scalogram analysis, 11 f.
Scanning, 442; ocular, 320
Schizophrenia, 117; body types, 511; childhood, 520; child-rearing antecedents of, 520; genetics and, 506 f.; learning theory and, 515 f., 519, 523; paternal dominance and, 520; process-reactive classification of, 519 f.
School, structural, 312
Schwann cells, 37
Science, abilities and, 428
Scientist, and social inhibitions, 447 f.
Score, difficulty, 317
Secondary needs (*See* Acquired needs)
Secondary reinforcement, 153; cue vs. reinforcing function, 239; durability of, 238 f.; general issues, 238 f.; parameters of, 235 ff.; principle of, 234; role in reducing drive, 238; theoretical issues, 238
Segregation, of alleles, 95, 104, 108, 117 ff.
Selection, 99, 111, 113; artificial, 111, 114; item, 198; phenotypic, 113
Selective function, of a motive, 223 f.
Self-actualization, 436, 468, 480, 482 f.
Self-concept, Rogers' concept of, 480 ff.
Self-stimulation, 54 f.
Semiautonomous process, 63, 70
Sensation, 273 f., 277 ff.; level, 290; negative, 331
Sense, distances, 274; ratios, 274
Sensitivity, differential, 279; to problems, 425
Sensory deprivation, 241 f., 299, 394, 396 ff.
Sensory function, during weightlessness, 402
Sensory, images, 74 f., 77; isolation, 394, 396 ff.; preconditioning, 163 f.; systems, 41, 52, 60, 74
Septal area, 51
Sequential analysis, 295 f., 298
Servotheory, 368 f.
Set, 63, 405
Sex-linkage, 96
Sexual behavior, 51
Sham drinking, 227
Sham eating, 232 f.
Signal, coherency, 366; detection, 405; plus noise, 300 f.; rate, 375 f.
Similarity, 187, 189, 203, 209, 315 f.; association by, 437; dimensional, 337
Simplification, 318
Simultaneous discriminations, 44
Situational cues, 523
Size, parent, 332
Skilled acts, 57
Slant, 320; geographical, 320; optical, 320
Sleep, 314
Slump, fourth-grade, 449 f.; seventh-grade, 450
Social, hierarchy, 51; interest, 477 f.; learning theory, 468, 489 ff., 533 ff.; perception, 560 ff.; structure, 108
Social psychology, definition, 529
Solving, problem, 314
Somatic cells, 94
Somesthetic discrimination, 44, 46 f., 51
Sour, 315
Space, visual, 333
Space cabin simulator, 397
Space perception, binocular, 289
Spatial visualization, 429
Spatiality, 47, 50 f.
Species differences, 59
Speech, rate, 314
Spike potential, 38
Spinal cord, 39
Spiral, rotating, 328
Split-brain, 45
Spontaneous activity, flexibility as factor DMC, 426; role of learning in, 249 f.; as measure of motivation, 248 f.; as related to primary needs, 248 f.
Spreading depression, 58
S-R, compatibility, 298; reflexes, 69, 82
Stabilimeter activity, deprivation and, 248
Stability, perception of, 321
Stabilized images, 64
Stage of learning, 53
Stages, Rassman, 434; Wallas, 434
Stance, 339
Standard deviation, 280
Stimulation, motivating properties of, 315; sensory, 315
Stimuli, achromatic, 285; background, 207; contextual, 285; directive, 319; phasic, 329; residual, 285, 287; steady-state, 329; subliminal, 337, 339; transient, 329
Stimulus, 275 ff., 281, 286, 292; discrimination, 189, 200; generalization, 153, 189, 259; identification, 72; impoverishment, 394, 396 ff.; in motivation,

259; predifferentiation, 189; rating, 278; as reinforcer, 231, 234; spatial separation of, 365 f.
Stimulus-response approach, 421
Stomach contractions, distention and thirst, 227; hunger and, 225
Storage, 183
Strain comparisons, 116 f.
Stress, in heterogeneous groups, 446
Striate cortex, 70
Structure of intellect, and creativity, 425 ff.; as morphological model, 453; transfer and, 437 f.
Struggle, for superiority, 476 f.
Study time, 214
Style of life, 476 f.
Subcallosal cortex, 52, 74
Sublimation, role of, 431
Successive categories, method of, 20
Successive discrimination, 44
Suggestion, 314
Superego, 471 ff., 513
Superior colliculi, 40, 46, 70
Superior temporal cortex, 48
Sursumversion, 333
Sweet, 315
Symptoms, habituation, 325; withdrawal, 325
Synapse, 38, 63
Synesthesia, 332
Syngamy, 103
System, coordinate, 319

Tabula rasa, 311
Taste, 315
Taxonomy, 315
Tay-Sachs disease, 115 f.
Teaching, creative, 452
Telencephalon, 39 f.
Telodendria, 38
Temperature, 314; relation to activity in rat, 248 f.
Temporal, lobe, 44 ff., 48, 56, 60; pattern, 48; relations in reinforcement, 237
Test, alternate uses, 424; apparatus, 425; ask-and-guess, 449 f.; brick uses, 424; camouflaged words, 429; cartoons, 425; checkerboard maze, 231, 248; consequences, 424, 430; controlled associations, 423; four-word combinations, 423; Gestalt transformation, 425, 428; hidden figures, 429; Ishihara, 323; Kohs Block, 399 f.; Luchins' water jars, 443; Mackworth Clock, 404, 406 f.; match problems, 424, 443; MMPI, 447; object synthesis, 425; penetration of camouflage, 429; plot titles, 424 f., 430, 445; quick response, 424; remote associates, 433 f.; riddles, 425; seeing deficiencies, 425; seeing problems, 425; Street Gestalt, 318, 425; Strong vocational interest blank, 447; symbol production, 425; Thurstone-Gottschaldt, 400; unusual uses, 424, 430, 454; Wechsler digit symbol, 399 f.; word-association, 433
Texture, 320; classification of, 320
Thalamus, 40 ff., 52, 54, 67 f., 73, 77, 333; external portion of, 41, 67; extrinsic nuclei, 41, 67 f.
Theorem, channel, 293; coding, 293
Theory, all-or-none, 193, 196; Arnold's, 72; association, creative production, 437; association, of originality learning, 454; classical sensory discrimination, 279, 284; conditioning, 149; consolidation, 58; electrotonic, 328; Estes' learning, 169 ff.; Guthrie's learning, 147 ff.; holistic, 60 f.; Hull's learning, 152 ff., 165 f.; incremental, 193, 196; information, 292 f.; of insight, 440; interference, 208; Lashley's, 62 f.; learning, 291 f., 329; Miller's learning, 173; Morgan's learning, 140 ff., 148; Mowrer's learning, 162 f., 172 f.; neural quantum, 281 ff.; physical correlate, 29; Pribram's, 66; psychoanalytic, 337; psychophysical, 276; serendipity, 431; Sheffield's learning, 173; signal detectability, 300 f., 303; social learning, 521 ff.; Spearman, 433; Thorndike's learning, 142 ff., 148; Thurstone, 433; Tolman's learning, 150 ff.; transfer, of creative production, 435, 437; transfer, and factor, 437 f.; vigilance, 407 ff.; Watson's learning, 147 ff.
Thinking, 70; creative, competition and, 446; group restriction on, 446 f.; irrational, 445
Thirst, blood conditions of, 226 f.; cellular dehydration in, 227; central nervous system and, 226 f.; dryness of mouth and throat in, 226; electrolytic concentration in, 227
Threshold, 194, 315, 331, 336; absolute, 274, 278 ff., 283, 286, 289, 339; awareness, 336; differential, 274, 278 f., 284, 289; relative differential, 290; time, 335; two-point, 325
Tilt, 320; cage, 248
Tolerance, frustration, 60
TOTE sequence, 434; unit, 81
Touch, active, 320 f.; passive, 320
Tracking, 356 ff.; compensatory, 358; definition of, 356; mathematical models of, 367 ff.; measurement of, 358 f.; multidimensional, 365 ff.; pursuit, 357; pursuit and compensatory compared, 358; research methods, 356
Tract, spinothalamic, 313
Training, inquiry, 436; persistence of creativity, 453; persistence of originality, 454; transfer of, 182
Traits, 94
Transactionism, 389, 391, 396
Transcortical reflexes, 61 f., 70, 78
Transfer, 46, 203 f.; of creative habits, 452; intermanual, 47; of training, 145 f.; with insight, 440
Transformation, factors of, 428 f.; flexibility and, 443; humor and, 427; of input, 79
Transmission, 293, 297; genetics, 94
Transmitter substance, 39
Transparency, 317
Transposition, 155 ff.
Treatment, 274, 291
Tremor, incoordination and, 47
Turner's syndrome, 128
Twins, dizygotic, 124; monozygotic, 124
Typologies, 466 f., 470

U-hypothesis, 363
Ultrastable system, 69
Unburdening, 360
Uncertainty, 187 f., 293 f.
Unconscious, 470 ff., 494; class concept, 440; problem solving, 439
Underloading, 299
Understanding, in animals, 140, 142, 145 ff.
Unfolding technique, 12 ff.
Unit, natural, 128 f.; neural, 281; quantal, 283

Unity, of science, 79
Unlearning, 209 f., 212
Unpleasant, 315
Unstructured visual field, 394
Unusualness, working time and, 441
Urban life, creativity and, 448
Urination, 314

Validity, construct, 429 f.; factorial, 429; predictive, 430
Variability, 278; stimulus-centered, 278; subject-centered, 278
Variable, continuous, 275 f.; dependent, 274; discrete, 275 f.; empirical, 275 f.; hypothetical state, 275; independent, 274, 278; intervening, 275, 284; organismic, 288; quantitative, 276
Ventricle, 68
Verbal, processes, 60; rating scales, 16
Vestibular nuclei, 58
Vigilance, 314, 372 ff., 393 f., 398, 404 ff.; activation theory of, 393, 409; activity level and, 407; conditions of stimulation and, 405 f.; decrement, 372, 374 ff., 405 f., 409; duration of watch and, 406; individual differences in, 407; intelligence and, 407; knowledge of results and, 407, 409; measures of, 405; observer and, 406 f.; personality and, 407; presentation interval and, 406, 409; reinforcement and, 407 ff.; reviews of, 405; sense modality and, 405; signal intensity and, 409; signal location and, 409; signal rate and, 405 f., 409; social variables and, 407; on successive vigils, 406; task complexity and, 406
Vision, 289
Visual, cliff, 320; discrimination, 43 f.; efficiency, 396; exploration, 241; horizontal, 319; searching, 61; vertical, 319
Visual data, display of, 346 ff.
Visual displays, research methods for, 347
Visualization, spatial, 429
Vocabulary, role of, 429
Voluntary behavior, 56, 65

Warming up, 208
Weber fraction, 290
Weightlessness, 392 ff., 402 ff.
Will, 71
Wire mother, 247
Withdrawal, 48
Word count, Thorndike-Lorge, 330
Word fluency, 423; as factor DSU, 426
Work inhibition, 375
Writers, most creative years of, 451
Writing, criterion for, 430

Zero, absolute, 4
Zöllner figure, 322
Zone, neutral, 326
Zygote, 94, 103, 125

Mahinya
Mahinya

Mahaupa

3321